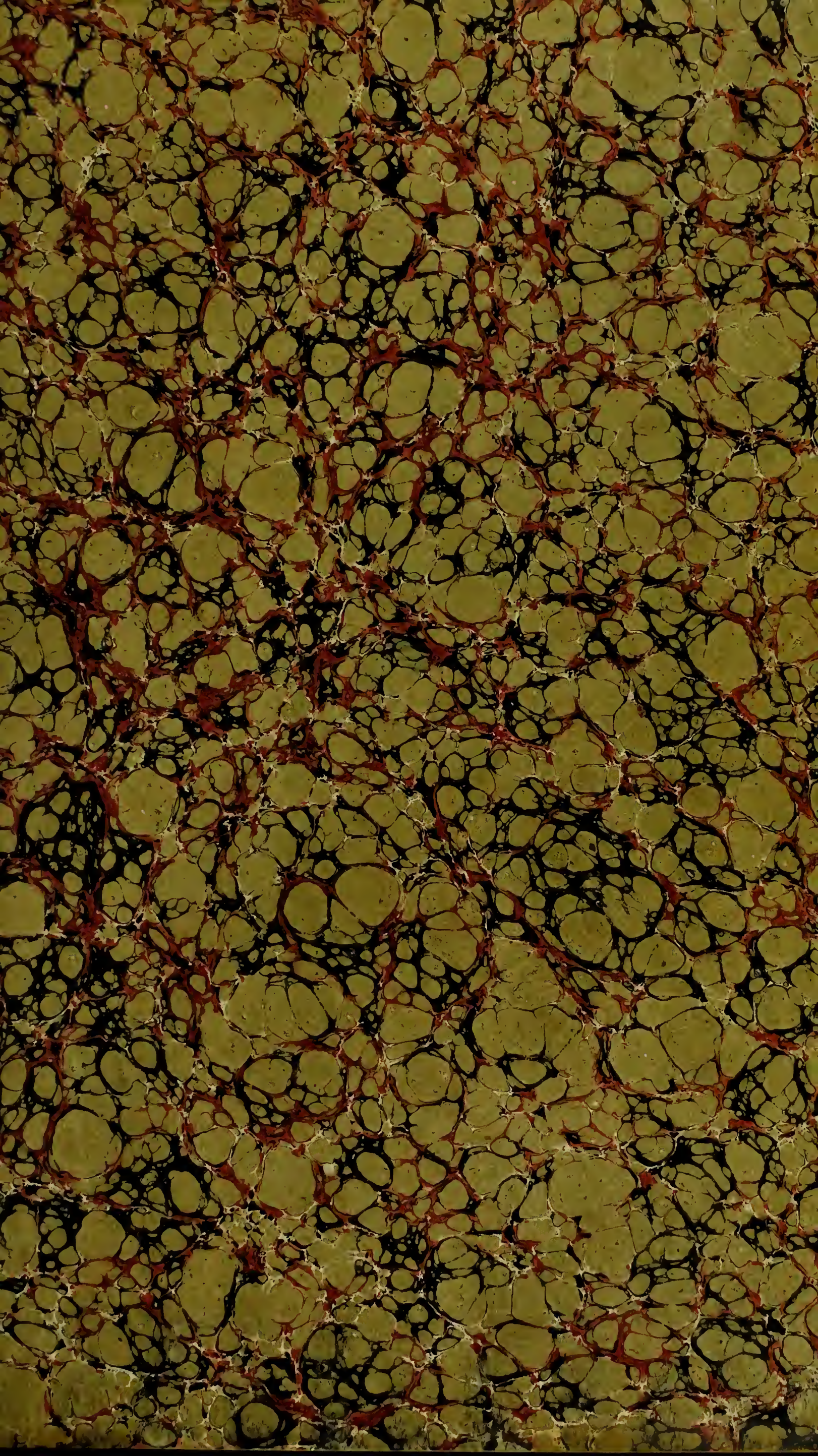


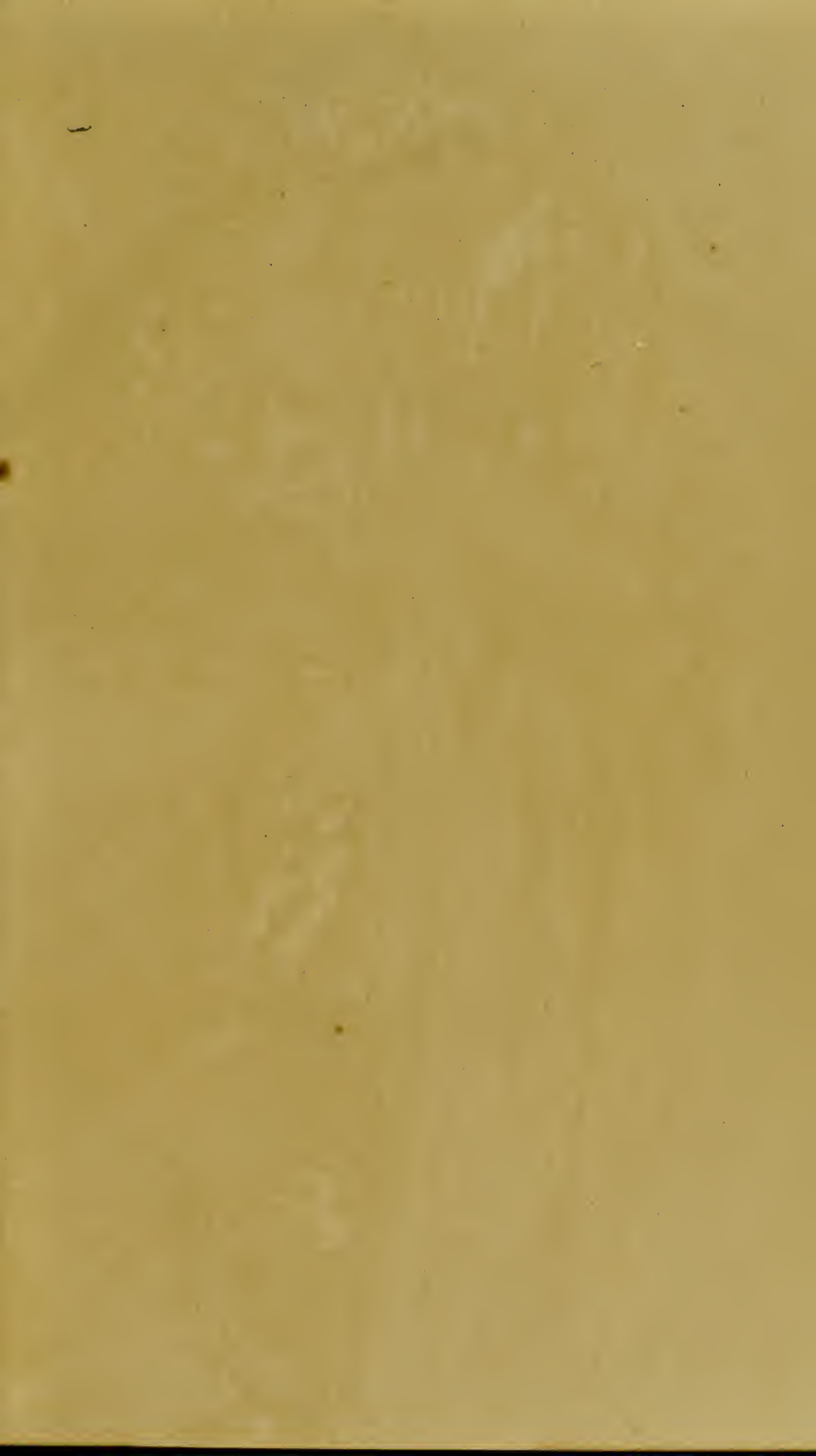
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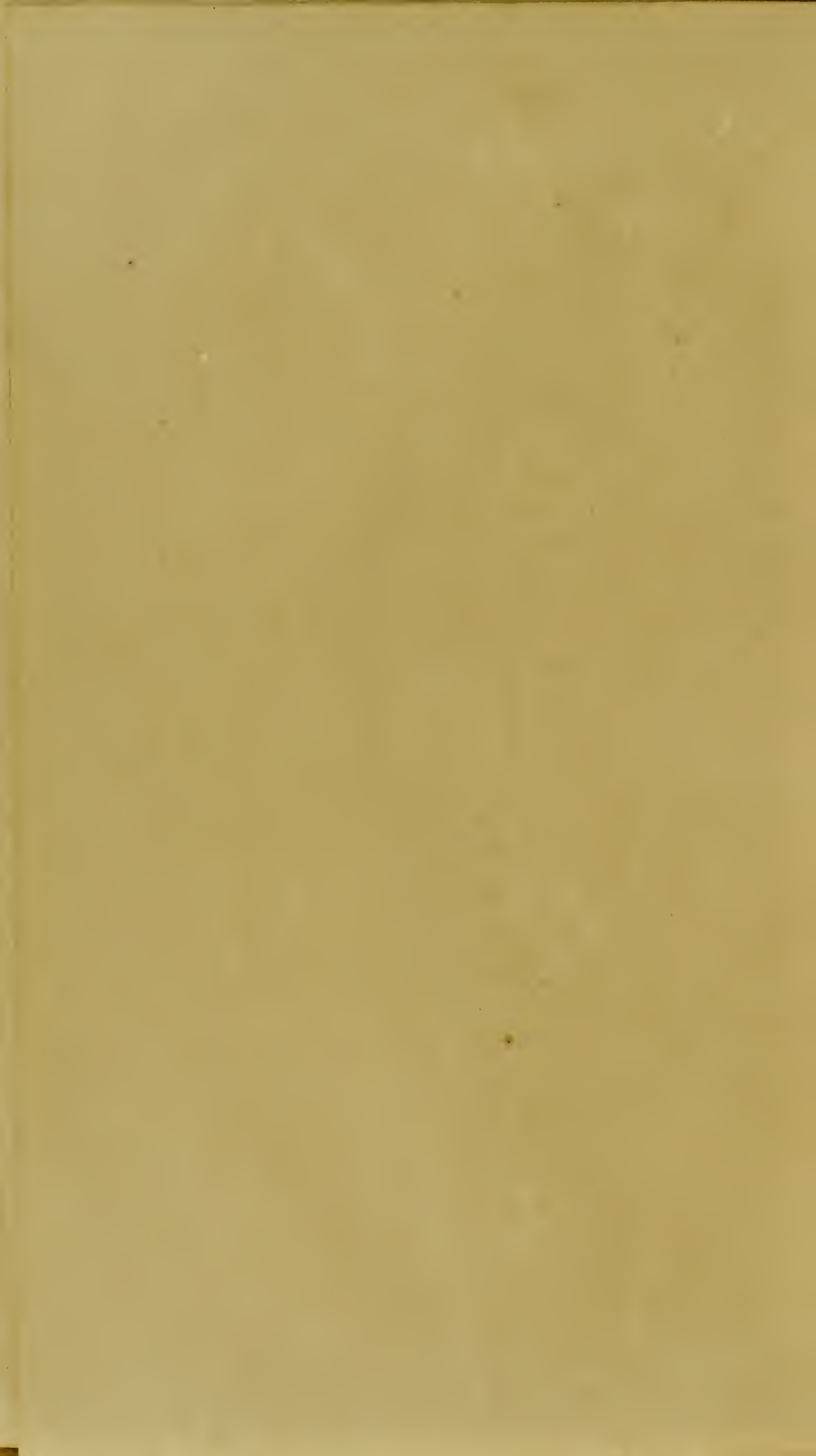
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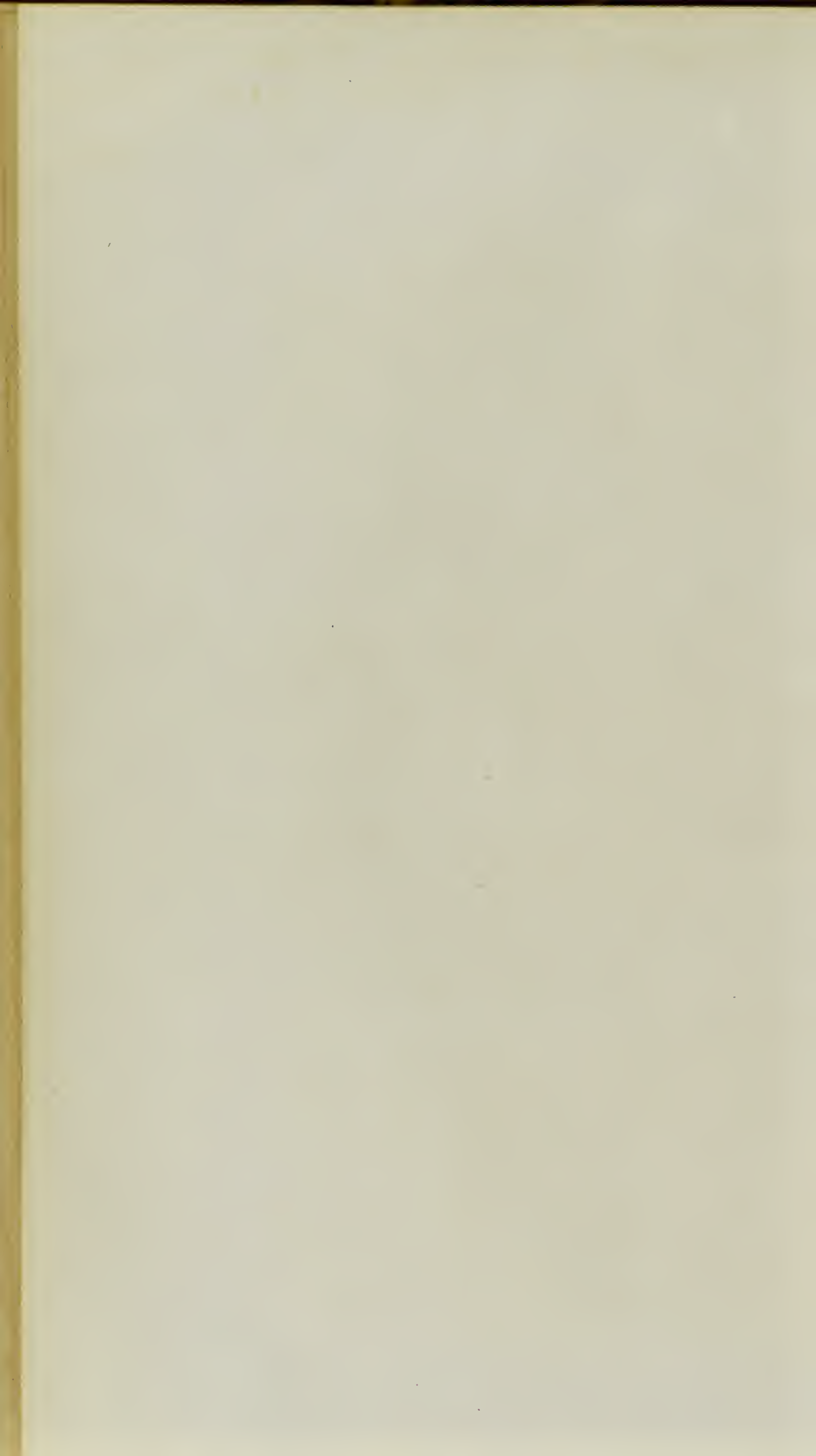
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*Presented by Thomas Watson M.D.
President of the College.*
YELLOW FEVER,
1864.

CONSIDERED IN ITS

HISTORICAL, PATHOLOGICAL, ETIOLOGICAL,

AND

THERAPEUTICAL RELATIONS.

INCLUDING

A SKETCH OF THE DISEASE AS IT HAS OCCURRED IN
PHILADELPHIA FROM 1699 TO 1854.

WITH

AN EXAMINATION OF THE CONNECTIONS BETWEEN IT AND THE FEVERS
KNOWN UNDER THE SAME NAME IN OTHER PARTS OF
TEMPERATE, AS WELL AS IN TROPICAL, REGIONS.

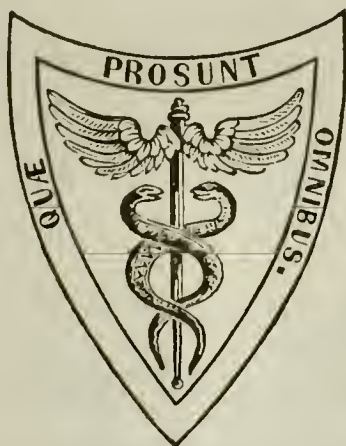
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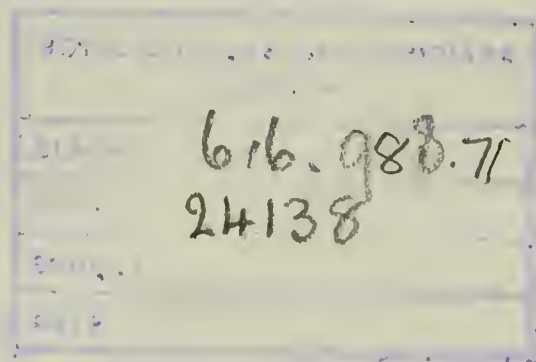
IN TWO VOLUMES.

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A

T R E A T I S E

ON THE

Y E L L O W F E V E R.

C H A P T E R I.

ETIOLOGY.

HAVING, in the foregoing volume, presented a history of the yellow fever as it has manifested itself at various times in the city of Philadelphia, its symptomatology, diagnosis, and prognosis, and compared it in those several respects with the fever of other cities of the United States and of foreign climes, I proceed, by a natural transition, to inquire into the causes of the disease. An examination of what has been written on the subject by our own and foreign inquirers, will show that these causes are usually divided into predisposing, exciting, and remote. But the reader will perceive, as we proceed, that however proper in regard to other cases such a division may be, it is scarcely admissible in the present instance, and would lead to endless repetitions and create confusion, inasmuch as some of the morbid influences—external and internal—which exercise an agency in predisposing to an attack, are often instrumental in exciting the development of the disease. To obviate this difficulty, I shall investigate those influences under the following heads: 1, causes depending on the individual; 2, hygienic causes; 3, contagion; 4, infection. The first of these sets of causes embraces those individual organic peculiarities, natural or acquired, depending upon the temperament, age, sex, or race of those exposed to the action of the efficient cause of the disease, and which tend to modify, promote, retard or prevent the development, progress, or issue of the latter; as well as upon the modifications induced in the system by an attack, or by habitual exposure to the other causes of the fever. The second series of causes embraces the various morbid influences which, in hygiene, are denominated non-naturals—namely, circumfusa, percepta, ingesta, gesta, applicata, and excreta. The third and fourth sets of causes which in former days were almost invariably, and continue even now

to be occasionally confounded with each other, have reference to the remote or efficient circumstances which are supposed to give rise to the disease, and without the agency of which the preceding causes would remain perfectly innocuous, so far, at least, as the production of that particular disease is concerned.

In accordance with this plan, I commence with the first of these series of causes, which will be examined in several separate sections. The first of these sections embraces the consideration of the classes of individuals prone to the disease. This naturally leads to an inquiry into the subject of acclimatization. The next embraces the question of reinfection, or repetition of the fever in the same individual—in other words, of the protection afforded by the modifications occasioned in the system of individuals who have passed through the disease. Under the third head I shall treat of the influence of temperament; while in the fourth, fifth, and sixth sections I shall examine those of age, sex, and race.

It is a fact well known in medicine, that some diseases exhibit a marked preference for certain temperaments, while they spare, to a greater or less extent, others differently endowed in that respect. Each period of life has its maladies. The two sexes, and the various races, are not equally liable to the same complaints, and, when seized, do not run the same risk as to a fatal issue; and the professional reader need not be told, that while some diseases affect the system but once—while some extend their ravages to all classes indiscriminately—others are subject to frequent repetitions, or, though widely diffused, affect certain individuals less violently than others, or even allow them to brave the danger of exposure with perfect impunity. On each of these points the yellow fever of this and other places offers so wide a field of investigation, and several of them have given rise to such a number of contradictory statements, that the objects of the present work—historical and comparative as they are—would be but imperfectly attained were we not to dwell upon these points somewhat in detail.

Acclimatization.—The power of habit in imparting such modifications to the organism as will enable it to support without injury the influence of external agencies which, on those unused to their effects, produce detrimental and even destructive results, has been recognized from the earliest dawn of medical science. It did not escape the keen eye of Hippocrates, who, in various parts of his writings, and more pointedly in the well known aphorisms,¹ and in his justly celebrated work on Air, Places, and Water, has left us the records of his experience. From his day to this, the subject has formed the theme of many a disquisition, and is insisted upon by Gaubius (§ 644) as one of the most valuable attributes of our organization, while later writers—though none

¹ Sect. 2, Aph. 49, p. 50.

“Those who are accustomed to endure habitual labours, although they be weak or old, bear them better than strong and young persons who have not been so accustomed.”

“Those things which one has been accustomed to for a long time, although worse than things which one is not accustomed to, usually give less disturbance; but a change must sometimes be made to things one is not accustomed to.”

more successfully than Cabanis¹—have pointed out the extended sphere of development, of which it is susceptible. It is in accordance with a happy provision of nature, in virtue of which the system adapts itself to, and is placed in equilibrium or harmony with, the physical and moral conditions of the country in which it is placed, and extends as well to the action of the natural elements, in the midst of which we move, and which constitute the climate of the place—temperature, moisture, winds, degree of elevation, and the qualities of the soil—as to that of abnormal agencies, which, whether arising from living or inanimate objects, tend to vitiate the atmosphere, and thereby produce disease.

The medical philosopher needs not to be reminded of the influence of climate (understanding this in the extended sense applied to the term by Hippocrates, and many of his more worthy successors) in moulding all living beings to the exigencies of the localities they are destined to occupy, or to which they may be transferred. He knows that in each country the temperature of the atmosphere, the quality of the water, the nature of the soil, impart to vegetable products a physiognomy and certain characteristics unobserved in other localities differently circumstanced in those respects. He knows that this physiognomy and these characteristics are modified through the influence of change of location, and that, however trying the effects of the transfer may at first be, the objects thus transferred gradually become inured to the peculiar action of the new agencies to which they are exposed, and, after assuming their new garb, thrive with as much readiness as they did in their former location. He is fully aware also that the lower orders of animals furnish ample illustrations of this modifying power of climate.² That the human system is placed under the control of the same modifying power—that the temperament, constitution, colour, shape, and mental manifestations of man are thereby changed, it would be a waste of time to illustrate; and though I am not prepared to refer to this cause alone, as has been done, the remarkable differences noticed in these respects in the numerous varieties of our race—though I am aware that a no small portion of the differences observed are ascribable to the power of transmission, hereditarily, of acquired peculiarities, and that something is due to the state, conditions, circumstances, habits and manners of society, and even to political institutions; yet it is certain that much is due to the agency of the cause in question.³ In effecting this purpose and in obtaining organic changes requisite to place the individual in harmony with the physical condition of the country, climate operates in a slow and gradual manner, and through the instrumentality of the aptitude which nature has implanted in our organs and tissues to be so modified by the numerous agencies that surround us as to become thereby inured to their

¹ *Rapports du Physique et du Moral de l'Homme*, ii. 7, &c.

² Pritchard, *Nat. Hist. of Man*, p. 27; *Encycl. du 19me Siècle*, i. 212; Buffon, *Hist. Nat.*, v. 289; *Encycl. Brit.*, xiv. 239, and other volumes; Foissac, *De l'Influence du Climat.*, pp. 12, 13; Roullin, *Mém. de l'Académie de Méd.*

³ Foissac, *de l'Influence du Climat*; Pritchard, *Nat. Hist. of Man*; Geoffroy St. Hilaire, *Encycl. Nouvelle*, art. *Domestication*, 1838.

action, and to experience changes which involve not only the external configuration, shape, and complexion of the body, but also internal organs, on the peculiarities of which depend, among other things, diversity of constitution, temperament, &c. &c.

Bichat¹ has erred in restricting the influence of habit to the functions of animal life. However true it may be, that this influence exhibits itself principally in relation to those functions, and however fortunate it is that such has been the will of the Creator—for, had it been otherwise, life would be perpetually in jeopardy—yet, experience shows that the functions of organic life are not placed beyond its reach. Its empire may vary in point of details, but it is universal. Our physical wants are greatly modified by the means in question. Our feelings, our affections, our aversions, our tastes, our mental wants, are, in many instances, under the moulding power of habit, and though our sensations, either of pleasure or pain, are apt to be blunted by a repetition of the causes by which they are elicited, the want of anything to which we have long been habituated, becomes often a source of pain, and continues so till, with the progress of time, other wants are created and take the place of the former. Through the operation of habit we become so inured to certain stimuli, that they finally cease to produce their wonted effects. We know also that, in virtue of this power of habit, the various articles of the *materia medica*, even the deadly poisons themselves, lose their effects by long continued use.

Habit enables us to support diseases, which, at first, were painful and depressing. To habit, we may refer the tendency to repetition exhibited by some complaints. A great master has said : “ *Memorabilis quoque est facultas adsuescendi, qua utrumque hominis principium gaudet. Hac si careret natura humana, paucissimis profecto sanis esse contingeret. Duramur nimium consuetudine ad innoxie ferendum innumera, quæ minus adsuetis obsunt. . . . Ita val morbi etiam adsuetudine magis et tolerabiles et tractabiles redduntur, et ipsa venena mitescunt.*”²

In a word, habit seems to possess the power of modifying the system to so great an extent and so permanent a degree, as to justify those who hold it in the light of a second nature. In virtue of the influence it exercises, and the peculiar organic changes resulting from long exposure to the sensible and insensible qualities of the atmosphere, or to the extraneous materials by which that atmosphere may be contaminated, man enjoys the faculty to which I have alluded, of living under climatic influences of the most diversified characters. He resists the inclemencies of the elements, the insalubrity of the seasons, the extremes of temperature as well as the action of malarial and other exhalations. With time, the native of the North acquires the privilege of supporting with impunity the scorching rays of a tropical sun ; though the result is not obtained without inconvenience, suffering, and even danger, and without, in the greater number of instances, subjecting the individual to the ordeal of disease. Not so easy is it to become habituated to the baneful action of those modifiers—such as malarial exhalations—which exercise their agency on the

¹ De la Vie et de la Mort, p. 56.

² Gaubius, Instit. Patholog., § 644.

principle of vitality. But even here immunity is obtained—either gradually and insensibly, without shock to the system, or more suddenly through the effect of an attack of fever. But whatever be the means by which the process is effected, that such a protection is thus obtained, to a greater or less extent, in regard to all malarial and some other forms of fever, no one who has examined the subject with attention will feel disposed to deny. By long habituation to infectious localities, and to the high temperature of hot regions, the system becomes acclimatized, and thereby acquires the power of tolerating perfectly and permanently the poison, or of eliminating it as soon as received, without succeeding reaction. The observation is of old standing. Pliny, nearly twenty centuries ago, called attention to the fact, “that they who are seasoned can live amid pestilential diseases,” and the statement has been confirmed by all subsequent observations. The immunity is more or less perfect according to the individual peculiarities of those exposed to the cause and to the salubrity of the country whence they come; and is enjoyed at all future time, except when from the concurrence of particular circumstances, the poison acquires unusual deleterious properties. To this it is we are in part to ascribe the well-known fact that the African lives with impunity amid the effluvia of his native soil, which is deadly to the unacclimated; and that strangers, by long and constant exposure, learn, though not without risk and suffering, to support the climate, if not as well as the natives, at least better than new comers. The inhabitants of the fenny districts of France are, as we learn from Monfalcon, in a measure, inured to the unhealthiness of their atmosphere—breathing it with comparative impunity, while new comers seldom escape its deleterious agency.¹ We are told that the maremmes of Italy furnish examples of the sort—the labourers who come down in harvest time being great sufferers from fever, while the permanent resident resists the action of the morbid cause.²

Lancisi says that they who are born and reared in the neighbourhood of marshes enjoy good health in an unhealthy atmosphere, and that children can, by degrees, be accustomed to take considerable doses of the poison, beginning with small ones at first. “By this power of habit does it happen that man can live upon the small islands in those ponds and lakes; and that even the floating islands which the winds drive about from place to place, have their human inhabitants. For Secundus, in his description of the Lake of Vadimon, declares he has seen them. P. Cabæus has observed them in the marshes of Ferrara, and I myself have seen them in the Alban Lake.”³

“In England,” says Dr. Pinkard (ii. 480), “the harvest-men and strangers who go into the fens of Kent and Lincolnshire in the autumn, are more readily attacked with the endemical fever of those provinces than the inhabitants who constantly reside in the atmosphere which causes it.” The protec-

¹ Hist. des Marais, pp. 196, 198, 204, 341; Alibert, p. 251.

² Chapman on Epid., Phil. Med. and Phys. Journ., viii. 382; Griffith on Malaria, ib., ix. 284; Lond. Quarterly Review, xxx. 139.

³ De Nox. Pal. Effl., lib. i. cap. v. 21.

tion afforded by acclimatization against endemic remittent is found to be enjoyed in this country, often giving entire immunity, and generally lessening the violence of the disease. Our own country furnishes us with an example of the power of habit in warding off the baneful effects of malarial agents. Dr. Coventry mentions it in reference to our lake fever.¹ It is also recognized as regards the fever of our southern States—Georgia and South Carolina.² Witness the negroes inured to the pestiferous atmosphere of our southern rice plantations. They are in a very great degree exempt from severe autumnal diseases, while house servants, who are not accustomed to that atmosphere, are very prone to them.

The term acclimatization, as we are told by Dr. Fenner, is just as familiar to the inhabitants of all the southern portion of the Mississippi Valley as it is to the citizens of New Orleans, “and is used to express the same idea, viz: that persons coming from a northern climate and settling there, are very liable to have attacks of fever during the first two or three years, but afterwards become quite exempt.³ The same power of resistance to the action of disease by the acclimatized has long been recognized in other countries.

Dr. A. Brown, whose observations have reference evidently as much to the common remittent as to the yellow fever of tropical climates, after stating that a soldier, or stranger, cannot be considered inured to such climates by a residence of less duration than from three to five years, remarks that if the latter period be adopted, as affording a greater probability that a regiment has passed through at least one epidemic season, on turning to table 76, p. 92, of the Statistical Report, we find that the average mortality, during the five years of service, was in the ratio of 147.7 per 1,000, whereas, in the second five years, it was 104.7 per 1,000 of the strength. If we now examine table 77, p. 93, from which the great epidemic years are excluded, we find that the mortality in the first five years of service was 102 per 1,000, and in the second five years 82 per 1,000 of the strength. From this we perceive that the immunity, or advantage enjoyed by the acclimatized, is not confined to epidemic years, though in such it appears to be about twice as great as in ordinary years; the diminution of the mortality amounting in the former to four, and in the latter to two per cent. of the force respectively.⁴

Persons inured to the rigorous climate of the high and well-known mines of Cerro Pasco, in Peru, are almost sure, when they go down to the vale of Huanuco, to the air of which they are unaccustomed, to have a bilious diarrhoea if they eat fruit and sleep in the shade.⁵ From this the inhabitants are free.

In regard to the fevers of Ceylon, we learn that the natives enjoy no exemption from its ravages—indeed, they are often prostrated while the well fed and sheltered European escapes, though resident of the same estate; but,

¹ Address, p. 42.

² Pendleton, Charleston J., vii. 451; Lewis, N. O. J., i. 323.

³ Southern Med. Rep., i. 32.

⁴ Second Report on Quarantine, pp. 294, 295.

⁵ Arch. Smith, Dis. of Peru, Edinb. Med. and Surg. J., lviii. 60.

in the former, agues are much more frequent than attacks of remittent, and this latter, though if left to itself very fatal, rarely presents the same high degree of symptoms, but rather saps slowly the powers of life.¹

The following remarks by Sir Gilbert Blane on this subject, though applying only to the Walcheren fever, may be appropriately introduced here: "One of the most important circumstances in the operations of marsh miasmata on the human body, is the power of habit in mitigating its influence. The natives are a robust people; they are of a very wan and sickly hue, with flaccid flesh, and have all suffered more or less from the bad air which they breathe. The children of both sexes are very subject to glandular and abdominal complaints; and the adults, particularly those of the lower orders, have all of them, some time or other in the course of their lives, laboured under the endemic intermittent. They are, however, infinitely less subject to intermittent fevers than strangers." "These strangers are also variously affected, according to the district from which they come. It was found, that of the British troops, the natives of mountainous countries and dry soils, such as the Highlands of Scotland, were more frequently affected than the natives of flat and moist districts, such as Lincolnshire." Sir Gilbert mentions the case of a French regiment, "which suffered in the second year of its being there only one-half of the sickness and mortality which it suffered the first year, and hardly suffered at all the third."² Monfalcon informs us that the same fate attends the inhabitants of the high and healthy localities in the vicinity of the fenny districts of the Bresse, who visit the plains in quest of work during harvest;³ and similar statements are made by Foderé in regard to the peasantry of Romagna, Modena, and Ferrara.⁴

From all the accounts that have been transmitted to us relative to the sickness and mortality of French troops in Algeria, as well as to the results—in a sanitary point of view—of the colonization of that country, we learn that Europeans, from the north particularly, experience great difficulty in becoming acclimatized there, and that their chances of success in that respect are, to say the least, extremely precarious.⁵ Diseases—miasmatic fevers particularly—carry off a large number of the troops; a larger number are soon invalided, and the rest must, sooner or later, be sent back to France to renovate their constitutions. While such is the result among the European troops, the natives of the country, and the Arabs enrolled in the army, and doing duties similar to those assigned to the former, remain free from fever, or take it in its mildest forms. Similar results are obtained along the shores of the Red Sea; for while the natives are exempt from the fevers of the country, and individuals of the Indo-Ethiopian race easily become inured to the climate, the Europeans are with extreme difficulty acclimatized.⁶

¹ Cameron, *End. Fev. of Ceylon*, Edinb. Med. and Surg. J., lxxi. 71.

² Rep. on Mission to Walcheren, *Dissertations*, i. 223–225; *Ib.*, Med.-Chir. Trans., iii. 8.

³ *Traité des Marais*, p. 196.

⁴ *Med. Led.*, v. 167.

⁵ Perier, *An. d'Hygiène*, xxxiii. 307; *Work*, i. 48; Boudin, *Ib.*, xxxvi. 381; Maillot, *Fièvres Int.*, p. 265; Haspel, *Mal. de l'Algerie*, i. 78.

⁶ A. Roche, *An. d'Hyg.*, xxxiii. 36.

Chomel and other writers state that typhoid fever attacks very readily those who have been only a short time in Paris, while it in a great measure spares the natives of that city. It appears from a table published by Dr. Davidson,¹ that among 568 eruptive cases of typhus, in whom this point was ascertained at the Glasgow Fever Hospital from November 1, 1838, to November 1, 1839, 176 were natives of that city, and 392 were strangers; 206 of these strangers had resided in Glasgow only from one day to two years, and 186 from two to twenty years and upwards. Dr. D. draws the following deductions: 1. That strangers are more liable to become infected with typhoid fever than native residents. 2. That the majority of strangers are infected within a comparatively short period of their residence in Glasgow. 3. That a minor portion of strangers, like the natives of Glasgow, may escape infection for many years, and yet be afterwards attacked.²

In 1851, typhoid fever prevailed epidemically at Montpellier, especially among the troops of the garrison. It was principally rife among those who had not been long in the place. So far as civilians were concerned, it more generally attacked workmen going the rounds of France (*faisant leur tour de France*), and recently arrived, as also people from the country who had come to work at the crops and the vintage; in a word, the disease selected its victims among individuals unacclimatized to the place.³

“The effects of the poison of jail fever upon the constitution are lessened by habit, like what happens in other poisonous substances, as opium, and ardent spirits. It is owing to this power of habit, that nurses in the foul and crowded wards of hospitals often escape the fever; and it must be imputed to the same cause, that those living in the poisonous air have the fever come on slowly, and with less violence and danger, than such as are exposed to the poison accidentally.”⁴ This has limitations.

Nor is it less true, that the system becomes habituated to even a plague atmosphere. “Constant observations,” says D’Ohsson, “prove to us that everywhere—though particularly in the capital—strangers, travellers, and all those lately domiciled there, are much more susceptible to it (the plague) than the natives of the country.” Similar statements are made by Maclean, &c. (*Epid.*, i. 270). And we are told that during the plague of London, in 1665, of three thousand who fell sick the first week of November, the greater number were new comers.⁵

With these facts before us, we are better prepared to understand that in places where the yellow fever is endemic—where the climate is continuously warm, and the causes of the disease are more or less permanent, or frequently evolved—individuals accustomed by long residence to the influence of that climate, and the agency of those causes, lose their susceptibility to an

¹ Thackeray, Prize Essay on the Causes of Fever, p. 68; Montault, *Mém. Ac.*, vii. 369.

² See also Bartlett on Fevers, p. 102.

³ Anglada, *Traité de la Contagion*, i. 124.

⁴ J. Hunter, *Trans. of College of Phys.*, iii. 356; Good, ii. 72; Bancroft.

⁵ Pugnet, pp. 131–176; *Hist. of the Plague*, by H. F., p. 263, quoted by Maclean, i. 270; Larrey, *Mém. de Méd. Milit.*, i. 340; Meade, p. 42; Bacon, *Hist. Nat.*, x. No. 914.

attack. They become acclimatized or creolized, and are henceforward placed beyond the reach of danger. While individuals thus circumstanced are enabled to live securely in such localities, strangers from more northern latitudes seldom escape an attack of the fever—their susceptibility being in a ratio to the northness or coldness of their native land, or of their accustomed place of residence. By a prolonged residence in cold and more salubrious localities, the individual thus protected loses, to a certain extent, the acclimatization he had obtained, and on his return to his former place of residence becomes once more liable to suffer from the disease. Such, experience teaches, is the case within the tropics, where the population consists of two classes—the first composed of the natives and acclimatized, who, so far as relates to the fever in question, live with impunity amid the sick and the dying; the second of strangers, who are almost inevitably attacked by the reigning disease, and perish in a large proportion.

It requires but a slight acquaintance with the works of writers on tropical diseases, or a limited intercourse with intertropical regions, to be convinced of the existence of these two classes—the privileged and the susceptible. Every medical writer, from the days of Towne, Warren, and Pouppé Desportes—whose works contain the earliest professional records we possess relative to the fevers of tropical climates—has dwelt upon the protective effects enjoyed in hot regions by those who are acclimatized to sickly localities.¹ The decided advantages resulting therefrom are adverted to by Labat,² Ulloa,³ Griffith Hughes,⁴ Moreau de St. Méry,⁵ Herrera,⁶ Oviedo,⁷ and other early, and by all modern travellers and historians; and among the public at large, the reality of the privilege was early, and continues to this day to be, viewed as placed beyond the possibility of doubt.

Warren (p. 13) long ago found that, at Barbadoes, “strangers and new comers” were the most liable to this disease, and that the natives of the torrid zone were “much less obnoxious to it.”

The whole history of yellow fever, as we are told by one who has thoroughly investigated the subject, shows that its most susceptible subjects in tropical climates are those who have recently arrived within its sphere, par-

¹ Williams, p. 51; N. Dickinson, pp. 11, 66; Edin. Journ., xxxvii. 154; Henderson, p. 5; Hume, p. 237; Wright, Med. Facts, &c., viii. 8; Jackson, Fevers of Jamaica, p. 250; H. McLean, p. 187; Madrid, pt. i. p. 32; Savarésy, p. 260; Bally, pp. 268, 332; Dancer, Med. Assist., p. 82; Lefoulon, p. 20; Leblond, pp. 18, 227, 245; Manson, p. 5; Rochoux, p. 31; Caillot, p. 13; Chevallier, p. 3; Pinkard, ii. 172, 177; Gilbert, pp. 75, 76; John Hunter, p. 19; Arnold, p. 26; Bancroft, Seq., p. 41; McArthur, Dis. of Barbadoes, Med. Obs., vii. 326; Peixotto, N. Y. Journ., i. 417; J. Clark, p. 22; Dariste, p. 21; Evans, p. 276; Gillkrest, Cycl., ii. 279; Boyle, p. 150; Blair, p. 59; Doughty, p. 65; Trans. Am. Med. Assoc., v. 589; Vincent, p. 13; Barry, in Boyle, p. 270; O'Halloran, p. 119.

² Nouveau Voyage en Amérique, ii. 264.

³ Voy. Hist. de l'Amérique Méridionale, i. 42.

⁴ Hist. of Barbadoes, p. 37.

⁵ Loix et Constitutions de Saint Domingue, i. 375.

⁶ Historia General de los echos de los Castellanos in las Islas, &c., de Mar. Oceano, lib. iii. cap. 15, lib. x. cap. 4.

⁷ La Historia General de las Indias, lib. v. cap. 11.

ticularly the inhabitants of northern climates, and that the predisposition to an attack increases with the degree of the northern latitude from which the stranger has arrived, and the shortness of the interval that has passed since he left the European for the equatorial region. In illustration of the comparative security of native inhabitants over new-comers, this author, Dr. Ferguson, adverts to the experience of the troops at Cape St. Nicholas Mole, St. Domingo, among whom, soon after disembarking, yellow fever broke out "at every station and in every place." During the earlier part of the residence of the British troops there, while all were deeply interested to stop the mortality, a census was taken of the inhabitants of the town, exclusive of the negro slaves and the white soldiers, when they were found to be very nearly equal in numbers. But by the time they had buried the original complement of 1,500 men, the inhabitants had not lost more than one in thirty of all ages.¹

Dr. Burrell states that, "of thirty regiments that arrived in the Windward and Leeward Islands, between 1816 and 1848, ten were attacked with black vomit fever a very short time after landing; two within three months; eleven within twelve months; five within two years; and two within three years of their arrival. Of thirteen regiments, which landed in Jamaica between the years 1816 and 1834, four were attacked within six months; seven within twelve months; and two within eighteen months. From 1838 to 1848, seven regiments arrived in that island, but the emancipation of the negroes permitting the troops to be quartered in the mountains, a few cases only of black vomit fever appeared within that period, in two of them soon after landing."²

Of the fever of 1850, at Rio Janeiro, where no epidemic having before occurred, the inhabitants could not have acquired the degree of protection observed in the West Indies, we are told that Brazilians, and acclimatized foreigners who were nearly on a par with them, suffered but little; whilst in new-comers, it proved fatal to about thirty per cent. of those attacked.³

Equally well ascertained is it, that the danger of infection among strangers increases in proportion to the coldness of their native land, or of their accustomed places of residence;⁴ that protection in its entire perfection is only enjoyed by the residents of towns or cities subject to the disease; and that the inhabitants of neighbouring, but more elevated and salubrious portions of the country, or of rural districts generally, though less prone to the disease than strangers from cold climates, are, nevertheless, liable to suffer when they venture in an infected place.⁵

¹ Ferguson's Notes and Recollections, p. 150.

² Second Report on Quarantine, p. 9, Lond. 1852.

³ Pennell, A short Report upon Yellow Fever, as it appeared in Brazil (1850), p. 16; Lallemant, *op. cit.*, p. 10; see also Report of New Orleans Sanitary Commissioners for 1853, pp. 158, 170.

⁴ Savarésy, p. 260; Bally, pp. 268, 334; Dariste, p. 210; Fontana, p. 170; Arnold, p. 26; N. Dickinson, p. 13; Blair, p. 59.

⁵ Dariste, pp. 37, 38; J. Clarke, p. 22; Humboldt, pp. 772, 773; Kéraudren, p. 24; Imray, *Edin. Journ.*, liii. 94; Ferguson, *Med.-Chir. Trans.*, viii. 144; Bancroft, p. 268.

Not less certain is it, that the protective influence of acclimatization is lost by a prolonged residence in cold climates;¹ that the children of the natives of, and acclimatized to, tropical regions, do not enjoy the same advantages in regard to protection as their parents,² but acquire them rapidly as they advance in age; that the residents of some portions of tropical regions suffer to a certain extent from the disease, on removing to another and more insalubrious part of the same regions,³ or even to one differing but little in point of salubrity. We find also that the natives and the acclimatized residents of cities or towns in southern latitudes, where the yellow fever has either never prevailed before, or had not done so for many years back, are liable to be attacked—to a certain extent at least—(as at Guayaquil in 1840 and 1842, or Caraccas about the same time), when the disease breaks out in an epidemic form; that individuals acclimatized to a yellow fever locality sometimes lose, to some extent, the protection they had enjoyed, by a long exemption of that locality from local sources of infection, or by its being favoured, during several successive summers, with a cooler and purer atmosphere than before;⁴ and that the same results obtain when individuals change their residence to places where the effluvia evolved, though not more detrimental to health, are of a different nature from those to which they were accustomed.

We cannot be surprised at this. It has been remarked, as we learn from Mr. Cornuel, whose experience in the matter is extensive, and whose judgment is mature, that all the mutations in the location of troops in the West Indies have to be conducted with the greatest care; for it is a matter of observation that all such changes favour in a singular manner the development of disease. Suppose two companies placed in two different localities: their sanitary condition is excellent: they have neither of them any sick in the hospital. Exchange their location—in effecting this, take the greatest precaution; make the change by sea, shelter the soldiers from the rain, the heat of day, and the dampness of night: in a word, act in the way best calculated to shield the men from the ill effects of fatigue and atmospheric vicissitudes. However you may act, each company will soon send a number of sick to the hospital. And yet, the hygienic condition of those localities has not changed; for the other portions of the troops that have not left their old quarters continue healthy.⁵

Captain Philips thought he escaped the fever by remaining in town and visiting the sick, thus accustoming himself to the town air. “This I did by

¹ Rochoux, pp. 40, 41; Maher, p. 884; Bally, pp. 332, 340; Arnold, p. 26; McArthur, in Johnson, 350; R. Jackson, *Fevers of Jamaica*, p. 250; H. McLean, p. 187; Pugnet, pp. 345, 346; Frost, *Med. Repos.*, xii. 224; Dariste, p. 37; Savarésy, p. 256; Bancroft, p. 268; Frazer, *Med.-Chir. Rev.*, xiii. 347; Rufz, *Med. Exam.*, iii. 109; N. Dickinson, p. 12; Veicht, pp. 111, 112; Hume, p. 267; Pinkard, ii. 481.

² Musgrave, *Med.-Chir. Trans.*, ix. 106, 107; Rufz, *Med. Exam.*, iii. 106.

³ Humboldt, pp. 761, 771, 772; J. Clark, pp. 1, 2; Pugnet, p. 346.

⁴ Chervin, *Report on Rufz's Mem.*, p. 44.

⁵ Cornuel, *Ann. Marit.*, 1844, ii. 739.

advice of the ever honoured and worthy Colonel Kendal, to whose kindness and affection I was beholden for a great many benefits and good offices here (being myself a stranger to the island), while those that kept in the country in better air, for fear of it, were commonly infected when they came on any business to town."¹ Leblond states that at Cayenne, in 1802, 300 soldiers arrived under General Degouges during the hot season of a sickly year; 200 of these died, including the general. While such was the fate of these men, an Alsacian regiment that had arrived there in 1793 to the number of 700, and most of whom were addicted to strong drink, remained exempt. They had arrived in a healthy year and during the cool season, and had had time to become acclimatized (p. 227).

Dr. Dickson (J. H.) in his *Essay on the Causes of the Tropical Endemic Yellow Fever*, relates a case which shows the effect of this process. It was communicated to him by letter. "While we were all ill, and dying in the *Alligator*, in English Harbour, shortly after our arrival in the West Indies, the *Emerald*, which had been two or three years in the climate, remained near us healthy, though under precisely the same circumstances of duty and exposure. The *Emerald* was succeeded in her situation by the *Carysfort*, fresh from Europe, which ship, in a few weeks, buried almost all hands."²

A few years ago, among others, two regiments arrived at Barbadoes—the 66th and 72d—the former from Gibraltar, the latter from Malta—and though stationed in the same garrison, and doing duty together, but occupying different barracks, though separated only by the parade-ground, at the outbreak of the disease, yet while the 66th in a few weeks after arrival suffered severely, the 72d escaped for not less than five months.³

In the French colonies, the time of the service of the troops is four years. Of course, the soldiers are all unacclimatized, and many of them fall victims to the yellow fever. To guard against the evil, the Governor of Guadaloupe distributed the above-mentioned force as follows: 1st year the troops soon after arriving are sent to Camp Jacob, situated in a healthy and high region: 2d year in the dependencies and on detachment service; 3d and 4th years in the towns of Point à Pitre and Basse Terre. The reason of this progression is obvious. The first year of residence in the colonies is that during which a European is most subject to the yellow fever. He is made to spend that time in the place where he is the least apt to suffer from the disease. The second year, his susceptibility has lessened, but it still exists in a high degree. Yet, as the exigencies of the service must be attended to, he is made to spend that time afar from the great centres of population, thereby avoiding the localities most subject to the yellow fever. During the third year, the soldiers reside in the towns, but only during the season when the yellow fever is less apt to occur; finally, during the fourth year, they are again transferred to the towns, where a portion of them continue to reside during the time of

¹ Philips, *Journal of a Voyage to Africa and Barbadoes in 1693-4*, Churchill, vi. 237.

² *Edinb. Med. and Surg. Journ.*, xiii. 39.

³ Davy, *Notes on Blair*, p. 58.

the *hivernage*—the most sickly and fatal of the year. By this means the mortality has been greatly lessened.¹

It is, however, proper to remark that, general as the rule undoubtedly is, it is not universal, but subject to occasional, and, according to some writers, more or less frequent exceptions,² especially during the prevalence of violent and widely diffused epidemics. This appears to have been the case very recently in the island of Martinique. Dr. J. Chapuis, in a communication to the Sanitary Commission of New Orleans, states that the epidemic of 1852, at St. Pierre, commenced on board the vessels anchored in the roadstead, and at first remained almost exclusively confined to their crews. The inhabitants of the city and the military of the garrison were then perfectly free from it. "I ought here," says Dr. C., "at once to turn to the question of acclimatization, for the military had nearly all been five years in the colony, while the sailors had only recently arrived in the country; but we shall see, further on, how much the epidemic deprived this privilege of acclimatization of its importance, so that it became no longer a certain preservative, but only a chance of diminishing the gravity of the disease. Nevertheless, I always reckon a recent arrival in the country among the predisposing causes of yellow fever, and I have to bring to notice the fact that, in this epidemic, contrary to what has happened in all those of which (accounts) have been recorded, the disease was observed to attack, and often carry off, not only Europeans who had already sojourned for longer or shorter periods in the country, but creoles."³ Phenomena of like nature are observed in our southern cities—New Orleans, Charleston, Savannah, and Mobile—where the fever is, as it were, endemic, and the heat considerable and of long continuance, and where the causes of insalubrity assume a character of permanency. There, as in tropical regions, the natives and long residents who have gone through the process of acclimatization remain, with few exceptions, free from the disease, which exercises its effects among strangers; so exclusively, indeed, as to have acquired, in Charleston and some other places, the name of the stranger's fever. So far as regards New Orleans, the power in question has been denied.⁴ But facts speak for themselves. In 1841, eighteen hundred deaths took place in New Orleans from the yellow fever. Of these, only three were natives of the city. One was three weeks, and three were two years old. In 1843, the deaths amounted to 692. Two only are certified as having been born in New Orleans; and these were proclaimed in a public journal to be two

¹ Cornuel, *Ann. Marit.*, 1844, ii. 738.

² Thevenot, p. 254; Griffith Hughes, p. 37; Chevalier, p. 6; Gillkrest, ii. 279; Pugnet, pp. 331, 346; Caillot, p. 142; Bally, p. 332; Savarésy, p. 134; Rufz, *Examiner*, iii. 109; Chervin, *Rep. on do.*, pp. 32, 44; Musgrave, *Med.-Chir. Trans.*, ix. 106; *Ib.*, *Med.-Chir. Rev. and Journ.*, iv. 981; Imray, *Edinb. Journ.*, liii. 79; Stevens, pp. 195, 201; Chisholm, ii. 234; Bancroft, *Seq. Ferguson Med.-Chir. Trans.*, viii. 139–150; Jackson, pp. 11, 35; *Ib.*, *Outlines*, p. 63; Lempriere, ii. 29; Hunter, p. 19; Hillary, p. 126; Catel, p. 6; Desportes, i. 192; Humboldt, pp. 771, 772; Dancer, p. 82; Guyon, p. 28; Warren, p. 4; Furlong, p. 290; Barry, in Boyle, p. 270.

³ Report, p. 211.

⁴ See Barton, in Fenner, ii. 128.

errors by the compiler of the dead list for that year.¹ Dr. Dowler found, on calculation based on the interments during the first two weeks of August, 1853, that, out of a mortality of 2,702, of which 2,369 were from yellow fever, one creole in 1,184 died of this disease, "out of a creolized city population four or five times greater than the non-creolized or strangers."² Again, he selected the first week in September, counting the number of deaths among persons who had been born in New Orleans. "Upon examining the alphabetical list as distributed among the twelve cemeteries, 9 individuals proved to have been born in New Orleans: 1 aged two, 1 three, 1 seven, 1 eight years; 2 aged six months, 1 eight months, 1 eighteen months; and 1 mentioned as an 'infant'—giving an average of $34\frac{1}{2}$ months. During these seven days, the total mortality was 741, of which 560 were from yellow fever, and 33 under the head of unknown, making 593—leaving 148 for all other diseases" (p. 32). In the St. Louis (French) Cemetery of the city, the mortuary tableaux for six months, ending November 1, show a total of 6 from fever among individuals born in New Orleans: 1 twenty months; 5, respectively, three, ten, twenty, and twenty-two; and 1 infant. The whole number in that cemetery was 206. In the Protestant Cemetery, in which creolized Americans are buried, the mortality in six months was 430; of these 8 were from fever: 1 a child; 1 twenty-two months; 3 two years; 1 five; 1 fourteen; and 1 eighteen. These people usually emigrate.³ The other cemeteries gave much the same proportions.

In our southern cities, as within the tropics, true and effective acclimatization is enjoyed only by the residents of localities liable to the disease, and not by those who reside in country districts, or even in the suburbs of infected cities. The inhabitants of Charleston may be cited in illustration of this: "Those who live in the higher parts of that State at the distance of two or three hundred miles, and who come to Charleston during the four months in which the yellow fever commonly prevails, are as liable to be attacked by it as strangers; and, therefore, all intercourse between the country and city is suspended for one-third of the year, excepting that of a few white persons who, from necessity, go to the latter, always taking care, however, not to *sleep* there." Basing his opinion on these and other facts, Dr. Dowler concludes that among the acclimatized to the city the security is perfect. "In a great number of works on the yellow fever of the West Indies, or even of Louisiana, where creoles do not suffer from this disease, the true explanation is that these persons are creoles of the country, not of the city; or, at most, they reside in the latter occasionally, chiefly in the winter, and are therefore liable to the disease, though they usually have it in a milder form than strangers, and rarely die."⁴ Dr. McFarland is also of opinion, based on extensive experience, that the protection afforded by acclimatization in New Orleans is perfect; and much the same views are entertained by other writers.

In our southern cities, as within and near the tropics, the protection, though

¹ Dowler, *Fever of 1853*, p. 36.

² *Ib.*, p. 32.

³ *Ib.*, p. 32.

⁴ *Ib.*, p. 35.

usually perfect, fails sometimes to be exercised. This generally occurs in times of violent visitations, when the epidemic influence spreads widely and imparts a malignant garb to the disease. In Charleston, in 1817, this peculiar circumstance existing, "several of the natives and old residents died of that complaint."¹ In 1854, the same thing took place in that city and Savannah. Even here in Charleston, as I learn from a distinguished correspondent, they can number quite a figure of natives, of adult age, who have suffered. The number, however, appears after all to have been smaller than at first believed. Dr. P. C. Gaillard, who investigated the subject, calls attention, in a communication intended to correct misapprehensions on it, to the fact that in the 621 deaths, which, according to the records of the City Register's office, constituted the full amount of the mortality from yellow fever, only three adults native of Charleston were included.² In Savannah, in 1854, the mortality from yellow fever amounted to 594. Of this number, 56 were white natives; 25 of these were above ten years of age, and 31 under.³ Of Mobile, in 1853, Dr. Nott says: "No acclimatization, short of an attack of yellow fever, has served this year as a protection; not only many who have lived here fifteen or twenty years, and passed through several epidemics untouched, but grown-up natives, and even those advanced in life, have been fatally attacked."⁴ The same fact has been recorded of New Orleans during the terrible epidemic of the same year. Dr. Fenner remarks on this subject: "One of the most extraordinary features of this epidemic is presented in the fact that the natives of the city, both white and coloured, have suffered severely, and many of them have died." "I am informed that the creoles on the coast, above and below the city, suffered much worse from yellow fever than those in the city, many adults having died of the disease."⁵ These, it should be borne in mind, have never been proof against the fever, when they visit the city in epidemic times, and could not well fare better when it broke out among them. The cases cited by Dr. Fenner as having occurred among city creoles, were all children except two, who were sixteen and seventeen years old. From a valuable document contained in Dr. E. H. Barton's excellent *Report on the Sanitary Condition of New Orleans in 1853*, we learn that, out of a population of 46,004 creoles of the city and State, the mortality was at the rate of 3.58 per 1,000. It must be stated, however, that this mortality, with very few exceptions, was entirely confined to individuals under ten years (p. 248); and that while such was the proportion among this class, the unacclimatized perished at the rate of some hundreds in a thousand. Like West Indians, the acclimatized inhabitants of our southern cities lose the protection they possessed by a prolonged residence in some northern place, or in a rural district of the same region; while their children, and those of the natives, are as prone to the disease as strangers themselves.⁶

¹ Dickson, Med. and Phys. Journ., iii. 266.

² Charleston Med. Journ., x. 137.

³ Report of John E. Ward, Mayor, &c., pp. 31-61.

⁴ New Orleans Journ., x. 577, 583.

⁵ Fever of 1853, p. 56; Trans. of Am. Med. Assoc., vii. 470.

⁶ See on all these subjects, Gros, Rep., p. 7; Girardin, pp. 49, 51; Thomas, 1st ed.,

The same, though to a more limited extent, is found to be the case in various of our less southern cities—as Natchez, Vicksburg, Grand Gulf, and Norfolk—where the protection afforded by acclimatization is not sufficiently effective to limit the inroads of the fever to strangers. For there the natives and long residents, while less liable to the disease than the latter, are nevertheless far from being entirely exempt. There, also, as in the cities of our Middle States and in Europe, the inhabitants of more southern or tropical localities, in which the yellow fever prevails endemically and assumes often the epidemic character, expose themselves with impunity to the causes of the disease.¹

If now we turn to the result of observation in the fever of this city, so far as relates to the subject more particularly before us, we shall find that here—as, indeed, in all localities in the Middle States where the disease has prevailed—and also in Europe, the existence of the two classes referred to—the acclimatized and the unprotected—does not hold. There is, strictly speaking, no privileged class among the natives of the soil, or long residents; all are more or less liable to become affected when exposed to the causes of the disease. If exemptions to this general susceptibility present themselves, they are few in number, and depend on organic peculiarities presently to be noticed. In other words, the process of acclimatization to the atmospherical agencies by which the inhabitants of these and corresponding latitudes in both hemispheres are surrounded, affords none of that protection against the disease which, as we have seen, the natives and the creolized stranger enjoys in tropical regions and in our southern cities. He is placed, as regards the yellow fever of his own place, in much the same condition of susceptibility as he would be respecting the fever of the tropics or of our southern cities.

Nor is it difficult to arrive at an explanation of this difference. Whatever

p. 77, 2d ed., p. 22; Michaud, *Voy. à l'ouest des monts Alleganys*, p. 5; Townsend (for fever of St. Augustin), p. 361; Ticknor, *N. Am. Journ.*, iii. 218; Barton, *Lecture on Acclimatement*, p. 3; Essay, p. 21; Strobell, pp. 139, 160; Bancroft, pp. 188, 192; Dickson, *Phil. Med. and Phys. Journ.*, iii. 257; Ramsay, *Rev. of Improv.*, p. 39; *Ib.*, *Med. Repos.*, iv. 218; *Ib.*, viii. 366–395; *Ib.*, *Hist. of South Carolina*, ii. 88; Drayton, *Rev. of South Carolina*, pp. 27, 28; Dickson, *Eclectic Journ.*, iv. 112; Simons, p. 13; *Rep. on Fev. of New Orleans of 1819*, pp. 35, 43; *Ib. on Fev. of 1839*, p. 324; *Ib.*, *Fev. of 1820*, p. 6; Monette, *Western Journ.*, iv. 339; Seagrave, *Med. and Phil. Reg.*, iii. 442; Waring, p. 59; Daniel, p. 63; Dupré, *Am. Journ. Med. Sci., N. S.*, ii. 382; Harrison, *N. O. Journ.*, p. 130; Dickson, *Essays*, p. 343; Lewis, *Fev. of Mobile*, *N. O. Journ.*, i. 417, v. 43; Fenner, *Southern Rep.*, i. 33; Shecut, p. 108.

¹ Merrill, *Chapman's Journ.*, ix. 246; Cartwright, *Recorder*, ix. 15; Taylor and Hansford, *Med. Rep.*, iv. 236; Selden and Whitehead, *ib.*, iv. 334; Archer, *Recorder*, v. 61. Bancroft, p. 266; Berthe, pp. 167–169; Fellowes, p. 59; Gilpin, *Med.-Chir. Trans.*, v. 317; Pym, p. 25; Arejula, pp. 183, 330, 446; Pariset, pp. 14, 15; Caisergues, pp. 200, 201; Rochoux, p. 123; Shecut, p. 108; Seaman, *Fev. of 1795 in New York*, p. 7; A. Hosack, p. 10; Warren, in Tytler, p. 501; Potter, p. 162; Drysdale, *Med. Mus.*, i. 39, 40; Dalmas, p. 100; Ramsay, *Edinb. Journ.*, viii. 429; Deveze, p. 55; Rush, iii. 80; Currie, p. 12; *Ib.*, *Barton's Journ.*, ii. 38; Cathrall, p. 6; Nassy, p. 38; Carey, p. 67; Condie and Folwell, v. 6; Caldwell, *Fev. of 1805*, p. 79; *Facts and Observ. by College of Physicians*, p. 19; O'Halloran, p. 119; Bartlett, p. 461.

view we take of the true agency of thermometrical heat in the production of yellow fever—whether we regard it as a predisposing and exciting, or as the efficient cause—certain it is that acclimatization, so far as relates to the fever in question, consists, in great measure, in the change produced in the system by long continuance of high atmospheric temperature, aided by peculiar hygrometrical and other meteorological conditions—barometrical pressure, electrical currents, &c.—and, in no small measure, by the permanent or frequently renewed impression of malarial impurities floating in the air, and constituting the usual sources of febrile infection. By constant exposure to these conditions of atmosphere—by a reduction of the stamina of the constitution—a reduction of animal heat and of the power of calorification, a natural effect of a continued residence in hot latitudes—a defibrination of the blood—a consequent diminution of the inflammatory diathesis—in a word, by a reduction of his vital forces, by the assimilation of his temperament to the standard of that of the natives, and by the power of adaptation of his organs to the poisonous exhalations floating around him, man becomes inured to the climate in which those influences are met, and gradually adapted to ward off or resist the action of the morbid cause.¹ Without this permanence or long continuance of high temperature, and of the other influences adverted to, the system cannot become modified in a way necessary to insure protection against the agency of the efficient cause. In tropical regions, where the temperature varies but little in the different seasons of the year, and in such of our southern cities where the winters are mild and of short duration, and followed by long and hot summers, this permanence exists to the degree required. Hence, individuals accustomed by time or otherwise to the climatic conditions they there meet with, become secure against the disease. But in Philadelphia and in temperate regions generally, things take a different turn; for while, in the south, the thermometrical depression and meteorological variations of winter are too trifling and of too short duration to correct the atmospherical conditions existing in the warmer seasons, to destroy the sources of atmospheric impurities, and to revolutionize the organism of those accustomed to the influence of those morbid agencies: while, I say, such is the case in southern latitudes, the resident of the temperate zone finds himself in a very different position. In a preceding chapter of this work I have called attention to the fact that the summers of this and corresponding regions are marked by high atmospheric temperature. It was there also stated that the heat in Philadelphia—and the same remark applies to every other large and populous city—is higher by several degrees than that of the surrounding country; and that, in fact, the summer climate of our and neighbouring cities may be assimilated to that of tropical regions. The heat thus experienced, and the morbid conditions of atmosphere created during the summer and autumnal months, are amply adequate to the production of the yellow fever. But this torrid temperature is

¹ Celle, p. 91–100; H. McLean, p. 188; Dickinson, pp. 39, 66, 69–99; Henderson, p. 5; Ferguson, *Med.-Chir. Trans.*, ii. 191; Lefoulon, pp. 20–1; Edwards, *Phys. Agents*, p. 154; Chisholm, ii. 472–8.

of comparatively short duration, and is succeeded by long and severe winters, and by a new train of meteorological phenomena, well calculated to destroy the telluric or other causes of the disease when they have existed, to revolutionize or renovate the system, and to prevent the establishment of those organic modifications which require time for their development, but which, when effected, render the individual who undergoes them insusceptible to an attack. To this must be added, that the efficient causes of the fever, whatever they may be, are not of annual occurrence, and the system cannot become accustomed to their agency. Natives and residents of temperate regions then find themselves, during each visitation of the disease, in the same position as the creolized or acclimatized inhabitants of warm regions, who lose the protection they enjoyed by a long absence in a colder and more salubrious climate. The low temperature and healthier atmosphere of a long winter destroy or prevent any change that might have been induced by the high thermometrical range of preceding seasons; and, on the reappearance of the disease, individuals so situated are as prone to an attack as if they had never been exposed to its causes.

This explanation, the most natural that can be offered, was suggested long ago by medical writers, and had not escaped the sagacity of Humboldt and other travellers.¹

But, although the natives and permanent residents of our city, like those of all other places in temperate regions liable to epidemical visitations of yellow fever, are all more or less prone, for the reasons mentioned, to the disease, it is found that they are somewhat less so than the inhabitants of the surrounding country, or of more northern sections, or than those of corresponding latitudes where the heat of summer is less oppressive and of shorter duration, and where the atmosphere is freer from sources of impurity. According to Deveze, the disease manifests a preference for Russians, Germans, and Englishmen; while the French—especially those from the southern departments—have it less generally. The same author remarks, that even a short residence in the pure air of the country increases the susceptibility to the disease (p. 108).² I have already stated that at New Orleans, in 1853, the mortality among the creoles of the city and State was at the rate of 3.58 in 1,000. In the same epidemic, the proportion among other classes was as follows:—

Strangers from Arkansas, Mississippi, Alabama, Georgia, and South		
Carolina	13.22	per 1,000
“ “ North Carolina, Virginia, Maryland, Tennessee, and		
Kentucky	30.69	“
“ “ New York, Vermont, Massachusetts, Maine, Rhode		
Island, and Connecticut	32.83	“

¹ Robin, *Voyage dans l'intérieure de la Louisiane*, i. 163-4; Humboldt, p. 772; Caillet, pp. 143-7; Barton, p. 21.

² Townsend, p. 250; Cartwright, *Recorder*, ix. 15; Potter on *Contag.*, p. 27; Archer, *Recorder*, v. 61; A. Hosack, p. 9; Drysdale, *Med. Mus.*, i. 39; Bayley, p. 87; Pariset, p. 35; Caisergues, p. 201; Fellowes, p. 219; Berthe, pp. 167, 374; Seaman, *Fev.* of 1795, pp. 6, 7; Rayer, p. 23; Arejula, p. 330; Pym, p. 160.

Strangers from Ohio, Indiana, Illinois, Missouri	44.23	per 1,000
“ “ British America	50.24	“
“ “ West Indies, South America, and Mexico	6.14	“
“ “ Great Britain	52.19	“
“ “ Ireland	204.97	“
“ “ Denmark, Sweden, and Russia	163.26	“
“ “ Prussia and Germany	132.01	“
“ “ Holland and Belgium	328.94	“
“ “ Austria and Switzerland	220.08	“
“ “ France	48.13	“
“ “ Spain and Italy	22.06	“ ⁽¹⁾

Nor is it less certain, that persons acclimatized in tropical latitudes and our southern cities escape the disease when exposed to it in temperate regions, and that long residence in India lessens the susceptibility (*Pym*, p. 10). This exemption, which was noticed in the various visitations of the fever in Spain and Italy—Cadiz, Seville, Gibraltar, and Leghorn²—as well as in all our Middle or other States, more particularly in reference to the French refugees from St. Domingo, who arrived in this country in large numbers in 1793,³ was fully exemplified by the result of observation in this city from the first to the last of our epidemics. By a French writer of some distinction and great pretension (*Rochoux*, p. 123), this insusceptibility of the individuals in question has been denied, under the impression that from this supposed liability of the acclimatized to the yellow fever of temperate climates he might derive an argument in favour of his peculiar views respecting the want of relationship between that disease and the yellow fever of the tropics. He bases his denial, so far as relates to this country, on an erroneous quotation from the Spanish translation of Dr. Rush's history of the epidemic of 1793, in which our great physician is made to say that “all the acclimatized were *not* exempt from the epidemic of Philadelphia.” He infers the same from some ambiguity existing in a sentence contained in the last edition of Deveze's treatise on the yellow fever; and appeals to the testimony of anonymous authorities. Another writer, not content with supposing the refugees to have been occasionally affected, makes the following assertion: “The creoles from the Antilles who pass to the United States are there subject to the yellow fever, but frequently resist the disease. In 1798, the French who had escaped to New York and Philadelphia *were all attacked*. Only one of them died.”⁴ This precious piece of information was furnished to the writer by Moreau de St. Méry, the distinguished author of a history of St. Domingo which was published here about fifty years ago. A reference to the authorities already

¹ Barton, Report, p. 248.

² Bancroft, pp. 190, 446; Berthe, pp. 167–9; Fellowes, p. 59; Gilpin, Med.-Chir. Tr., v. 317; Pym, p. 25; Pariset, pp. 14, 35; Caisergues, pp. 200, 201; Arejula, pp. 183, 330.

³ Seaman, Fev. of N. Y. in 1795, p. 7; A. Hosack, p. 10; Shecut, p. 108; Warren, in Tytler, p. 501; Potter, p. 162; Drysdale, i. 39, 40; Dalmas, p. 100; Ramsay, Fev. of N. Y., Edinb. J., viii. 429; Archer, Rec., v. 61.

⁴ Fournier Pescay, in Dict. des Sci. Méd., art. Fièvre Jaune, xv. 360.

cited would alone be sufficient to counterbalance all that Roehoux and other writers may say on that subject; but it may not be improper, with the view to set the question at rest, to appeal to the very writers he cites, and to other testimonies we possess. If there exists some ambiguity in the statement made by Dr. Deveze in the last edition of his work, "that the inhabitants of the Antilles are never affected by the disease, *or resist perfectly its attack*;" if the meaning of the author, though plain enough to common understandings, can be explained away by a constrained interpretation of words, no apprehension of the kind can attach to the meaning conveyed by the following passage taken from his second edition, which, like the former, is now before me: "I have observed and acquired the certainty, that individuals arriving from the Antilles where they had become acclimatized by long residence, or an attack of the disease, were always exempted from the yellow fever, though exposed a long time to the causes giving rise to it in other individuals. This fact, which I noticed during the epidemics of 1793 and 1797, has since been confirmed to me by correspondence" (p. 55). Dr. Rush, whose expressions, had they been accurately translated, would have saved Dr. Roehoux from hazarding the erroneous inference alluded to, says expressly that "the refugees from the French West Indies universally escaped it" (iii. 80). Dr. Currie, in the history of the same epidemic, says: "The French West Indians, particularly those from St. Domingo, who had taken shelter here from the storms of a sanguinary war, almost to a man, escaped the disease, though they made no use of any precaution for the purpose; while those from France were as liable to it as the Philadelphians" (p. 12). In other publications, the same distinguished writer estimates the number of these emigrants at 2,000, residing in different parts of the city, and adds: "They escaped the disease, though surrounded for more than two months by the sick, the dying, and the dead, and breathed the same air as the rest of the inhabitants."¹ Similar, and as positive statements are made by Cathrall (p. 6), by Nassy (p. 38), and by Carey (p. 674).

The same result was observed to occur in subsequent epidemics. Speaking of 1798, Dr. Pascalis, who then resided in this city, says that the disease attacked all classes "except the West Indians, and they have escaped it as heretofore."² In 1805, according to Dr. Caldwell (p. 79), creoles, &c., acclimatized to the West Indies, escaped. The following remarks, borrowed from a work of authority, substantiate the fact beyond the possibility of doubt: "A striking peculiarity which does not occur in any other disease, attends the yellow fever in the West Indies. The natives, and persons who have resided long in those islands are very seldom seized with this fever. It was likewise remarked, and it is a circumstance that deserves particular attention, that very few, if any, of the creole French in this city suffered from the contagious malignant fever which prevailed here in 1793, 1797, and 1798, though the disease was introduced into their families, and children born in

¹ Barton's Journ., ii. 38.

² Letter to Condie and Folwell, in their Essay, pp. v. and vi. of Appendix.

this country of creole parents, died with it last autumn, while the parents and the children born in the West Indies were entirely exempt from it.”¹

To this, I may be permitted to add, that issuing as I do from, and reared as I have been, in a family of St. Domingo colonists, who reached the shores of this country in 1793, settled in Philadelphia, and went through the epidemical seasons of that and subsequent years—familiar, from constant intercourse, with the habits, course of conduct and opinions of hundreds of other colonists, as well as with the professional views of all the French physicians who, like Dr. Deveze, emigrated from that island, and practised here, from that to a recent period, I possessed the best opportunities for ascertaining the real amount of protection enjoyed by the acclimatized refugees. I am now prepared to state that from none of those individuals, professional or otherwise, did I hear the expression of the faintest doubt as to the reality of that protection, or of a well-authenticated case of the reigning fever among the whole class of West India refugees. And in confirmation of this statement as to the views of the physicians in question, I must be allowed to refer to a MS. essay, prepared in 1798 by one of them for the Academy of Medicine, and which now lies before me. In it, the writer, while combating the doctrine of importation and contagion, makes use of the following positive language: “Il semble même qu’on aurait du en éloigner d’autant plus l’idée que les émigrés de ces différentes îles qui sont venu en grand nombre chercher un asile sur votre continent ont vécu au milieu de vous et des malades, sans qu’un seul d’entre eux ait été attaqué.”

What may have been the duration of the protection which the acclimatized refugees carried with them to these shores; whether, as happens in tropical regions and our southern States, with those who return there after an absence of some years in cold latitudes, the West India emigrants lost the privilege of exemption by a residence north, cannot now be positively ascertained. Dr. Potter, of Baltimore, is of opinion that in that city it was so far impaired as to place the individual, once possessing it, on a par, in point of susceptibility, with the natives themselves; and states in proof that the emigrants who had securely passed through the epidemics of 1797 and 1810, “suffered as much, *cæteris paribus*, in 1819 and 1821, as any other variety of the human species.”² It must be borne in mind, however, that Dr. Potter did not recognize the existence of any difference between bilious remittent and yellow fever, against the former of which emigrants have never been found protected. On the other hand, Dr. Dalmas, who practised in New York, and was in constant intercourse with the French refugees there, testifies in favour of the continuance of this immunity, and states that during the eight years he spent on this continent, where the number of refugees was very large, none were attacked, though they almost invariably remained in the city throughout the whole course of the epidemics—residing in districts

¹ Facts and Observations by College of Physicians, p. 19.

² Notes to Gregory, i. 162.

recognized as the most insalubrious; and exposing themselves in every way possible to the influence of the disease (p. 100).

In reference to this city, we find little or nothing calculated to justify a belief in the loss by time of the protective power of acclimatization. The remark of Dr. Deveze, that negroes, *like the whites from the West Indies*, acquire a susceptibility by residence in cold and temperate regions, conveys no positive information as regards our fever. We have seen that, in 1798 and 1805, the refugees were still in possession of the privilege they had formerly so strikingly enjoyed; and it is a fact well known to me, that the late Dr. Monges, who died in 1827, and who probably saw more of the fever than any other physician here, discovered nothing during our later epidemics likely to modify the views he had formed as to the continuance of that immunity.

As to the time required to obtain protection through the process of acclimatization, no positive information has as yet been obtained. We have seen that, in the French West Indies, a residence of four years is regarded as sufficient to secure the troops from the fever. According to Dr. Doughty, however, a residence of that duration does not secure the British soldiers at the season when, and in situations where, it most prevails, if, previous thereto, the constitution had not been exposed to the operation of the cause (p. 65); in other words, the individual must, in order to be well secured, have become acclimatized to the place infected. Dr. Barton is of opinion that there is no period of residence, in New Orleans, that will *certainly* exempt one from the fever, and adds, in confirmation of this, that the cemetery records show the fact that people die of it after having been in that city five, ten, or more years.¹ We have seen that, in Mobile, a residence of fifteen or twenty years has not been sufficient to protect individuals against the fatal impression of the cause. But these, as well as the occurrence of the disease among resident natives, are exceptions to a rule, and I feel disposed to believe, from all the information I have been able to collect on the subject, that, in the West Indies and our southern yellow fever cities, while such exceptions are sometimes seen in ordinary sickly seasons, and comparatively often in periods of violent epidemics, immunity from residence, with or without the fever, is equal to that afforded by congenital or native creolism; that it is usually acquired in less than ten years—often in five—not unfrequently sooner; and that it is much more perfect when the individual has resided in the place during the prevalence of an epidemic, especially when the latter has been of a severe and malignant character, than when he has done so during a succession of healthy seasons.²

¹ Fenner's Southern Rep., ii. 131; Rep. to the State Med. Soc., p. 31.

² Dowler, p. 37.

CHAPTER II.

SECOND ATTACKS.

By those who are practically acquainted with the autumnal fevers of these regions, it is affirmed that an attack of the common bilious remittent—differing, in that respect, from attacks of some other zymotic diseases—invariably fails to exhaust the susceptibility of the system to the morbid impression of the efficient cause, and thereby never serves as a protection against subsequent attacks; the system remaining ever after as liable to infection as it was before passing through the complaint. There are not wanting those, indeed, who affirm, that one attack increases the susceptibility of the system, and predisposes it to a repetition of the disease from even a slight exposure to the cause.¹

However unwilling I am to adopt these views—particularly the latter—to their fullest extent, and still less to extend the denial of the loss of susceptibility in question to all the various modifications of form of such fevers as they occur in all the diversified latitudes within the range of their extensive empire, it is not to be denied, that a repetition of the disease in the same individual, on renewed exposure to the morbid impression of malarious influences is an occurrence familiar to every medical observer. On this point there is no difference in the results, whether we turn to the remittents of this country—north, south, west, or east—or whether we inquire relative to the fever of Europe and of tropical regions generally. Everywhere individuals who have passed through the disease remain more or less liable to a repetition of it; in other words, the susceptibility of the system to a second attack is not, as a general rule, exhausted by the prior infection.

In regard to the protective influence exercised on the system by an attack of yellow fever, much difference of opinion has long existed, and continues to exist. By some it is strenuously maintained, that an attack of the disease, whether mild or severe, securely and forever shields the individual from further danger; while by others it is affirmed with equal confidence that the yellow fever is in no way different, in respect to its protective power, from the bilious remittent of our vicinity.

Postponing, for the present, a statement of the views entertained on this question by both parties, and of the facts by which they are sustained, it may be remarked, that in tropical climates and Europe, as well as in the various parts

¹ Ashbel Smith, *Tr. Acad. of Med. of N. Y.*, i. 59; Tullock, *Statist.*, 46, 340; Parry, *Am. J.*, Jan. 1843; Dickson's *Essays, &c.*, pp. 344-5; *Ib.*, in Strobell, p. 129; *Ib.*, *Charleston Journ.*, vii. 840; Wood, i. 304; Simons, p. 21; Strobell, pp. 203-4; Cameron (*Fev. of Ceylon*), *Edinb. Journ.*, lxxi. 71.

of this continent which are obnoxious to the disease, the same question has long been agitated. A reference to the writings we possess will show that, in the former, the doctrine of protection has so far gained the ascendancy as not only to be upheld by the highest professional authorities, but to be received, with scarcely a dissenting voice, by the public at large. Let the reader consult the accounts extant of tropical fever—by whatever name designated—and he will perceive how generally adopted is the belief in the rare occurrence of second attacks;¹ and he will see, also, that those who entertain different sentiments usually regard the fever as one of *seasoning*, which generally at least preserves the system, while the individual remains under the influence of the climate, and is mindful of the precepts of hygiene. Dr. Blair remarks that, although some of the inhabitants of Georgetown, who passed through the epidemics he describes, had resided there during the fever of 1819, there was no instance of a sufferer from the latter being attacked in 1837 and following years. “Neither do I believe there is an instance of a second complete attack after a month’s perfect restoration of health” (p. 85). The disease, however, was frequently baffled, and returned; and the liability to a return was in proportion to the mildness or shortness of the previous attack (pp. 85, 6).

In Europe, also, the belief in the protection afforded by an attack of yellow fever is very generally prevalent among those who have there had occasion to investigate the character of the disease. “The pestilential fever of Spain,” says Sir James Fellowes, “has never been known to attack a person a second time in that country; this fact, which was first observed by the native practitioners, has now been confirmed by the experience of several years, and by the concurrent testimony of all the surviving inhabitants of those places where the disorder had most prevailed.”² A writer quoted by Berthe,³ whose account of the epidemic of Andalusia appeared in 1802, speaks of the immunity from second attacks in the most explicit manner; and Arejula is not less positive on the subject when he says “the yellow fever of Andalusia only attacks persons once in their lives; and it is of great importance for the physician to know this in order to form his prognostic and his plan of cure, as well as for the individual who may have passed through the disorder, that both of them being assured of this fact, may step forward without fear to the relief of their fellow creatures who may hereafter be afflicted with so dreadful a malady.”⁴ It was with the view of insuring better attend-

¹ Rochoux, p. 38; Pugnet, pp. 348–9; Chisholm, ii. 233; Ib., Manual, p. 192; Humboldt, p. 775; R. Jackson, *Fev. of Jamaica*, p. 250; Ib. (Sketch), i. 43; Frost, *Fev. of Demerara*, Med. Rep., xii. 224; Doughty, p. 183; Williamson, i. 311; Dickinson, pp. 40, 49, 68; Blair, pp. 85–6; Veitch, p. 111; Davy, *Notes on Blair*, p. 85; Stevens on the Blood, pp. 198, 201; Savarésy, p. 256; Madrid, pt. 2, p. 32; H. McLane, p. 187; Pym, p. 25, and Appendix; Maher, p. 884; Caillot, p. 249; Gilbert, p. 76; Dariste, pp. 38, 112; J. Clark, p. 19; Blane, *Diss.*, ii. 147–8; Peixotto, i. 415; Bally, p. 289; Moseley, p. 432; Pinkard, ii. 96; Vincent, p. 13; Hume, p. 241.

² P. 23 of Introduction.

³ Note, pp. 156, 394.

⁴ Arejula, *Breve Descripcion de la Fiebre Amarilla, &c.*, pp. 190–1; Sir J. Fellowes, p. 67.

ance on the sick that the fact of immunity resulting from an attack was publicly placarded, by advice or direction of that celebrated physician, in the streets of Medina Sidonia in 1801,¹ and of Malaga in 1803.² Equally positive on the subject are Becker (quoted by *Fellowes*, pp. 129–198); Gonzales, p. 310; Sir Joseph Gilpin (*Med.-Chir. Tr.*, v. 318); Amiel (*Johnson on Tropical Climates*, p. 269); Pariset (*Fever of Cadiz*, p. 97); Ib. (*Fev. of Barcelona*, p. 456); Gillkrest (*Cycl.*, p. 280); T. Smith (*Edinb. Journ.*, xxxv. 37); Wilson, pp. 73–4; Sir David Barry (*Med.-Chir. Rev.*, xii. 540, xiv. 76); Louis, pp. 312, 319; Acad. of Med. of Barcelona (*Rapport, &c.*, by Rayer, pp. 20, 42, 49); Rubini (*Reflessioni sulle Febbri Gialla*, p. 51); Tommasini, ii. 515; Risueno (quoted by *Burnett*, p. 243); and we all know how strenuously and laboriously Sir William Pym, whose knowledge in yellow fever was in great measure³ derived at Gibraltar, advocated the fact of the immunity from second attacks both by a reference to the results of his own experience and to the reports of Drs. Vance (p. 64), Frazer (p. 55), Redmond (p. 74), and Rocket (p. 69).

But while the doctrine of this immunity has thus obtained the sanction of high medical authorities as regards the fever of tropical climates and Europe, there are not wanting writers of experience in both regions who maintain that second attacks of yellow fever are of occasional occurrence; and others again who affirm that instances of the kind frequently present themselves. In a word, though there is reason to believe that in many cases of reputed second attacks the disease has been confounded with bilious remittent fever, we are not without testimony to show that the preservation in question is not universal and absolute, and that, like the protection afforded by simple acclimatization obtained by long residence in hot climates, it is liable to occasional exceptions, and is in fact, as Dr. Hennen observes, only a general result. On this point, we have the testimony of numerous and high professional authorities.⁴

Nay, even among those who advocate the doctrine of protection, we find some—as Savarésy (p. 256), Caillot (p. 229), Dariste (p. 38), Amiel, in *Johnson* (p. 269), McWilliam (*Fever of Boa Vista*, p. 112)—who admit

¹ Berthe, p. 336; *Cycl.*, ii. 280.

² T. Smith, on Fever of Gibraltar (1828), *Edinb. J.*, xxxv. 37.

³ *Bulam Fever*, p. 29, &c.; Appendix, p. 302.

⁴ Labat, ii. 74, iv. 303; H. McLean, p. 188; Ferguson, *Med.-Chir. Tr.*, viii. 124; R. Jackson, *Sketch*, i. 18, 43; Musgrave, *Med.-Chir. Trans.*, ix. 26, 116; *Ibid.*, *Med.-Ch.*, R. and J., iv. 785; Ralph, *Edinb. Med. Ch. Tr.*, ii. 78; Bancroft, p. 178; Maclean, *Epid.*, i. 219; Frost, *New York Med. Rep.*, xii. 224; Fraser, *Med.-Chir. Review*, xiii. 347; Gray, and W. Trot, in *Burnett*, *Off. Corresp. on Boa Vista Fever*, *Edinb. J.*, lxviii. 497; Joubert, p. 969; Birnie, *Edinb. J.*, xiii. 338; McArthur, *Johnson on Trop. Cl.*, p. 350; Dickson, *Edinb. J.*, xiii.; Mortimer, *Med.-Chir. Rev. and Journal*, iii. 100; Comrie, *Edinb. J.*, i. 179; Evans, pp. 275–6; Pinkard, ii. 170; Arnold, p. 62; Maher, p. 884; Doughty, p. 183; Report of Sickness, &c., of Mediterranean, p. 9; *Burnett*, Report on Fever of Ascension, p. 50; Furlong, p. 291; Wilson, p. 190; Catel, p. 17; Veitch, and many others cited by Bancroft (*Sequel*, p. 51, &c.), and Chervin.

directly, or by implication, the occasional and exceptional occurrence of such cases, which they attribute to peculiarities of constitution or idiosyncrasy.

Dr. Ferguson, who, by the way, calls it seasoning fever (*Recol.*, p. 142), says, in the *Med.-Chir. Trans.*, viii. 124: "We are aware that as it is particularly a disease of the robust, the sanguineous, and the rigid of fibre, he who has escaped from one serious attack is completely disqualified for a second in this climate, until he can find the means of restoring the inflammatory diathesis by a course of unnaturally high gross living to which Englishmen are so prone; and we do not deny that the susceptibility towards a very acute disease may be greatly lessened by the exhaustion of that principle through the attack of malarious yellow fever."

Dr. Davy, in his notes on Blair, says that cases of the kind have been brought to his notice by an experienced medical officer, staff-surgeon Richardson, who states that a friend, a staff-surgeon, whom he attended in Barbadoes, had a narrow escape from a second attack—some years intervening and the individual having been out of the yellow fever zone. In the Report of Sickness, &c. (Jamaica), of British Army, we find the following statement: "Unlike the yellow fever of Gibraltar, one attack of the remittent fever of this country (Jamaica) secures no immunity from a second; so far from that, the returns show that 75 out of every 100 of the strong that have been under treatment for it annually; that is, on the average, each man, whether resident for a long or a short period in the climate, has had an attack every 16 months" (p. 46).

While such is the array of testimony in favour of the occasional or frequent occurrence of second attacks in the yellow fever of the tropics, Dr. Jackson states that in Spain, during the epidemic of 1820, twenty well-authenticated instances came within his knowledge, of persons being attacked who had the disease before. Mr. Doughty (*Fever of Cadiz*, p. 183) remarks, that though persons who had passed through the aggravated form of endemic fever (yellow fever) are comparatively secure, yet they are so only for a time, till the susceptibility is again regenerated by a colder climate, or a purer air in the same climate. Other writers,¹ while believing in protection, in like manner admit the more or less frequent occurrence of second attacks of the disease.

Turning now to this continent, we find that the belief in complete or very general immunity was long ago proclaimed. Very nearly a century ago, Lining, to whom we are indebted for the first account of the yellow fever of this country, affirmed that, at Charleston, those escaped "who had formerly felt its dire effects though they walked about the town, visited the sick in all the different stadia of the disease, and attended the funeral of those who

¹ Bally, pp. 97, 290; Burnett, *Mediterranean Fever*, pp. 333, 334, 468, 470; Gardiner, (quoted by Bancroft—*Sequel*, pp. 56, 57); Gillkrest, ii. 280; Rochoux, p. 122; Frazer, *Med.-Chir. Rev.*, xiii. 346; Audouard, p. 31; T. Smith, *Edinb. Med. Journ.*, xxxv. 37; Maelean on Epidemics, i. 218, and on Quarantine, p. 223; Chervin's Notes to his Translation of Wilson, p. 79.

died of it."¹ And at subsequent periods, the same view of the subject has been taken by writers on the epidemics of the same city,² of New York,³ of Norfolk,⁴ New Orleans,⁵ Mobile,⁶ and Galveston.⁷

The admission of the occasional or frequent occurrence of such attacks has, on the contrary, been made in most of these places—in New York,⁸ Baltimore,⁹ Natchez,¹⁰ New Orleans,¹¹ and other parts of the South.¹²

In 1854, as we learn from Dr. Wragg, "the non-exemption extended to those who had it before. Six of these were so well proved as to admit of no doubt on the subject. Some of the patients were identified as having gone through the fever in this (the Roper) hospital in 1852, throwing up black vomit on both occasions. Others claimed to have had it in New Orleans, the West Indies, or South America; and two asserted most positively that they had already had it twice" (x. 83).

Such being the difference of opinion on this subject, not only in the West Indies and Europe, but in various parts of our country, we may readily understand that in this city the question of the protective power derived from an attack of yellow fever has given rise to considerable discussion. Dr. Cathrall, who practised extensively during the memorable epidemic of 1793, and for a long time after, states, as the result of his experience, that the disease does not appear ever to affect the same person twice. "Although," he says, "careful inquiry has been made by several of my medical friends and myself, it only appears that some of the patients had a slight relapse of fever, but without any of the distinguishing symptoms of the disease, and very soon recovered. This I have frequently observed in my own practice, and likewise in that of my friends; but I have never remarked a second attack of the disease, however constantly the patient may

¹ Edinb. Phys. and Lit. Essays, ii. 400.

² Dickson, i. 352; Ibid., Med. and Phys. Journ., iii. 273; Ibid., in Strobell, p. 129; Irvine, p. 31; Strobell, p. 202; Simon, p. 21.

³ Townsend, p. 247; Francis, New York Journ., i. 299; Hosack, i. 385; Kissam and Walters, quoted by Townsend; Seaman, in Webster's Collection, p. 40; Ibid., Med. Reposit., i. 319.

⁴ Archer, Med. Recorder, v. 61.

⁵ Rogers, quoted by Townsend, p. 247; Forry, Clim. of U. S., p. 205; Barton, p. 21, and Am. Journ., xv. 47; Lect. on Accl., p. 15; Med. Rep., xxii. 76; Dowler, p. 35; Girardin, pp. 47, 51.

⁶ Kelly, xiv. p. 375.

⁷ A. Smith, Tr. of Acad. of Med. of New York, i. 59.

⁸ Med. Repos. of New York.

⁹ Cartwright, Recorder, ix. 15; Stone, N. O. Journ., vi. 551; Hogg, West. Journ., i. 416.

¹⁰ Potter, Notes to Gregory, i. 162.

¹¹ Baxter, N. Y. Med. Repos., xxi. 5; Fenner, N. O. Journ., Sept. 1848, v. 206.

¹² Whitbridge, Remarks on Yellow Fever, Southern Med. and Surg. Journ., ii. 460; Lewis, of Mobile, N. O. Journ., i. 418; Ibid., on Fever of 1847, ii. 43, 44, 162; Nott, N. O. Journ., x. 577; Levert, in Drake, ii. 221; Cooke, Fev. of Washington (La.), N. O. Journ., x. 617; Fenner, Fever of 1853, p. 55; Wragg, x. 83; Barton, Rep. on Sanit. Condition of New Orleans in 1853, p. 289.

have been afterwards exposed to the contagion" (p. 8). The late Dr. Monges, than whom no physician had a more enlarged experience in the yellow fever of this city, and who observed all the epidemics by which the latter has been visited—from 1793 to 1820 inclusive—states that he never witnessed a second attack of the fever in the same individual.¹ The late Dr. Griffiths, in a letter to Dr. Hosack, of New York, remarks: "Respecting the question of reinfection in the disease called yellow fever, my mind has been long made up. I have never seen an instance of its occurring twice in the same person, during the seven periods of its occurrence among us; and as I was absent from the city only two weeks in 1793, whilst it was prevalent, and at no other time, my opportunities of making observations of this kind were abundant."² A similar opinion appears to have been entertained by the late Drs. Kuhn, Wistar, and (at a later period of his life) Currie, all of whom had ample experience in the disease. It is entertained also by other writers.³

On the other hand, those familiar with the writings of Dr. Rush need not be told that that distinguished physician, so far from being an advocate of the doctrine of protection, maintained, in his various writings, that cases of reinfection were of frequent occurrence. In his account of the epidemic of 1793, he states that cases of the kind "were very common during the prevalence of this fever. They occurred most frequently where the first attack had been slight. But they succeeded attacks that were severe in Dr. Griffiths, Dr. Mease, my pupil, Mr. Cox, and several others, whose cases came under my notice" (*Works*, iii. 88). The same writer mentions instances of the sort in his accounts of the epidemics of 1794 (*Ibid.*, iii. 217), 1797 (*Ibid.*, iv. 17), and 1798 (*Ibid.*, iv. 43). Dr. Currie, whatever may have been his opinion at a later period, held much the same language as Dr. Rush, in his description of the epidemic of 1793, affirming that there were several instances of its occurring a second time, when the circumstances were so unequivocal that it could not fairly be ascribed to a relapse (p. 15).

When, unprepossessed in favour of either of the foregoing opinions, and calling to mind the facts that have fallen within the sphere of our personal observation, we weigh the testimony adduced in support of both, in the West Indies and Europe, as well as in the southern section of our country, it is impossible to withhold the conclusion that it would be little short of absurdity to deny that, as a general rule, an attack of the yellow fever exhausts the susceptibility of the system to further attacks, or renders it less liable to be severely affected by the poison of the disease: I say as a general rule; for instances of an exceptional kind are encountered. But, although facts, establishing the truth of the statement that second attacks are observed, are too numerous and too well attested to be called in doubt; and although some of those adduced as evidence of protection, by Dr. Pym

¹ N. A. Med. and Surg. Journ., ii. 58.

² Hosack's Med. Essays, i. 380.

³ *Ibid.*, pp. 379, 380; Chapman, on Epid., Phil. Med. and Phys. Journ., ix. 130; Klapp, Med. Recorder, v. 472; Wood, Practice, i. 304.

and others, may, with as much propriety, be ascribed to the operation of the simple process of seasoning as to the modification occasioned in the system by an attack of the fever, since we have no evidence of these having all passed through the disease; and, therefore, although the immunity in question may not be as absolute as some of its advocates fancy it to be, nevertheless the conclusion is inevitable, that second attacks, in individuals who have passed through the disease, are somewhat rare, and that the immunity obtained by that means is much more secure than that derived from simple acclimatization. Second attacks constitute, therefore, exceptions to a rule, and are perhaps but little more frequently met with than repetitions of other complaints through which the system usually passes but once. Like attacks of fever in persons acclimatized by long residence, they occur more especially in times of severe visitations, when the epidemic influence is powerful and wide-spread; and it is not unworthy of remark that, in seasons when the disease shows itself only in the sporadic form, it never affects individuals who have had it before. They were more common in New Orleans in 1853 than they had ever been before. Such was also the case in Charleston in 1854. Dr. John Davy, who believes, with many others, that second attacks are rare—"the exception, not the rule"—states, in his notes on Blair, that in 1818, in Trinidad, the fever being very malignant, thirteen cases of the kind occurred. In the barracks of Orange Grove, near Port of Spain, in less than two months—from the 28th of August to the 13th of October—the deaths from second attacks amounted to forty-two; and yet the number of men stationed there must have been small, as the accommodations are on a small scale (p. 85). Like the security obtained from long residence, though doubtless in a less degree, the susceptibility to an attack is reacquired by a prolonged absence from the place where it had occurred, and a residence in a colder and more salubrious climate.¹ Second attacks are more common in individuals who have had the disease in a mild form than in those who have suffered severely and during a violent epidemic;² and, as a general rule, it may be stated that such attacks are milder than primary ones.³

It may be said, doubtless, that much of what has been ascribed, in the West Indies, to the protective power acquired from an attack of the yellow fever, is always due to the effect of acclimatization; and no one who is aware of the extensive influence of this process, will refuse to admit that it is difficult to award to each its due share in the protection enjoyed by an individual when he remains there, or for some time after he returns to a temperate region where the fever may happen to prevail. Hence, we find that Dr. Bancroft,⁴ in impugning the views so strenuously advocated by Sir William Pym, as also Dr. Maclean,⁵ endeavours to explain in that way the fact

¹ Barton, *Lecture on Acclimatization*, p. 15; Veitch, pp. 110, 111; Fenner, *Fever of 1853*, p. 55.

² Fenner, p. 55.

³ Lewis, *N. O. Journ.*, ii. 43; W. Humboldt, *Rep. of Sanit. Commiss. of New Orleans in 1853*, p. 134.

⁴ *Sequel*, p. 42, &c.

⁵ *Epid.*, i. 223.

that individuals usually take the disease but once while continuing to reside within the tropics, or within the sphere of operation of the cause giving rise to it. In the language of the latter, Pym has confounded "the *fact* of the disease *not affecting*, with the *principle* of its *incapability to affect*, the same person more than once." In speaking of the *seasoning fever*, the same writer says: "It is occasioned principally by exposure to the sun or other powers of intense action, in persons unaccustomed to the operation of these agents in so high a degree. The habit, however, being established, or the disease having once occurred (and Dr. Bancroft thinks *any* fever will have the same effect), it will afterwards require a higher sum of power to produce in the same person similar effects; and hence, the same species of fever rarely occurs again in the same person. But it does not, by any means, follow that the liability to it does not exist; or that, by increasing the force of the cause in the ratio of the habit established, or of the susceptibility lost in consequence of the first attack, it would not be produced as before" (i. 225). Dr. Bancroft refers it to seasoning by residence, or to a similar effect, "known to result from the attack of *any disease* by which the vigour, excitability, and inflammatory diathesis, produced by a cold or temperate climate, is for a time removed." Dr. Jackson (R.) says that the same effect is produced by the ordinary remittent of the country (p. 250). Lempriere makes the same remark (i. 29). Pugnet (p. 349) says the same of malignant remittents or intermittents, but not of simple remittents; and several writers maintain that protection from the yellow fever is afforded by ardent fever caused by the action of the sun on constitutions unaccustomed to it. Dr. Ferguson's views are not very dissimilar, ascribing, as he does, the usual exemption observed to the destruction of the inflammatory diathesis by an attack; an effect which, if sufficient, may be reasonably supposed to be produced by a variety of fevers. It is probable, however, that the protection afforded by the fevers mentioned is not as effectual as thus stated, and we may safely suspect that the loss of susceptibility to the yellow fever ascribed to an attack of them was more probably due to the process of acclimatization than to any change occasioned by them in the system.

But, whatever may be the justness of the explanation offered by Drs. Bancroft and others, so far as regards the West Indies—whatever may be the support it receives from what has been said of the immunity from second attacks being enjoyed only by individuals who continue to reside within the sphere of action of the cause which produced the disease, and being lost by residence in a different climate—it can scarcely be supposed to apply to the south of Europe, where the natives, and those accustomed to the climate, being the sufferers, it is impossible to account for the innumerable instances of immunity by referring them to the operation of simple seasoning. It may be doubted whether the explanation will be acceptable to the physicians of our southern States, where the protection from acclimatization is obtained, for there the system is well known to acquire no security by having gone through attacks of bilious remittent and other fevers. Nothing, in those States, will afford such a security but long

residence or having gone through a *violent* epidemic untouched—a *mild* epidemic not being sufficient (*Harrison*)—and an attack of the true yellow fever. The explanation given by Drs. Maelean, Baneroft, and other writers of the same school, becomes still less satisfactory when we bear in mind that this protection, while not obtained in our southern States from severe fevers of other kinds, or by a residence of several years during healthy seasons, is so at once in individuals recently arrived and not acclimatized, from a sporadic attack of the disease which forms the subject of our present inquiries. Here, then, the immunity is the result of the change produced in the system by the fever in question, and not by the agencies assigned.

Be this, however, as it may, respecting the yellow fever of Europe, the West Indies, or of our southern regions, the explanation can have no force in regard to the disease as it appears in our middle States. In another part of this work, I have endeavoured to show that though our natives are somewhat less prone to the disease than the inhabitants of more northern latitudes whose constitutions are more robust and inflammatory, still the degree of protection they enjoy is, after all, so limited that nearly the whole force of the epidemics is borne by them; the difference, if any exists, appearing to arise more from the difference of temperament, mode of living, and social condition of the two classes, than from any other cause. For reasons there stated, there is no acclimatization possible in our latitudes, and the individual who has passed twenty years among us is as open to an attack of the fever as the one who has lately arrived—all things else being equal. Nor is it less certain that an attack of our ordinary remittent and bilious fever has never been found to afford any protection whatsoever against the true yellow fever; and the history of our epidemics shows that, when this disease prevails in cities or towns, individuals recently from the country, most of whom have probably passed through the former in some shape or other, are more prone to the latter than the inhabitants of the infected places.

Such being the want of acclimatization in this and other cities of the middle States—such the deficiency of protection obtained from attacks of the ordinary fevers of the country—we might be prepared to encounter here a greater number of cases of repetition of the yellow fever in the same individuals than appears to occur in other and warmer latitudes. But such is not the case; for, though no one can be prepared to gainsay the testimony of Dr. Rush and others, who have witnessed the occurrence of such repetitions, or affirm that the immunity is complete and absolute in every instance, and that those who have passed through the disease are, henceforward, irrevocably placed beyond the reach of further attacks, nevertheless, it may safely be affirmed that here, as elsewhere, attacks of the kind cannot be viewed otherwise than as exceptional. No more justified could we be in doing so in reference to our yellow fever than in regard to the fevers of the West Indies, in which, as has been shown, such cases undeniably do occur. Those who have passed through the disease, as a general rule, need have no further apprehensions; and, as there is no protection obtained from simple

seasoning, they must owe their future exemption to the organic modifications produced by the attack under which they had laboured.

When, with this before us, we investigate the facts adduced by the deniers of protection, we are disposed to conclude that the instances of repetition are even less numerous than they are reported to be. It is true, Dr. Rush speaks of the thing as of common occurrence, and mentions instances of persons having had the disease three or more times; and Dr. Potter, of Baltimore, not only states that, between 1793 and 1821, he attended more than one hundred persons in a second attack, but that he saw twenty-one in a third, seven in a fourth, three in a fifth, and one in the eighth attack.¹ Positive, however, as are such asseverations, and respectable as the authorities from whom they emanate may be, it is difficult to withhold the belief that many of such second, and especially the third, fifth, and eighth attacks adverted to, were nothing more than instances of bilious remittent fevers; in other words, we cannot greatly err in suspecting that, in the majority of instances adduced as evidence of repetition in the same individual, the bilious remittent has been mistaken for the yellow fever. This explanation is not a novel one. It has been advanced in the West Indies, where the inflammatory or malignant seasoning fever has often been confounded with true yellow fever, against which it affords no protection. The same explanation has also been offered by not a few in our country, one of whom says, of the fever of Charleston, that "as persons in that climate can never have the disease but once, there can be no doubt that when strangers experience a second attack, either in the course of the same season or at any subsequent period, the disease was mistaken in the first instance, and was not yellow fever."²

This opinion will be the more readily adopted when it is borne in mind that, with few exceptions, those who here and elsewhere contend for the frequent repetition of the yellow fever in the same individual are found principally among the advocates of the identity of that and other forms of autumnal fevers; while we look in vain, in the writings of those who entertain different pathological views, for the admission that such cases should be regarded otherwise than as exceptional. Entertaining the opinion that all the varieties of fever prevailing during sickly seasons are mere degrees or modifications of the same disease, the former may readily be understood to discover second, third, and eighth attacks in instances in which those who have no faith in the identity in question would feel disposed to establish a very different diagnosis.

But, even were we to admit that the disease, in all the instances of repeated attacks mentioned—whether the second or the eighth—was really the true genuine yellow fever, we should not be justified, from that circumstance, in denying the protective power of the disease; not only because such instances are, after all, comparatively rare, but because occurrences of an analogous kind are encountered in diseases which possess that power in a marked degree, and even in some of which it has been said to be a distinctive attribute. Second attacks of smallpox, scarlet fever, and other kindred diseases, are not

¹ Notes to Gregory's Practice, i. 162.

² Irvine, p. 31.

unfrequently encountered—nay, it is doubtful whether yellow fever repeats itself more frequently than either; and cases are on record, in which they have outdone, in point of repetition, anything that can be related respecting the yellow fever. Dr. Davy informs us, on the authority of a general officer (whose mother was the subject), of a case in which the smallpox was repeated eleven times in the same person.¹ Much has been said of the protection afforded by an attack of oriental plague. “It is a rooted opinion among the Frank population of the Levant,” remarks Dr. Williams, “that the same individual cannot contract the plague more than once, and it would appear that exceptions to this rule are rare.”² Dr. Tully is inclined to this belief. He informs us that of twelve persons employed as expurgators and hospital attendants, at Corfu (1813), ten of the number had the plague at Malta nearly three years prior, whilst the remaining two had suffered from the disease about four years previously at Constantinople. He employed four soldiers, who had had the plague at Corfu, as orderlies at Cephalonia. They all escaped. There were also amongst the number of expurgators and persons employed in removing the sick and burying the dead at Cephalonia, several who had the plague years before at Smyrna and other parts of Turkey. They used no precaution, and yet all escaped. While such was the exemption among these, every exertion to secure individuals who had not passed through the disease, too frequently proved abortive.³ To this subject I shall have occasion to revert in a future chapter, when facts will be adduced to show that the statements of Dr. Tully and others are opposed by many more leading to a different conclusion, and distinctly showing that the repetition of the disease in the same individual is not of unfrequent occurrence.

It may be remarked that the protection from reinfection, to which I have all along alluded, is afforded here by an attack of the disease, whatever be the place where the individual has gone through the ordeal. During the prevalence of our epidemics, it was found that persons who had had the disease in the West Indies or in our southern cities were secured against infection.⁴ On this point, the yellow fever of this city approximates to that of Europe and the West Indies, an attack of which serves to protect against each other. For it results from the testimony of Pym (pp. 25–28, 55–56, 150), Gilpin (p. 317), Sir James Fellowes (pp. 129, 198), Redmond (cited by *Pym*, p. 73), Rocket (*Pym*, p. 73), as well as of Pariset (pp. 456–7, 541), and of Ardeval, Cortez, Méry, and Dias (all four referred to by *Louis*, pp. 321–2), that, though instances of a contrary kind have been adduced by Dr. Ferguson⁵ and other writers, an attack of the fever in the West Indies placed the system beyond the reach of reinfection at Gibraltar and Barcelona; while the converse is established by facts mentioned by Sir William Pym (pp. 73, 306), on his own authority and that of other army and navy medical officers. It is shown, besides, that the fever of this city protects against reinfection in

¹ Notes to Blair, p. 86.

² On Morbid Poisons, ii. 295.

³ Tully, Hist. of the Plague of Malta, Gozo, Corfu, Cephalonia, &c., pp. 236–7.

⁴ Deveze, 2d ed., p. 55.

⁵ Med.-Chir. Trans., viii. 124.

Europe¹ (*Pym*, p. 28); and by Dr. Bryson we are told that those who had the fever elsewhere were free from it on the coast of Africa.

To what cause this exemption from reinfection is attributable, is a question upon which we need not tarry long, inasmuch as it may properly be ranked among those on which our knowledge is but limited. That we cannot in our attempts at explanation rest contented with the fact of the disease destroying so far the stamina of the system as to place it more effectively in a condition similar to that produced by acclimatization, and more apt to bear the usual agents of that process, is evident. The natives and residents of our Middle and Eastern States are deprived of the benefit of acclimatization, the long-continued and intense cold of the winter being an effectual and very natural preventive to those modifications in the economy imparted by the long-continued and intense heat of tropical and southern climates, and which constitutes, in part, the process of acclimatization. The bracing atmosphere of one or several successive winters renews the vital activity which may have been impaired by an attack, and not a long time elapses before the system finds itself in the same condition it was in before, and, as takes place in the native of the West Indies, who loses his acclimatization by a residence of some years in northern latitudes, his system, if no other causes were assignable for the exemption, would be as prone to the disease as if it had never passed through an attack.

Now, is it true that the exemption in question is solely due to the habit acquired of exposure to those local sources of atmospheric impurities which

¹ Sir Wm. Pym claims credit as the discoverer of the protective power imparted by an attack of yellow fever. In a communication which appeared in the twelfth volume of the *Edinburgh Medical and Surgical Journal*, he affirms that he made the discovery on the 20th of October, 1804. "The first Spanish physician that mentioned it was Arejula, and he did not publish until 1806. Sir James Fellowes gives the credit of it to the Spanish physicians generally: no individual one has claimed it. It certainly was not known among them in 1803, and I do not believe it was ascertained in 1804 until after the time that I discovered the non-liability of the West Indians." The *Report of the Army Medical Board* has awarded him the credit he claims on that subject. Another English medical writer, Dr. Chisholm, affirms that the promulgation of that important fact is due to him, and not to Pym; for, in his *Essay on the Malignant Fever of Grenada* (i. 233), which appeared many years before Dr. Pym's book was published, and several—at least three—before the epidemic of 1804 at Gibraltar, he distinctly stated that the fever attacked the human frame but once. (*Manual*, p. 192.) In opposition to all this, it may be mentioned that the publications placarded in the streets of Medina Sidonia, and Malaga, appeared in 1801 and 1803, and were, of course, founded on information obtained anteriorly to those dates. Berthe, in the work quoted in the text, and which appeared in 1802, quotes (note 156, p. 394), from a public paper, a letter dated Cadiz, 16th Florial (6th May), 1802, in which the writer states (in opposition to the opinion of Berthe) that, like the smallpox, the yellow fever is rarely to be feared by those who have been once attacked. Nor is this all. The opinion of non-infection has long been prevalent in the West Indies; and, were this not sufficient to disprove the claims of Sir W. Pym and Dr. Chisholm, we find, on reference, that in Dr. Cathrall's work on the epidemic of this city in 1793, which appeared in 1794, and in Dr. Lining's essay, which was published so early as 1752, the same view of the subject is upheld. The doctrine appears to be American.

are supposed to give rise to yellow fever? I presume not. These impurities manifest themselves at too long intervals, and exist during too short a time to enable the system to become so habituated to the impression as to bear it with impunity. Let it not be forgotten, besides, that there is no reason why the system should not, in the supposition of those causes existing permanently, have been so inured to their action as to be shielded from a first as well as from a second attack. And yet such is not the case. At every appearance of the fever, hundreds of our inhabitants are stricken down, and the survivors, though having none of the benefits of acclimatization—though enjoying renovated constitutions by succeeding winters, and having been placed for years beyond the reach of local sources of infection—pass through other epidemics with perfect impunity; while others, who have not suffered from an attack of the fever at some antecedent period, now experience its fatal effects.

If such be the case, and the effect cannot be due to the causes mentioned, we must look for the explanation to some other agency. But, as regards the nature of the latter, and the changes it occasions in order to produce the result in question, we are, and will probably long remain, in a blissful state of ignorance.

By some it is presumed that we may apply to the yellow fever and other kindred complaints, the explanation offered by Liebig of the protection from contagious diseases obtained by an attack, *i. e.*, that each of these is produced by the action of a species of ferment peculiar to it, upon as peculiar a matter contained in the solids or fluids of the body; by which means said matter is consumed, and thus is a reproduction of the disease prevented by the want of the material upon which the morbid action may be founded.¹ In reference to such a view of the subject I am not prepared to express an opinion, and have little difficulty in uniting in sentiment with those who state that, in the present state of our knowledge respecting the laws of vitality, and the modifications brought about in our tissues through the effect of morbid agencies, it would be impossible to offer a plausible explanation without running the risk of encroaching on the boundless field of hypothesis. All we need do, therefore, is to refer the power thus acknowledged to be possessed by the yellow fever to the yet mysterious property appertaining to certain peculiar specific poisons of occasioning such alterations in our organization as will place the latter in a condition necessary to enable it to resist the further agency of the same cause.

¹ Mitchell, Cryptog. Orig. of Fev., p. 128.

CHAPTER III.

TEMPERAMENT—SEX—AGE—RACE.

Temperament.—The history of the yellow fever in this and other cities of the United States—as indeed in every place where the disease has prevailed—shows that it affects differently, both as regards extent of diffusion and violence of attack, persons of various temperaments and constitutions. Writers on the fever of the West Indies tell us that individuals of the sanguine temperament—the robust, strong, and plethoric—are the most prone to the disease; that the danger lessens in proportion as the elements of the bilio-lymphatic temperament—the most prevalent among the natives and long residents of hot climates—predominates; that hence the disease selects strangers, among whom the endangering temperament is more commonly found to prevail; and that in proportion as the place of nativity or habitual residence of those strangers approaches those climates where the peculiarity in question prevails to the fullest extent, does their liability to the disease increase. They further state that the disease is more apt to occur, and assume a more virulent character, in persons whose mode of living is calculated to keep up that temperament, and retard the salutary changes effected by the process of acclimatization, or to bring out its elements in bolder relief, or again to elicit it artificially, for a greater or shorter space of time, in those originally differently circumstanced. At the same time they admit that this rule, though general, is subject to exceptions, especially in times of unusually violent epidemics, when the fever spares none, and affects indiscriminately individuals of all temperaments. Such is the language, with slight modifications, of every writer on the fever in question.¹ Now, when with these statements fresh in our memory we revert to the events of our epidemics, we might almost fancy that they had reference to the fever of this country. Thus Dr. Deveze, in his account of the epidemic which prevailed in this city in 1793, remarks, while having in view the occurrences of that year (p. 105), that it is a truth which has become popular in all countries subjected to the baneful influence of the yellow fever, that individuals of a feeble constitution are much less liable to it than those that are strong. “The man,” he adds, “who is endowed with a robust con-

¹ Desportes, i. 23, iii. 195; Williams, p. 51; Rochoux, pp. 26, 36; Moseley, p. 434; Leblond, pp. 95, 97, 98; Hillary, p. 146; Chisholm, i. 140; Pugnet, p. 347; R. Jackson (Sketch), p. 12, and other works; Musgrave, ix. 104; Ferguson, viii. 138; Savarésy, pp. 259, 260; Osgood, p. 16; Pinkard, ii. 80; H. McLane, p. 36; Dariste, pp. 19, 40, 218; Hume, p. 237; Caillot, pp. 15, 135–6; Bally, pp. 270, 292, 376; Arnold, p. 34; Blair, p. 60; Dickinson, pp. 9, 13, 82, 115; Catel, p. 10; Rufz, p. 34; Veitch, p. 92; Boyle, p. 270; Winterbottom, Med. and Phys. J., p. 7; Henderson, p. 4.

stitution, and with a sanguine or bilious temperament, is promptly attacked; and in him the disease marches on with more violence, and proves more usually fatal." Dr. Rush entertained much the same views. In his history of the epidemic of 1794 (iii. 202), he informs us that "the causes which predisposed to this fever were the same as in the year 1793; persons of full habits were most subject to it:" and in that work, as also in his account of the visitation of the preceding year (iii. 49), he dwells at some length on the injurious effects of those habits of living which place the system in a condition simulating the sanguine temperament, or increase the elements of the latter in those in whom it exists naturally—augment the strength and stamina of the individual, and produce fulness of habit. Similar statements are made by Carey (p. 74), Nassy (p. 18), Currie (p. 11), Condie and Folwell (*Essay on Fever of 1798*, Appendix, p. 5), and Barnwell (p. 374). Nor is this all. The fever, as already remarked while on the subject of acclimatization, has prevailed more among the natives of climates where the sanguine temperament, and the robust and plethoric constitutions are predominant; while individuals coming from regions where such conditions of system are unusual, are comparatively spared. Thus, in contradistinction to the fact that the inhabitants of the tropics, where the bilious and lymphatic temperaments are more frequently met with, escape the disease, Deveze states that Russians proved prone to the disease—the Germans and English scarcely less so; while the French, especially those from the southern provinces, where the West Indian temperament, as Savarésy remarks (p. 260), prevails, stand a better chance of escaping than either of the others (p. 108). The same observation as to the liability of strangers was made by Dr. Rush, and dwelt upon by the College of Physicians (p. 19). And if we turn to the accounts extant of the fever as it has appeared in other parts of this country,¹ as well as in Europe, we shall find that equal stress is laid on the greater liability of individuals of sanguine temperament and strong constitution, and of such as indulge in those habits of living which, as we have seen, predispose to an attack, and cause the disease to assume a more malignant and fatal character. At the same time, the liability of strangers in proportion as they arrive from regions where such peculiarities of system prevail; and *vice versâ*, the comparative immunity of southerners, and the

¹ Valentin, p. 90; Thomas, p. 77; Girardin, p. 12; Barton, p. 20; Bayley, pp. 88–9; Dickson, iii. 257; Gros, p. 7; Shecut, p. 109; Hogg, p. 413; Francis (in Perlee), p. 10; Townsend, p. 381; Ticknor, N. A., iii. 218, 219; Rept. of New Orl. 1839, p. 159; Waring, p. 60; Alex. Hosack, pp. 10, 11; Thomas, pp. 21, 73; Fev. of N. O. in 1839, p. 159; Palloni, pp. 9, 10; Sir J. Fellowes, pp. 59, 129; R. Jackson, p. 126; Berthe, p. 170; Louis, p. 360; Caisergues, pp. 191–2; Pariset, Fever of 1819, p. 35; Arejula, p. 325; Velasquez, in Pariset, p. 14; Copland, iii. 150; Gonzales, pp. 316–17; Drysdale, Mus., i. 39; Townsend, pp. 251–2; Cartwright, ix. 15; Potter, on Cont., p. 27; Taylor and Hansford, Med. Repos., iv. 205–6; Bancroft, pp. 189–90; Ramsay, Med. Repos., iv. 218; Simons, p. 13; Dickson, Med. and Phys. J., iii. 257; Archer, v. 61; Seaman, Fev. of N. Y. in 1795, pp. 6, 7 in Webster's Collection; E. H. Smith on same fever, *Ib.*, p. 79; Sir J. Fellowes, p. 219; Berthe, pp. 167–9; Caisergues, p. 201; Rept. of Acad. of Barcelona, pp. 23–4; Pariset, Rept. on same fever, pp. 455, 507.

usual, if not complete safety enjoyed by the natives of the tropics, who, for the most part, are differently constituted, have been equally noticed by the writers of our own country generally, and those who have investigated the fever in Spain and Italy; thus establishing one more proof of the identity of the fever of the West Indies and Europe with that of this country. These facts have already been dwelt upon while treating of the subject of acclimatization, but very naturally find a place under the present head, inasmuch as one of the principal effects of acclimatization—through means of which danger from the disease is greatly lessened, if not entirely averted—is the reduction of the temperament and constitutional habits of the individual subjected to the ordeal, to the standard, and the approximation of his organic condition to that of the natives themselves.¹

Sex.—It is a fact well known to medical observers, that among individuals of the female sex, the sanguine temperament and robust and plethoric constitution—which, as we have seen, are the most prone to the yellow fever—are less frequently encountered than among males. Whether, from this circumstance, as maintained by some observers,² or from females being less exposed to the predisposing and exciting causes of the disease,³ their more temperate habits and less exposure to the deleterious influence of night air, or from some peculiar influence exercised by the menstrual process, and uterine activity, as believed by Bally (p. 302), Copland (iii. 139), or from other causes of resistance, certain it is, that we find enough to convince us that females are less obnoxious to the impression of the poison than individuals of the other sex; and that when attacked they have the disease in a milder form. The epidemics of this city, from that of 1793 downwards, have all exhibited this comparative immunity of the female sex. Deveze long ago stated in reference to the former of these visitations, that the disease prevailed more among men than women; and that among the latter it assumed a milder and more tractable character (p. 54, 2d ed., p. 105, 3d ed.). Dr. Rush,⁴ in like manner, noticed the greater prevalence and severity of the disease among males during the epidemic in question. Similar was the result in 1797,⁵ and we have the authority of Dr. Caldwell for the fact that the same observation was made during the epidemic of 1805 (p. 78).

In other parts of the United States, the comparative immunity of females to which I have here called attention, has been found as common as it was here; for, if we except Drs. Thomas and Harrison, of New Orleans, the first of whom (pp. 21, 22) thinks the less liability of females has been greatly exaggerated; while the latter remarks that he cannot say he “has observed any difference as regards the sexes either as to the number or malignity of

¹ Girardin, p. 47; Osgood, p. 16; Trotter, i. 337–8; Lempriere, ii. 23; Kéraudren, pp. 24–5.

² Rochoux, p. 35; Desportes, i. 195; Moseley, p. 433.

³ Hunter, pp. 20, 21; Trotter, i. 347.

⁴ Rush, iii. 80; Carey, p. 74; Barnwell, p. 374; Condie and Folwell, *Fever of 1798*, Appendix, p. v.

⁵ Condie and Folwell on *Fever of 1798*, Appendix, p. v.

the cases (p. 130), I cannot recall the name of any competent authority who upholds the opinion of the equal liability of the two sexes. Valentin (p. 90) and Archer (v. 61), who saw the disease in Norfolk; Drysdale (i. 38) who describes it as it occurred in Baltimore in 1794; Cartwright (ix. 16), Merrill (ix. 246), Perlee (i. 10), Hogg (i. 413), who encountered it at Natchez; Townsend (p. 252), Alex. Hosack (p. 9), Waring (p. 60), S. Brown (p. 83), Simons (pp. 7, 14), and others,¹ who have communicated the results of their observations made in New York, Savannah, Boston, New Orleans, and Charleston; all these, I say, confirm what has been said in relation to Philadelphia.

In Europe, the fever has, in general, manifested the same predilection for the male sex as regards the extent of its prevalence, and more frequently in respect to the severity and fatality of the attack. On this subject, the writings of Berthe (p. 354), Arejula (pp. 182, 438), Sir J. Fellowes (pp. 120, 121), Caisergues (p. 190), Short (quoted by Fellowes, p. 303), Gonzales (p. 316), Pariset (p. 12), Louis (p. 261), Gillkrest (ii. 279), Palloni (p. 9), Pariset (*Rept.*, p. 454), Bally (p. 301), Rochoux (p. 121), and the *Rept.* of the Acad. of Barcelona (pp. 23, 49), are sufficiently explicit to justify the above conclusion. That exceptions have occasionally presented themselves it would be impossible to deny; nevertheless, they are of rare occurrence and do not invalidate the rule. The only one that needs to be mentioned is furnished by the epidemic of Xeres in 1811, in which females appear to have suffered in a special manner.² As to the vague assertions of Audouard (p. 30), and the insinuations of Rochoux (p. 121), respecting the fever of Barcelona, they are not entitled to our regard, as they are contradicted by the testimony of the native practitioner who signed the Report of the Academy of that city (*loc. cit.*); and because Rochoux, who alludes only to the admissions in the hospital under charge of the French commissioners, acknowledges that the mortality among males was much larger than that among females.

Nor is the comparative immunity of females from the yellow fever noticed in Philadelphia confirmed only by the results of observation in other parts of the United States and in Europe. A few instances of a contrary kind have been recorded in the accounts we have of the disease as it shows itself within the tropics, as at Barbadoes in 1816,³ at Antigua in the same year,⁴ at Martinique in 1838 and 1853,⁵ in Jamaica in 1819,⁶ and in Dominica in 1793.⁷

¹ See Bahier, Fortin, Daret, Sabin, Martin, in their Report on the Fever of N. O. in 1839, p. 139; Gros and Girardin, Fever of 1817, p. 7; Report to Med. Phys. Soc. of N. O. on Fever of 1820, p. 7; Dowler, Fever of 1853, p. 31.

² *Cycl.*, ii. 279.

³ Ferguson, *Med.-Ch. Rev.*, Jan. 1840, p. 304.

⁴ Musgrave, *Med.-Ch. Tr.*, ix. 106.

⁵ Chervin's Report on Dr. Catel's Mem., p. 10; Rufz, *Med. Exam.*, iii. 108; Chervin's Report on do., p. 32.

⁶ Arnold, p. 147.

⁷ J. Clark, p. 2; Chapin's Report of N. O. Sanit. Com. for 1853, p. 211; Report on Sickness, &c. of Army, p. 53.

But great as the extent and fatality of the disease may have been on those occasions among females, it is not the less positive that the testimony in favour of the immunity in question is almost universal among writers on the fever of the tropics. A reference to the works of Pouppé Desportes (i. 14, 195), Blane (*Seamen*, p. 405), Madrid (pt. i. p. 32), Moseley (p. 433), Pugnet (p. 347), H. McLean (p. 37), Chisholm (i. 140), Bally (pp. 269, 299), Savarésy (p. 264), Poissonnier (p. 55), R. Jackson (*Fev. of Jamaica*, p. 250), Trotter (i. 347), Humboldt (p. 775), Gillkrest (*Cycl.*, ii. 279), Ferguson (*Med.-Chir. Rev.*, Jan. 1840, p. 304), Hunter (p. 201), Arnold (p. 34), Caillot (pp. 15, 135), Dariste (p. 218), Rouvier (*Diss. sur le Fièvre Jaune qui a régné en l'an X. dans l'Ile de la Guadeloupe*, p. 15, quoted by Chervin, *Rept. of Rufz*, p. 32), &c. will satisfy any one on that head.¹

The reader needs scarcely to be reminded of the greater immunity of females from kindred forms of fever. In speaking of paludal fevers generally, Dr. Williams remarks that in the West Indies, in civil life, a woman is esteemed "twice as good a life as a man," and he adds that "in barracks the same difference of liability is observed between the sexes."² The same fact is pointed out by other writers in reference to all those fevers conjointly, as well as by those who have described the ordinary forms of the disease—intermittents and remittents—in France, Africa, Germany, and Italy. In 1,036 cases reported by Dr. Boling, of Alabama, there were 585 males and 451 females.³

Age.—Writers on the yellow fever have usually remarked that it affects in preference individuals of adult age—sparing, to a greater or less extent, young children as well as persons advanced in life. In regard to the disease as it appears within the tropics, this comparative immunity of the two extremes of age is fully recognized. Epidemics occur, it is true, in which this character is not observed, as was the case, for example, at Antigua in 1793, where, according to Dr. Byan, children were, if possible, more liable than adults;⁴ and at Martinique, in 1838 and 1852, when, Drs. Catel⁵ and Chapuis⁶ inform us, the disease attacked indiscriminately and with equal severity individuals of all ages. At Dominica, in 1793, according to Dr. J. Clark (p. 2); at Jamaica, in 1819, as stated by Major Tullock (p. 53) and Dr. Arnold (p. 147); and, again, at Antigua in 1853,⁷ similar facts were recorded. Such occurrences, however, are, as justly remarked by Dr. Chervin, comparatively rare. They constitute exceptions to a general rule, and are usually found in seasons when the infectious effluvia, whatever be their nature, are concentrated and uncommonly powerful; or when exposure to their influence is unusually great. As was mentioned in speaking of the comparative liability of the sexes, this ordinary exemption of the young and old may, in some measure, be accounted for by their less exposure to the action of the efficient cause. Never-

¹ Pinkard, ii. 476; Leblond, p. 95; Dickenson, pp. 13, 32; Henderson, p. 5; Williams, p. 51; Copland, iii. 139; Hume, p. 237; Report on Fever of Cayenne in 1850, p. 160.

² Morbid Poisons, ii. 456.

³ Tr. Am. Med. Assoc., v. 425.

⁴ Chisholm, i. 143.

⁵ Report by Chervin, p. 10.

⁶ Report of Sanit. Com. of N. O., 1853, p. 211.

⁷ Furlonge, Lond. Lancet for 1854, ii. 440, Am. ed.

theless, a review of all the facts we possess on the subject can leave no doubt on the mind as to a greater liability to infection of adults and individuals in the prime of life than of persons at any other age, as well as to the fact that if, during the course of some epidemics, or at the close of others, a larger proportion of children suffer, the result must be ascribed in part to the circumstance that the adult portion of those exposed are acclimatized¹—an advantage children do not enjoy; and also to the fact that the proportion of this class remaining unattacked, or liable to be infected (*Copland*, iii. 168) was, owing to peculiar causes, greatly enlarged. Be this as it may, if we lay aside these apparently exceptional cases, and examine the accounts of the yellow fever as it prevails in tropical regions generally, we shall find that though children and old people are often attacked, they enjoy, to a very great extent, the exemption to which I have alluded; and the disease among those attacked assumes a milder character, and gives rise to a smaller proportionate mortality.²

Similar has been the result of observations in Europe: "Subjects belonging to the two extremes of life," says M. Rochoux, "infancy and old age—appear to be but little liable to be attacked by the disease, judging at least from Barcelona, where a very limited number of old people were found to be affected—comparatively with subjects of middle age—and where a vast number (*foule innombrable*) of children survived their entire families." "It is," adds this author, "on persons between the ages of 30 and 40, that the epidemic bore with the greatest violence (p. 120). This statement, controverted though it is by another eye-witness of the same epidemic,³ is entitled to our confidence, coinciding as it does with what we have seen regarding the fever of hot climates, and from its being confirmed by the testimony of almost every other known writer on the yellow fever of Spain and Italy—Barcelona, Cadiz, Medina Sidonia, Seville, Gibraltar, and Leghorn.⁴

The yellow fever, in the various visitations which the city of Philadelphia has suffered from it, has exhibited the same disposition to affect most usually the age most prone to its baneful influence elsewhere. Deveze states that "the age of vigour is the most favourable to the yellow fever," and that "it prevails principally among individuals between (the age of) twenty and forty." "It generally," he adds, "respects old age, and seldom attacks children" (p.

¹ Catel, p. 10; Byan, in Chisholm, i. 143; Arnold, p. 147; Mortality of the British Army, p. 52.

² Chisholm, i. 148; Rochoux, p. 120; Pugnet, p. 348; Savarésy, pp. 264, 265; Caillot, pp. 15, 136; Bally, pp. 269, 296, 297, 298; Moseley, p. 438; H. McLean, p. 36; Madrid, pt. i. p. 32; Monson, p. 6; Dariste, p. 218; Arnold, p. 34; Dickinson, pp. 13, 82; Henderson, pp. 4, 5; Williams, p. 51; Pinkard, ii. 476; Hume, p. 238; Leblond, p. 96; Copland, iii. 139, 150; Blair, p. 50; Winterbottom, Med. and Phys. Journ., p. 7.

³ Audouard, p. 416.

⁴ Sir James Fellowes, pp. 58, 420; C. McLean, p. 15; Mem. of Acad. of Barcelona, pp. 23, 44, 49; Berthe, p. 170; Caisergues, pp. 191, 192; Louis, p. 260; Burnett, p. 493; Pariset, p. 454; Riseuno, quoted by Burnett, p. 242; Arejula, pp. 182, 183; Gonzales, p. 416; Palloni, pp. 9, 10; Pariset, Fever of Cadiz in 1819, p. 34; Velasquez in same, p. 14.

106). All ages, says Dr. Rush, were affected by this fever (1793), but persons between 14 and 40 years of age were most subject to it. Many old people had it, but it was not so fatal to them, as to robust persons in middle life. It affected children of all ages. I met with a violent case of the disorder in a child of four months, and a moderate case of it in a child of only ten weeks old. It had a deep yellow skin. Both these children recovered. It is but proper to remark that the same writer, with the view to show the proportion of children who suffered by this fever, states that of 75 persons who were buried in the graveyard of the Swedish church in the months of August, September, and October, 24 were children. "They were buried," he adds, "chiefly in September and October, months in which children generally enjoy good health in our city" (pp. 93, 94). Whether the whole number of these deaths is attributable to the yellow fever is a point which now cannot be satisfactorily ascertained. It is presumable that many of those children were cut off by other diseases—of the digestive and pulmonary organs—which, during healthy seasons, do not cease to cause a large mortality among individuals of that period of life. At the same time I do not wish to deny the agency of the fever in causing the mortality alluded to by Dr. Rush. In saying that children and old people suffered less than adults—a fact which the illustrious writer himself has recorded—I am far from meaning that they enjoyed complete immunity from the disease; and were not this implied admission of their proneness to an attack sufficient, we find in some of the accounts we possess of that epidemic, statements which go far to justify our placing many if not the greater number of the deaths alluded to, to the score of the reigning fever. We are told, for example, by Dr. Currie (p. 11), that neither age nor sex were exempt—not even infants at the breast—and Dr. Barnwell more pointedly states that "at the first appearance of the disease in August and September, while the warm weather lasted, the robust, middle-aged, and plethoric, had the worst chance; when, at the same time, the aged, or young children, and delicate women, generally were safest. But, as the weather began to grow cool, and the disease became milder, with the strong and the robust, and the middle-aged, it was worse upon the weakly and delicate children, and the aged—and, in many, very tedious and obstinate" (p. 374).

Be this as it may, it remains well established, from the admissions of most of the writers just mentioned, as well as from the statements of Carey (p. 74), and Condie and Folwell (Appendix, p. v.), that individuals in the prime of life were the principal sufferers during the epidemic of 1793. Such was the case also in 1797. (*Condie and Folwell, ib.*) We have the authority of Dr. Caldwell (p. 78) for a similar result in 1805; while Drs. Monges, Matthieu, and Rousseau, who saw much of the disease, testified, in documents they furnished to Dr. Chervin, to the fact that they had seldom known it to affect infants.¹ At the same time, it is impossible to conceal that children are sometimes affected. During the summer of the last year, several such instances

¹ Rapport de l'Académie de Médecine, p. 28.

fell under my observation, principally in one family. They occurred in the infected district, at a time when unmistakable cases of a malignant kind and fatal tendency were prevailing close by. They were of a mild character, marked by a single paroxysm of from forty to sixty hours' duration, and attended with intense head and backache, red eyes, and white tongue, and ended by profuse perspiration.

It may be proper, in conclusion, to remark that facts of an analogous character are recorded in almost every account handed down to us of the disease as it has prevailed, at various times, from Boston to New Orleans.¹ The ratio of mortality among children in 1853, at New Orleans, was very high. But it was mainly so in comparison with the results obtained in other years, especially in 1841. Dr. Dowler, who made an extensive analysis of the epidemic of that year, says: "I made thirty series, each consisting of thirty persons; I then took the youngest one in each series (among these, 990 dead), which gave these ages: 15, 17, 17, 2, 5, 20, 19, 16, 20, 17, 15, 17, 18, 19, 8, 2, 7, 18, 18, 19, 8, 6, 8, 2, 15, 3, 18, 14, 2, 18, 3, 5, 19. Scarcely an infant in the whole series" (pp. 31, 32).

In our southern cities, the mortality among the native children may, when compared with that of adults, appear large.² But, were it still more considerable than we know it to be, the fact would not militate against the greater immunity of the younger portion of the population, inasmuch as native children are not, like native adults, inured to the climate—they are not acclimatized—and must therefore be more prone to the disease than the former. It is only by contrasting the susceptibility of such children, conjointly with those who may have arrived from the northern latitudes, with the liability to the disease of unacclimatized adults, as also the mortality in both classes, that we can arrive at any satisfactory inference on the subject in question. Now, a reference to the writings of Dr. Simons and Professor Dickson³ will show that, so far as regards Charleston, the chances of escape are decidedly in favour of children. The former⁴ of these writers remarks that "the number of native children who die during the prevalence of fever is small, considering the great number liable to the disease, in proportion to strangers, and remarkably small in comparison with those who die of other diseases to which they are subject;" "and it cannot be doubted that many of the elder children have been exposed to the sun and other circumstances producing powerful exciting causes." Dr. Simons further remarks that, "where children have not been allowed to expose themselves much to the night air,

¹ Drysdale, i. 37; Valentin, p. 90; Thomas, pp. 73, 77; Harrison, N. O. Journ., Sept. 1845, p. 136; Barton, p. 20; Cartwright, ix. 16; Merrill, ix. 246; Townsend, p. 253; Gros, Rep. of Epid. of 1817, p. 7; Thomas, p. 21; Simons, pp. 7, 8, 11; Archer, v. 61; A. Hosack, p. 9; Waring, p. 60; Rep. of Med. and Phys. Soc. of New Orleans on Epid. of 1820, p. 7; also Epid. of 1839, p. 159; Shecut, pp. 108-9; Seaman, Webster Collection, p. 7; S. Brown, p. 83; Dickson, Ecl. Journ., iv. 112.

² Ramsay, Hist. of South Carolina, ii. 85; Dickson, iii. 257.

³ Ecl. Journ., iv. 112.

⁴ Report, p. 11.

when the dews are heavy, &c., they are more generally exempt, and, when taken with fever, it is commonly mild and manageable."

Race.—The yellow fever, which, as is universally acknowledged, must be viewed as one of the most formidable diseases to which the human frame is liable, spreads to all classes of individuals who are placed within the sphere of its operation and do not enjoy the privilege of exemption through the agency of those organic changes obtained from acclimatization. But, while such is the tendency of the disease to extend its ravages to all classes, the liability to the action of the causes from which it originates is not shared in equal degree by the several races of men. Experience everywhere teaches that the disease, without completely sparing, particularly under peculiar circumstances, the individuals of African birth or origin, whether in its sporadic or epidemic forms, affects more generally and severely the white race. A distinguished writer of the present day states that negroes born in Africa and carried to the West Indies have nothing to fear from the yellow fever, while those born in northern latitudes would probably be liable to its attacks if placed under like circumstances. On the other hand, he thinks that sufficient facts have been collected to show that no race is exempt from the fever of temperate regions, which extends its influence indiscriminately, with about equal violence, to all individuals.¹ In support of the first of these points, he appeals to the result of his personal experience; and, in relation to the second, makes erroneous references to the works of Drs. Rush and Jackson, of this city, and of his countryman, Dr. Valentin. The ensuing facts and remarks will, it is thought, demonstrate that on both these points the author is wrong; that the liability of the negro race to the yellow fever presents no greater difference in the two regions than might be anticipated from diversity of climate and other circumstances exercising a like influence on the white race; and that, so far as the fever of Philadelphia is concerned, the negroes, though, to a certain extent, obnoxious to the disease, are much less so than he represents them to be.

The exemption from yellow fever of negroes born and raised or acclimatized in countries where the disease is endemic—the West India Islands and the western coast of Mexico and South America—and especially of the natives of Africa, has been noticed and recorded by almost every writer. This immunity they possess in much greater perfection than the whites born, bred, or acclimatized in the same localities, and they hence may be supposed to owe a large share of it to the peculiarity of their organization. Few among them take the fever; and those affected have it generally, though not universally, in a mild form.²

¹ Rochoux, pp. 35, 121.

² Custin, in Duncan's Com., ix. 238; Warren, p. 14; Moseley, p. 146; Lempriere, ii. 29; Bancroft, pp. 51, 270–2; Jackson, Fevers of Jamaica, p. 250; Hunter, pp. 20, 308; Pugnet, pp. 346, 347; Savarésy, pp. 256–8; Frost, Med. Repos., xii. 223; Rufz, Med. Examiner, iii. 129; Ib., Chervin's Rep., p. 30; Ferguson's Med.-Chir. Trans., viii. 121; Ib., Med.-Chir. Rev., Jan. 1840, p. 300; Ib., Recol., p. 142; H. McLean, p. 187; Humboldt, p. 772; Hume, pp. 237, 238; Chisholm, i. 142, 225; Bourgeois, Maladies de

But, although creole negroes are thus shown to be generally placed beyond the reach of yellow fever, their exemption is not found to hold universally. Like all other rules, it is liable to exceptions; instances presenting themselves, particularly in times of violent and malignant epidemics, of negroes taking the disease, generally in a mild, but sometimes in a severe and even fatal form. In 1830, the yellow fever prevailed in Senegal. At Goree, in the peninsula of Cape Vert, and at St. Louis, it carried off a large number of blacks—nearly as many as whites.¹ The negro population of Boa Vista do not appear to have enjoyed an immunity. They were attacked in as large a proportion as others by the disease, though generally in a much milder form. There are not wanting facts to show, also, that negroes newly from the coast of Africa have at times suffered from the disease, and added their quota to the amount of the mortality.² Nor is this all. The acclimatization of negroes, like that of the whites, is occasionally lost by a prolonged residence in cold climates, so that, on their return to tropical regions, they become liable, though in a less degree, to the disease.³ Negroes from northern climates, though less susceptible to the disease than the unacclimatized whites, are nevertheless prone to its attacks, sometimes in its most aggravated form, when exposed to it in tropical regions.⁴

St. Domingue, in *Voy. Intéressants*, &c., p. 417; J. Clarke, p. 3; Caillot, p. 14; Monson, p. 5; Bally, pp. 269, 303–5; Arnold, p. 34; Dickinson, pp. 12, 48; Evans, p. 276; McWilliams, *Niger Exped.*, p. 128; *Ib.*, *Fever of Boa Vista*, pp. 93, 94; Doughty, p. 50; Wright, *Med. Facts*, &c., vii. 8; Henderson, p. 7; Bryson, p. 54; Pinkard, ii. 484; Copland, iii. 151; Leblond, pp. 18, 245; McCabe's *Rep. on Dis. of Warm Climates*, p. 43; *Dict. de Méd.*, xix. 151; Herrera, lib. iii. and x.; Barry, in Boyle, p. 270.

The greater power of the negro race to resist the action of the causes of yellow and other fevers is shown in the difference of mortality from these diseases among white and black troops in the British command of the West India station. While in Jamaica the annual loss among the former amounts to 102 per 1,000 of the mean strength, the deaths among the blacks did not exceed 8 per 1,000. In the Bahamas, the mortality of the whites was 59 in 1,000, that of the blacks 5.6 in 1,000; and in the windward and leeward commands the whites suffered at the rate of 36.9 in 1,000, the blacks at the rate of 4.6. The disease is milder; for while, in the windward and leeward commands, the admissions among the whites were 717 in 1,000, and the deaths 36.9, or 1 in 1.92, the admissions among the blacks were 168 in 1,000, and the deaths 4.6 in 1,000, or 1 in 3.66. In the Bahamas, the admissions among the whites were 506, and the deaths 85, or 1 in 5.9; among the blacks there were 2,260 admissions, and only 40 deaths, or 1 in 56. (*Statistical Reports.*)

¹ Thevenot, p. 254; Chevé, *Rélation des Epidémies de Fièvre Jaune au Sénégal*, pp. 17, 47. Thèse, 1836. See also J. Clark, *Fev. of Dom.*, p. 2.

² Blane, *Dis. of Seamen*, p. 405; Hunter, p. 20; Chisholm, *loc. cit.*; Ferguson, *Med -Chir. Trans.*, viii. 120–1; Curtin, *loc. cit.*; Savarésy, *loc. cit.*; Bancroft, p. 57; *Cycl. of Pract. Med.*, ii. 279; Imray, *Fev. of Dom.*, *Edinb. Med. and Surg. Journ.*, liii. 95; Stevens, pp. 195–201; Grieves, of Antigua, and Bow, of Barbadoes, quoted by Chervin, *Gaz. des Hôp.*, Oct., 1839; O'Halloran, *Rep. on Epid. of Jam.*, 1825–6; Bourgeois, *loc. cit.*; Furlong, p. 290; Rufz, *loc. cit.*; Evans, p. 276; Chevalier, p. 7.

³ Jackson, *Dis. of Jam.*, p. 250; H. McLean, p. 187; Lempriere, ii. 29; Bancroft, p. 196; Savarésy, *op. cit.*; Gillkrest, *Cycloped.*, ii. 279.

⁴ Frost, *Med. Rep.*, xii. 224, note; Ferguson, *Eclectic Journal*, iii. 14; Veitch, p. 112; Pinkard, ii. 480.

In Europe, the result has not been very different. The disease, in Spain, has attacked some men of colour (*Gillkrest*, ii. 279). But in Cadiz, they were seldom affected; and those that were, had the disease in a mild form. A similar observation was made at Carthagená and Leghorn.¹

Turn we now to the yellow fever of the United States, we shall discover that the negro, though prone to the disease, enjoys a degree of exemption far greater than that of the whites. In those of our southern cities, where, notwithstanding the length and heat of the summer, the white population does not enjoy, to the fullest extent, the benefit of acclimatization—Natchez, Norfolk, Washington (Miss.), &c.—the negro race is liable to the disease, but much less so than the corresponding class in the more northern States—and to a still less degree, both in point of frequency and violence, than the whites. On this head we have the support of high authorities,² and among them of Dr. Valentin himself, who, so far from lending support to the assertions of M. Rochoux, states that the disease prevails more severely among the whites than the blacks, and in a note adds: “Je n’ai vu que très peu de nègres en être atteints en Virginie (Norfolk), et partout ailleurs le nombre en a toujours été beaucoup moins considérable” (p. 90).

In other cities of the Union subject to yellow fever, where the advantages of acclimatization are enjoyed to a greater extent, the native negroes, or those inured by long residence to the climate, are usually, if not always, proof against the disease. Instances have no doubt occurred in which negroes, so inured, have taken the infection, and even perished³ at Charleston or elsewhere. But the general result of observations in that city from the days of Lining (p. 409), of Moultrie (p. 4), would lead to the conclusion that the number of these is very restricted. In the epidemic of 1838, the official report shows that among 538 interments of yellow fever subjects, only 7 were blacks, or about 1 in 50; and these, as Dr. Dowler remarks (p. 38), were probably, as usual, not city creoles. In Savannah, in 1854, the whole number of reported deaths from yellow fever amounted to 594; of these, 14 were blacks.⁴

There, as also in New Orleans, Savannah, Pensacola, and Mobile, the exemption is nearly as effectual as in the West Indies, if not equally so. Cases of infection among them are fewer than among the acclimatized whites, and occur principally during malignant epidemics; while those affected have the disease in a milder form than the other races.⁵ “It is a well established

¹ Berthe, p. 167; Caisergues, pp. 191, 200; Burnett, p. 242, who quotes Riseuno; Rappt. of Med. Acad. of Barcelona, p. 23; Pariset, Fever of Barcelona, p. 542.

² Cartwright, Recorder, ix. 15; Selden and Whitehead, Rep., iv. 335; Merrill, ix. 246; Archer, Recorder, v. 61; Monett, Am. J., i. 245; Hogg, West. J., i. 413, 416.

³ Valentin, p. 90; Dickson, Med. and Phys. J., iii. 252.

⁴ Report of John E. Ward, Mayor, &c., p. 28, &c.

⁵ Ramsay, ii. 85; Chalmer, i. 37; Simons, p. 14; Ib., Address, p. 12; Dickson, p. 345; Gros and Girardin's Rept., p. 7; Townsend, Fev. of N. Y., p. 249; Daniel, p. 65; Dowler, p. 38; Thomas, p. 77; Fenner, Fev. of 1853, p. 56; Seagrove, Register, iii. 420; Fev. of N. O. 1839, Rev. Med. Soc., 1840, pp. 159, 325; Cooke, N. O. J., x. 616; Lewis, Fev. of Mobile in 1843, N. O. J., i. 416; Bartlett, p. 345; Ticknor, N. A. J., iii. 218.

fact," Dr. Fenner remarks, "that there is something in the negro constitution which affords him protection against the worst effects of yellow fever; but what it is I am unable to say. During an epidemic he will take the fever almost, if not fully as readily as the white, but it will be altogether milder and less dangerous in its tendency. In short, it will correspond more exactly with the bilious remittent fever that prevails in the country, and requires precisely the same treatment. And yet this type of fever in the city negro must be produced by the very same cause that gives rise to malignant yellow fever in the white race. Occasionally, we see the hemorrhagic diathesis of yellow fever displayed in the negro, but it is by no means common. The least mixture of the *white race* with the *black* seems to increase the liability of the latter to the dangers of yellow fever; and the danger is in proportion to the amount of white blood in the mixture. Very few negroes ever die of yellow fever in this city" (p. 56). Dr. Dowler, in illustration of the insusceptibility of the black race, points to the year 1841: "Among 1800 deaths from yellow fever, there were but three deaths among the blacks—two having been children—or 1 in 600, or 1 in 14,000 of the whole black population." He corroborates the statement that, "although non-creolized negroes are not exempt from the yellow fever, yet they suffer little from it, and very rarely die" (p. 38).

In places of the kind referred to, while the acclimatized or creolized negroes are spared in the way mentioned, those from the Northern States are prone to the disease, though in a less degree than the unacclimatized whites; their susceptibility increasing in a ratio to the northernness of the place whence they come.¹ Even those who come from the country and are unaccustomed to the city, suffer, but mildly, from the disease.² Like the whites, they lose their acclimatization by a prolonged residence in the north of this country, and become liable to the disease on their return home.³ They likewise become prone, after long expatriation, to the same disease when exposed to it in the Middle States.⁴ Negroes from the West Indies remain exempt in the Southern States;⁵ as also the African blacks recently from their native soil.⁶

In the more northern parts of this country, the negroes—native of the soil, or inured to the climate from long residence—being, like the whites, deprived of the power of resistance imparted by the long-continued operation of atmospherical heat and habitual exposure to the causes of the disease, suffer like them from the yellow fever. In all the epidemics that have occurred in Baltimore, New York, &c., numerous cases of infection have occurred among the blacks; and of these some have proved fatal.⁷ But there, as else-

¹ Ramsay, Rep., vii. 244; Ib., Hist. ii. 85; Thomas, p. 78; Fev. of N. O. in 1819, p. 35; Fenner (1853), p. 56.

² Simons, Rep., p. 14; Waring, p. 59.

³ Daniel, p. 64; Thomas, p. 78.

⁴ Ibid., pp. 65, 109.

⁵ Ramsay, loc. cit., p. 244; Ib., Hist. of S. C., ii. 85; Thomas, p. 77; Barton, p. 20.

⁶ Daniel, p. 64; Waring, p. 59.

⁷ Drysdale, Med. Mus., i. 38; Townsend, Fev. of N. Y. in 1822, p. 255; Ramsay, of Edinb., Fev. of N. Y. in 1803, Ed. Med. and Surg. J., viii. 424.

where, the disease has not spread so generally among this class as among the whites; and, with few exceptions, the cases that occurred were of the milder kind.

It follows from all that precedes, that in all places, whether within the tropics or in temperate climates, in which the yellow fever has manifested itself—sporadically or epidemically—the negro race has manifested a greater or less susceptibility to the influence of those causes that give rise to the yellow fever; that everywhere, however, that susceptibility is far inferior to that exhibited by the white race—the disease in the former spreading less extensively, and assuming usually a milder and more tractable character; that the almost general exemption of the blacks in warm regions is due, in some measure, to their being acclimatized to the country—a circumstance they share with creoles, and those who are inured to the climate; and that their more frequent liability to the disease in colder than in warmer regions, is due to the same cause which renders the white inhabitants, whether natives or long residents, more prone to the disease than creoles—the want of acclimatization; that by losing, through means of expatriation, the power of resistance imparted by acclimatization, they are placed much on the same footing as negroes of temperate climates, being no longer as surely exempt as they were before from the disease when again they are exposed to its influence.

If, now, with these facts before us, we examine the result of observations made during the various epidemics which constitute the main object of our present inquiries, we shall find that so far as the degree of liability of the negro race is concerned, the yellow fever of Philadelphia differs in nothing from that of other temperate regions, and no more from that of tropical latitudes than might have been anticipated from the difference in the climate and the peculiarities of locality of the two regions. Here, as elsewhere, the negro is not exempt from the disease; but here, also, his susceptibility is very inferior to that of the white.¹ Of these facts, all our epidemics have afforded ample proofs. In all, blacks have been attacked, and in all, some have fallen victims to its malignancy; but, in general, they have furnished a less proportionate number of cases, and in those affected the disease has assumed a milder character. Mr. Carey, after quoting Lining, who denied the susceptibility of the negroes to the disease, says: “The same idea prevailed for a long time in Philadelphia, but it was erroneous. They did not escape the disorder; however, there were scarcely any of them seized at first, and the number that were finally affected was not great, and, as I am informed by an eminent doctor, ‘it yielded to the power of medicine in them more easily than in the whites’” (p. 85). Dr. Rush, likewise, made a similar observation. In the commencement of the epidemic of 1793, believing, from the statements of Lining, that the negroes of our city would escape the disease completely, he was instrumental in obtaining nurses for the sick from among that class. “It was not long,” he continues, “after these worthy Africans undertook the

¹ Rush, iii. 81; Caldwell, Med. and Phys. Mem., p. 10; Cathrall, p. 6; Currie, pp. 13, 14; Deveze, pp. 108–9; Folwell, p. 57; Carey, p. 85.

execution of these humane offers of services to the sick before I was convinced I had been mistaken. They took the disease in common with the white people, and many of them died with it. I think I observed the greatest number of them to sicken after the mornings and evenings became cool. A large number of them were my patients. The disease was lighter in them than in white people" (iii. 81). In 1794 the fever seems, according to the same writer, to have spread more generally among the blacks than it did before (iii. 202). As we have seen, in a former chapter, the mortality in 1793 amounted to about 3,500. In MS. notes by the late Dr. Currie, attached to a copy of his work on the epidemic of that year, now before me, we are informed that in the above number are included 305 negroes, according to the register kept by Richard Allen, an intelligent black man, who acted in the double capacity of physician to the sick and undertaker to the dead. The number of the blacks that died in the whole course of the year 1792 amounted only to 67. From this it appears that 237 more died of the yellow fever than died of all other diseases the preceding year.

But while the native blacks were susceptible to the disease—not enjoying, much more than the white population, the benefit of that organic process which enables the West Indian or the southerner to resist the action of the causes of infection; while they simply manifested, as do the unacclimatized negroes in tropical countries and our southern cities, a less aptitude to take the fever than the whites—the West Indian negroes, great numbers of whom found their way to Philadelphia at the time of the troubles at St. Domingo, escaped untouched throughout all our epidemics, though acting in the capacity of nurses and constantly exposing themselves to the causes of the fever. Neither Dr. Monges, Dr. Matthieu, nor my father, who, from 1793 downwards, practised much among that class, could record the occurrence of one solitary case of yellow fever among them. Dr. Deveze (p. 109) also notices their exemption.¹

¹ M. Rochoux, in his attempt to show that negroes do not enjoy a greater exemption from the yellow fever of temperate regions than the whites, and who adduces this supposed equal susceptibility as an argument in support of his favourite doctrine respecting the difference between that disease and the fever of the tropics, points out the example of the epidemic that occurred in this city in 1820, and refers to Dr. Samuel Jackson as saying that it commenced with the blacks. But every one who has read Dr. Jackson's excellent history of that epidemic must know that he has not made any statement of the kind, or said one word calculated to make one believe that such was his opinion. To any one less wedded to a favourite theory than M. Rochoux, the thing must be palpable; for Dr. J. merely states, in reference to the diseases which preceded the outbreak of the epidemic, that, "in the month of May, a fever of a bilious and remittent character, combined with typhoid symptoms, appeared among the blacks;" that "it continued to spread during the months of June and July, in the latter part of which month it attained its height, and was seen in its most aggravated forms;" that "it declined through the month of August, and terminated as an epidemic in September;" and that "it was so generally confined to the blacks (few whites being attacked), that it acquired the name of the negro fever" (p. 13).

But to mention that the yellow fever of 1820 was *preceded* by a disease of a bilious and remittent character, combined with typhoid symptoms, is very different from stating that the disease *commenced* among the blacks; which would be equivalent to representing the

As regards the cause to which individuals of the African race are indebted for their comparative immunity from the yellow fever, I cannot here inquire into at large. Partaking with the white race in the protective effects afforded by acclimatization, a large share of their power of resistance within the tropics, and in our southern States, must necessarily be ascribed to the organic constitutional changes induced by that process. For the superiority of exemption they manifest over the creolized whites; the less prevalence of the disease among them in climates where acclimatization does not extend, and the greater mildness of the disease among them everywhere, we are doubtless to look to their peculiar temperament, which, in general, is not of the kind to render them prone to the disease. I think I do not err in saying that examples of the sanguine temperament are not of frequent occurrence among negroes, and that more generally they are found to present the characteristics of the lymphatic; which, as we have seen, affords the best chance of escape. Much, also, is to be attributed to those physical peculiarities which nature has endowed them with, and which render them able to resist with impunity the action of what will be found to be one of the most prominent exciting causes of the disease—atmospheric heat. The negro was destined by nature to live under the vertical sun of the torrid zone, and, with a view to enable him to do so in safety and without physical suffering, he was clad with a black skin and furnished with woolly hair, which we all know are admirably adapted for resisting the morbid influence of the intense solar heat of that region, and causing him to feel cool to the touch of an European, even under circumstances when the latter would be overpowered, would sicken, and be literally scorched. By this means, aided by other peculiarities of constitution, which allow him to imbibe with impunity the usual cause of infection, the negro is enabled to enjoy a comparative exemption from fevers of all grades arising from malarial exhalations, not only in Africa, but in the West Indies and this country. This exemption is perhaps too well attested to require a long array of illustration in this place. It was noticed in the West Indies from the earliest

disease prevailing among them as being yellow fever. Dr. Jackson also records that the epidemic was preceded by searlatina, which continued as late as August (p. 12). Were M. R.'s conclusions correct, one might as well say that this last-mentioned disease formed part and parcel of the fever that followed. Dr. Jackson, as every physician who had occasion to watch the progress and phenomena of the fever in question, knew full well that it differed essentially from the yellow fever of that season. Of this every one may be satisfied who reads the descriptions of the disease for which we are indebted to him, as well as to Dr. Emerson* and the late Dr. Joseph Klapp,† of this city. He will therein find that its symptoms were different, that it originated and spread in a different part of the city, called for a different course of treatment, attacked a different class of individuals, proved less fatal, and reappeared the next year, when the city was perfectly free from the yellow fever.

* Acc. of an Epidemic Fever which prevailed among the Negroes of Philadelphia in the Year 1821, by G. Emerson, M. D., *Philad. Journ. of the Med. and Phys. Sci.*, iii. 193.

† Correspondence between Joseph Klapp, M. D., of Philadelphia, and J. B. Sutherland, Esq., concerning the Cases of Malignant Fever admitted into the Philadelphia Almshouse Infirmary in the Summer of 1820, *Med. Recorder*, iv. 80-90.

period of the introduction there of negroes. It did not escape the attention of Herrera (lib. 3 and 10) and others of the early historians, whose observations on the subject have been confirmed by every subsequent writer on the climate and diseases not only of those islands, but also of every other portion of tropical regions. "The negro," says Dr. Ferguson, "may also be said to be fever-proof; and the marshy savannas, which lie low and scattered and unventilated, prove to him the most healthful abode. From peculiarity of idiosyncrasy, he appears to be proof against endemic fevers. To him, marsh miasmata, which so infallibly destroy our white soldiers, are in fact no poison. The warm, moist, low, and leeward situations, where these pernicious exhalations are generated, prove to him congenial in every respect. He delights in them, for he there finds life and health, as much as his feelings are abhorrent to the currents of wind that sweep the mountain tops, where alone the whites find security against tropical diseases; but the black, when placed there, is almost infallibly struck with the bowel and heart complaints, which prove so fatal to him."

The comparative average prevalence of fever among the European and black troops of the British army in the West Indian and African commands, and the like mortality from the same disease, afford a striking illustration of the insusceptibility in question.

In all the localities from whence the returns are made—Jamaica, the Bahamas, Honduras, Windward and Leeward Islands, Mauritius, and Sierra Leone—we find the average number of fever cases among white troops far exceeding that among the blacks, while the mildness of the disease among these is evinced by the smallness of the mortality they suffered, compared to that among their Caucasian comrades. In speaking of the sickness of the British troops in the Sierra Leone command, Major Tullock says: "Fatal as the fevers of this colony have proved to the white troops, the blacks have been but little affected by them; indeed, the attacks have been fewer, and the deaths have not materially exceeded the proportion among an equal number of white troops in the United Kingdom or other temperate climates. Though fevers are much more frequent and fatal among the whites than in the West Indies, the reverse is the case with the blacks."¹ By another writer, Dr. Bryson, we are told (p. 22) that the natives of Fernando Po are a healthy, athletic race of people, yet this island is more detrimental to health than any spot in the known world; even the Africans from the continent are always sickly here. Of thirty white mechanics who arrived in November, 1827, all had the fever in a very short time; the number that died cannot be ascertained. A few were invalided, and five only remained in June, 1828 (pp. 22, 69, 70).² The reader is doubtless already conversant with the universal exemption of the Kroomen, not only from the common remittent of the coast, but from every deadly fever, as a circumstance rendering them of extreme utility to the coast squadrons, and to all traders. Moseley informs us, that

¹ Dis. and Mort. of the British Army (Western Africa), p. 16.

² See also Daniell's Topography of the Coast of Guinea, p. 134.

none of the Europeans sent in 1780 on the expedition against St. Juan "retained their health above sixteen days, and not more than three hundred ever returned, and those chiefly in a miserable condition. It was otherwise with the negroes who were employed on this occasion; a very few of them were ill, and the remainder of them returned to Jamaica in as good health as they went from it" (p. 163). The same thing occurred at the taking of Fort Omod from the Spaniards. "On that expedition, half the Europeans who landed died in six weeks. But very few negroes, and not one of two hundred that were African born." (*Ib.*, note.)

The negroes of our southern States are but little subject to malarial fevers; and while, on the rice plantations of Georgia and South Carolina, the white man cannot reside for fear of the country fever, the negro works with impunity, exposed to the rays of the broiling sun and to the humidity of the flooded fields.¹ Dr. Ketchum, in a report on the topography, sanitary condition, and vital statistics of Mobile, says of the slaves owned by the creole population: "They seem to be entirely exempt from the attacks of yellow and bilious fevers."² Another writer, Dr. Pendleton, of Georgia, remarks: "That the African is less susceptible to malarious influences than the white, I have believed from general observation heretofore. Although more exposed to the cold dews and hot sun of autumn, as well as having more filth about their habitations, they seem to be less liable to periodic fevers, and more readily recover than the white." In illustration of this, Dr. P. shows that, in the county he resides in, the number of idiopathic fevers among the whites greatly predominates over that among the blacks, being in the proportion of 14.5 to 10.4.³ After remarking that congestive fever prevailed epidemically in his neighbourhood, Dr. Lewis, of Mobile, says: "There were, in my professional circle, two blacks to one white; yet I did not see a single case of congestive fever in a negro, nor did I hear that any died of the disease in that section of country. I have made inquiries of several medical gentlemen who have long been practising in the country; their experience does not materially differ from mine. The fact is, the remarkable exemption from yellow fever which this race enjoys extends, in a great measure, to all the malarious fevers of hot climates; they may all have intermittent and light bilious fevers, as well as the milder grade of yellow fever, but it is only under extraordinary circumstances that these diseases affect them so seriously as to cause death." Dr. Lewis remarks that any one who weighs calmly all the influencing circumstances by which plantation negroes are beset—the vicissitudes of heat and cold to which they are hourly exposed, such as running from the fields during a shower of rain; sleeping in wet clothes on a cold bluff or earthen floor, from which they arise with a pain in the head, or cold, stiffened limbs; their inattention as to the preparation of their food, which they eat in a crude, half-cooked state—it will be found that these, and not malaria, are the

¹ Daniel, pp. 64, 65; Wood, i. 240–267.

² Fenner's Southern Med. Rep., ii. 307.

³ General Report on the Topography of Middle Georgia, Charleston Journ., vii. 455.

chief causes of the mixed, undefinable fevers, dysenteries, and diarrhœas that annoy them.

Constituted in this way, and shielded thereby in a great measure from the baneful action of those agents which give rise to febrile affections generally, they may easily be understood to be better able than the whites to ward off attacks of yellow fever, which, as we shall have occasion to see, there is every reason to believe owes its origin to causes of a kindred nature.

CHAPTER IV.

PERCEPTA—INGESTA—GESTA—APPLICATA.

PERCEPTA. *Passions and Emotions.*—The influence of the moral on the physical system of man is so well known and so fully appreciated, that to point it out in this place would be a useless occupation of time and space. Every one knows that, when carried to excess, the various passions and emotions of the mind, by which we are swayed, at times exercise a beneficial influence in the mitigation or cure of diseases, while at other periods and under different circumstances they give rise to effects of an opposite character. They modify the functions of animal and organic life—for good or for evil—and thereby manifest, among other tendencies, that of enhancing the disposition in the system to receive the impression of morbid causes, or of exciting an attack in those already under the influence of its agency. Without engaging here in an examination of the manner in which the passions exercise the influence in question, it will be sufficient to state that, while the effect of some is the result of an impression on the epigastric centre, paralyzing at the same time the nervous power,¹ while others occasion an increased activity in the cerebral organs, in the nervous system, and in the functions of the circulatory apparatus, others expend much of their action on the secretory organs of the abdominal cavity, impairing the digestive process and the powers of innervation. But, in whichever way the system may be affected, it would not be difficult, even did we not derive from experience the proof of the fact, to foresee the injurious effects adverted to.

Nicholas Massa, very many years ago, said: “Multi ex solo timore et imaginatione inciderunt in febrim pestilentialem.” Pigray denominates panic “pabulum et nutrimentum pestis.” Similar views have been entertained, respecting the injurious effects of that and other depressing passions and emotions, anxiety, grief and sorrow, as well as of anger and other exciting ones, as productive causes of oriental plague, by Diemberbroeck, Chicoyneau, Riverius, Hodges, Desgenettes, Larrey, Clot-Bey, and almost every writer on that disease. Equally explicit on the subject are Hoffman, Aretius, Cœlius

¹ Bichat, *Life and Death*, p. 56.

Aurelianus, Fazio, Senac, Falconer, Crogan, Home, Cullen, Lieutaud, Astruc, Huxham, and many others, so far as regards pestilential, typhus, nervous, or petechial fevers. In our days, Dr. R. Hamilton, of Edinburgh, has shown the influence of panic in propagating contagious fever, as exhibited in the Magdalen Asylum of that city, in the spring of 1821.¹ Lind, who, like Vandermere and many others since that time, had pointed out the baneful effects of mental depressions in the production of scurvy and other camp and ship diseases, and the benefit arising from cheerful and buoyant spirits, remarks, in respect to the influence of the former in the production of autumnal fever, that it is quicker and more violent in hot and insalubrious situations than in purer and cooler air. A fit of passion, he affirms, often brings on an instantaneous attack of fever; a violent fit of anger or grief will immediately produce a jaundice or the yellow fever; the sight of a corpse, or any object of horror, or even a shocking story told to a person, have been often known, through an impression of fear upon the mind, to bring on delirium, sometimes a violent vomiting and purging, which vomiting carried off the patient in twenty-four hours. In another work, the same author reiterates the sentiment, remarking that it is a received opinion that *fear* is a cause of itself sufficient to produce, in certain dispositions, a bad or malignant fever, there being many instances in besieged towns where no other reason could well be assigned for the rise of malignant disorders than the dejection of spirits, grief, and panic of the inhabitants, occasioned by the bombardment and the apprehensions of a violent death from some sudden assault of the enemy.²

I am not prepared to say, from personal observation, nor have I now leisure to examine, how far the opinion of Lind, as to the great injury arising from the action of the passions, under the circumstances mentioned, exclusive of some other agency, may be founded. Especially am I not prepared to admit, on so equivocal an authority as Lassis³—who, not content with discarding all idea of contagion, disbelieves the existence of infection and meteorological influences also—that nine-tenths of the mortality attributed to yellow and other fevers, should be ascribed to the effects of the fear and panic under which every one labours, to a greater or less extent, in times of epidemics. But there can be no doubt that such affections of the mind, as also most others which are incident to our nature, will be found to play an important part in the production and aggravation of the diseases of hot climates and of the summer season of temperate ones, whether in Hindostan, in Africa, in the West Indies, in this country, in Europe, or anywhere else. Upon this subject there can be no mistake. Facts and statements in relation to it will be found recorded in almost every publication extant on those diseases, and on none in a more striking manner than the one to which attention is more especially called on this occasion. Warren, after remarking that debauches are injurious, because the next day the spirits are prostrated, adds: “But,

¹ Med.-Chir. Trans. of Edinburgh, p. 296.

² Op. cit., p. 149.

³ Causes des Maladies Épidémiques, p. 88; Ib., Calamités résultant du Système de la Contagion, p. 9, &c.

even upon any dejections of mind, fright, and the like, particularly in the case of the smallpox, and this vulgarly imputed to the animal spirits retiring to the centre and gathering about the heart, and so leaving the outlets or avenues unguarded, let the reasoning be what it will, the observation is of good weight; and I have often had some grounds to think that it might take place in this malignant fever, too, having seen it lay hold on most of those who were under the greatest uneasiness and apprehension about it" (p. 25).

Desportes, also, speaking of the *Maladie de Siam* of St. Domingo (i. pp. 24, 196; ii. 264), long ago remarked that those who apply themselves too intensely to study, or to business, or give themselves up to sorrow, are the first attacked, and perish very rapidly. Bally, in our own times, remarked that the injurious effect of terror, in times of epidemics, is strikingly illustrated by the history of long sieges and defeats, and states that in the city of the Cape, in the island of St. Domingo, which then was ravaged by the fever in its worst form, the day after an alarm was sure to be marked by an increase in the number of cases (p. 366). In his account of the fever of Barbadoes, in 1817, Dr. Ralph mentions, in illustration of the injurious influence in question of anxiety of mind, the tendency which the fever showed to affect different classes of persons. "First, among the people of the huts it prevailed; then in the barracks. After we had lost one officer by fever, several others soon became affected; and, in like manner, when one of the hospital attendants had died, others soon fell sick, grew alarmed and died." "*Un caractère ferme* is," he remarks, the best preservative from fevers on all occasions. To this cause we may attribute, in some measure, the terrible mortality of the fever in Spain, "where fear, the handmaid of ignorance, reigns triumphant over the minds of the people."¹

Dr. Blair (p. 62) states it was discovered, during the progress of the epidemic of Demerara, which he has so well described, that if a sailor affected with the fever happened to be brought to the hospital when the hearse was present, the worst prognosis was to be formed. The intelligence of the arrival of the hearse had also the most injurious effect on the sick and convalescent within the wards. So much was this the case, that a new dead-house had to be built out of sight of the hospital, and the approach of the hearse so managed that its visits were unknown to the patients.

In another island, Dr. J. Clark informs us that fear produced the most striking and sudden effects in aiding the remote cause of fever; and the reader will find in the works of Chirac (i. 191), Hillary, (p. 146), Lempriere (ii. 10, 11), Chisholm (ii. 53), Ibid. (*Manuel*, p. 198), Hunter (p. 18), Johnson (on *Trop. Clim.*, p. 71), Clark (*Dis. of Long Voy.*, i. 165), Caillot (p. 138), Savarésy (p. 228), Gillespie (pp. 66, 132), and others mentioned below—Rochoux (pp. 24, 114), Rouppe (pp. 293, 296), Catel (pp. 17, 18), Chervin (*Rept. on Rufz*, p. 34), Osgood (p. 22), Ib. (*Cautions*, p. 13), Copland (iii. 151), Dariste (iv. 23, 39, 63, 64), Dutroulan (p. 15), Lefort (pp. 30, 31), R. Jackson (*Outlines*, p. 249), Gilbert (p. 71), Bancroft (p. 185), Mouillé (*Disser-*

¹ Med.-Chir. Trans. of Edin., ii. 63, 64.

tation sur le Fièvre Jaune Observée à St. Dom., p. 16), Chambolle (xii. 200), Repey (p. 16), McWilliams (p. 105), Lallemand (*Fever of Rio Janeiro*, p. 42), *Quelques mots sur l'Influence des Affections Morales sur les Marins* (p. 9), Mabit (p. 15), Arnold (p. 34), Lorein (p. 9), *Fièvre de Cayenne*, 1850 (p. 160), Bertullus (p. 19), Heastie (p. 59), Bourdon (p. 8), Hume (p. 236), Blair (p. 60)—that the effects of the depressing passions and emotions, fear, despondency, grief, inquietude, and even of indolence of mind, have long and continue still to elicit attention in regard to the fever of the tropics. So proverbial are the injurious effects of fear in the West Indies, that to this cause is attributed, in the French colonies, the great prevalence and mortality observed among emigrants from Provence—noted for their timorous disposition—though they are natives of a climate which, by its mildness, should render them less under the influence of that of the tropics than people from northern regions.¹

Not less evident have been the morbid influences of the depressing passions in the yellow fever of temperate regions—both on the other and this side of the Atlantic. Dr. Amiel, in his official answers to queries relative to the epidemic of Gibraltar, places *Fear* far above all other predisposing causes in its injurious tendencies; “and above all, the painful apprehension of soon becoming a victim to a scourge which every day cuts off so many people—an apprehension which staggers the most resolute—has increased the malignity of the fever, and rendered it pernicious to the highest degree.”²

Rochoux (p. 114), Pariset (p. 506), Blair (p. 21), Berthe (p. 147), Audouard (p. 435), in Europe, have noted results of an analogous character;³ and long before any recorded outbreak of the yellow fever in Spain or Italy; Chirac, in his highly interesting account of the malignant fever which desolated the city of Rocheford in 1694, and bore a strong resemblance to the former disease, has enlarged on the injurious effects of the depressing passions; and attributes the smaller mortality which occurred there, compared

¹ Chervin's Report on Rufz, p. 36.

² Johnson, *Trop. Clim.*, pp. 271–2.

³ “Dr. Broussonet, professor in the medical school of Montpellier, communicated to me the following fact: When he was sent to Spain, by order of the government, to investigate the fever which was then prevailing at Cadiz, he learned that the disease having broken out in Malaga, the preservation of that city from an extensive prevalence of it was due to the measures adopted by the captain-general commanding the province. Fully convinced of the fact that fear is one of the most fruitful causes of epidemics, he called a meeting of all the physicians and surgeons of the city, and after demonstrating to them the necessity of concealing from the public the knowledge of the existence of the yellow fever, he caused them to sign a declaration to the effect that the fever then prevailing bore no resemblance to that of Cadiz, and was not contagious. He forwarded this document to Madrid, and obtained permission to dispense with the establishment of a cordon. Giving the example to all around, he visited the sick and frequented the hospitals. He consoled the sick and assured them that the disease under which they laboured in no way resembled that which committed such ravages in Cadiz. At the same time he gave all the necessary orders for the cleansing and purification of the city. His measures had the desired effect. The number of cases was not large; the disease was less fatal, and the epidemic ended much sooner.” (*Dariste*, pp. 63, 64.)

to that of Marseilles in 1720, to the care he took to remove from the minds of the people the idea of the contagiousness of the disease—thereby depriving the latter of the frightful aspect it would otherwise have assumed.¹ On this side of the Atlantic, similar observations have been made by Moultrie (p. 26), Archer (*Med. Rec.*, v. 66), Drysdale (*Med. Mus.*, i. 34), Perlee (*Phil. Med. and Phys. Journ.*, iii. 10), Tooley (p. 16), Barton (p. 20), Gros (pp. 7, 8), Lewis (*N. O. Journ.*, i. 415), and the several reporters on the fever of Baltimore in 1819. By the writers on the fever of this city greater stress seems to have been laid on the subject than by those of other parts of this continent.² Barnwell speaks of the danger of allowing a patient to be swayed by a feeling of fear.³ The state of the mind, according to Currie (p. 3), had great influence in hastening or retarding the effects of the contagion. “Those under the influence of fear, which is the case with the majority, were sooner affected after exposure to the contagion, than those who were less concerned.” Dr. Rush⁴ says that in many people the disease was excited by a sudden paroxysm of fear.

It is to be remarked that, differing from most writers on the subject, the distinguished physician just named was disposed to think that a moderate degree of fear served to counteract the excessive stimulus of the miasmata, and thereby to preserve the body in a state of healthy equilibrium. He was certain that fear did no harm after the disease was formed, in those cases where great morbid excess of action had taken place. “The fear,” he adds, “co-operated with some of my remedies in reducing the morbid excitement of the arterial system” (p. 49). Whether the reader will feel disposed to attach much weight to an opinion based, in great measure, on a more than doubtful theory respecting the mode of action of the remote cause of the disease, I leave him to decide. From what has been said, I am, myself, inclined to regard the views of Dr. Rush on the subject as more fanciful than well founded, and to adopt the sentiments of his distinguished contemporary, Deveze, who, after pointing out the highly injurious tendencies of the passion in question says: “Fear is the more terrible, as it does not abandon the patient during the course of the disease, and thereby prevents his resisting its effects. Or, if it disappears, it is only to leave in its place a deceiving security” (p. 113).

Dr. Rush (iii. 202) speaks in decided terms of the effects of terror, and on the subject of grief, makes the following remarks: “It was remarkable that the disease was not excited in many cases in the attendants upon the sick, while there was a hope of their recovery. The grief which followed the extinction of hope, by death, frequently produced it within a day or two afterwards, and that not in one person only, but often in most of the relations of the deceased. But the disease was also produced by a change in the state of the mind directly the opposite to that which has been mentioned. Many persons that attended patients, who recovered, were seized with the disease

¹ Op. cit., i. 186, &c.

³ Fever of 1793, p. 10.

² Op. cit., p. 373.

⁴ Med. Inq., iii. 49.

a day or two after they were relieved from the toil and anxiety of nursing. The collapse of the mind from the abstraction of the stimulus of hope and desire, by their ample gratification, probably produced that debility and loss of the equilibrium of the system which favoured the activity of the miasmata" (iii. 50).

Excessive joy, fits of anger, by stimulating the action of the heart and arteries, as well as the nervous system generally; strong emotions of any kind; despondency from pecuniary or other losses; disappointment from even trivial causes; intense mental application, are no less to be deprecated, though producing their effects in a different way; while the feelings of hope, courage, cheerfulness, as well as equanimity of temper, have the contrary tendency of shielding the system from the morbid influence of the efficient cause of the disease, both by promoting the healthful play of the functions, and placing the happy possessor of them beyond the reach of the depressing passions. They prove powerful adjuvants of treatment. All the above-mentioned affections of the mind act, under the circumstances, only as predisposing or exciting causes; but are not, for that, less to be dreaded.

"Were I," says Dr. Rush, "to enter a city, and meet its inhabitants under the first impressions of terror and distress from its appearance, my advice to them should be, BEWARE—not of contagion, for the yellow fever of our country is not contagious—nor of putrid exhalations, where the duties of humanity or consanguinity require your attendance, but BEWARE OF EXCITING CAUSES."¹

Sleep.—Dr. Rush enumerates sleep among the causes of the disease, and says: "A great proportion of all who were affected by this fever were attacked in the night. Sleep induced direct debility (which he afterwards called debility by abstraction), and thereby disposed the contagion which floated in the blood to act with such force upon the system as to destroy its equilibrium, and thus to excite a fever. The influence of sleep as a predisposing and exciting cause was often assisted by the want of bedclothes suited to the midnight or morning coolness of the air (p. 35). Dr. Deveze, on the other hand, regards the idea of sleep exercising a positive morbid agency as one of a singular character. "It may," he says, "appear specious, because it is truly during or after the sleep of night that most individuals are seized with the disease." But he discovers in that circumstance nothing to justify the opinion that sleep has excited the disease; and thinks it more probable that the phenomenon in question must be attributed to the fact that the fever breaks out during the night, in the same way as other diseases appear more generally during the morning or evening (pp. 114, 115).

Without stopping to inquire how far the views of Dr. Rush, as to the mode of operation of sleep in exciting the disease, are entitled to our regard; and without doing more than pointing out that the explanation offered by Dr. Deveze amounts merely to the statement of the fact that the fever attacks at night because it is in its nature to do so, I am disposed to think that the

¹ On the Means of Preventing Summer and Autumnal Diseases, Med. Inq., iv. 126.

opinion of the former as to the agency of sleep in predisposing to or exciting the disease is far from being as singular and unfounded as the other regards it. The medical reader need scarcely be reminded that during the state of sleep certain changes take place in the system which may render the latter more prone to be acted upon by the efficient cause of the yellow fever.

By a reference to some of the older and many later and contemporary writers, from Lancisi to the present day, we shall find statements of facts observed in Europe, Asia, Africa, and America, which confirm the views of our eminent countryman. Speaking of the remittent and intermittent fevers of Peru, Ulloa long ago remarked: "*Quand ces fièvres régnant dans les Quebradas (or deep valleys) il suffit d'y séjourner pour en être pris; qu'on y dorme de nuit ou de jour, on ne les évite pas.*"¹ Nor can the effect be matter of astonishment. Every one knows that certain functions are modified in a more or less marked degree by the state of sleep. Dr. Edwards has shown,² and the same observation was made before by Sanctorius and Keill, that the state in question has a tendency to increase the perspirable process. At the same time, the nervous energy is diminished, the circulation is less rapid, calorification is lessened, other functions are performed with less energy, and the whole system is in a state of relaxation. In that condition we are more prone to take cold than we would be in the waking state; and the same individual who exposes himself with impunity while awake to a cold atmosphere and to currents of air, experiences injurious effects from these when asleep. Such being the case, we cannot but regard it as reasonable to conclude, that the state of sleep may place the system in a condition calculated to impair greatly the elimination of the noxious agents which give rise to the disease, and diminish the power of resisting their injurious effects. Should this be admitted, we derive a proof of the propriety of regarding the opinion of Dr. Rush, on the subject in question, as worthy of serious consideration. Nor is this opinion of the injurious tendency of sleep supported by the above consideration only; for, on examination, it will be found that other writers besides Dr. Rush have entertained the same views, and that in other fevers of a kindred nature a similar agency has, with great show of reason, been ascribed to sleep. Dr. Bancroft (pp. 86, 87, 100) mentions several facts from Clark and Trotter to show this agency, and says: "We have, therefore, reason to believe not only that the morbid miasmata are condensed or precipitated with the falling dews by the diminished temperature of the night, and thus accumulated near the surface of the earth, but that the body is rendered more accessible to their noxious influence during sleep by its greater relaxation, and by a suspension of those protective exertions of the living power which accompany our wakeful exertions" (p. 172). Lind, both in his work on hot climates (p. 182), and in that on seamen (p. 78), cites cases which go to prove the reality of that

¹ Ulloa, Mémoires Philosophiques Hist. Physiques concernant la Découverte de l'Amérique, i. 245.

² On Physical Agents, p. 102, Am. ed.

agency. In the latter, he remarks: "One great means, then, of the safety of the men which are employed on shore would be to relieve them often, and to permit none to sleep in the tents. Sentinels should be placed with a midshipman at watering-places, and strictly charged to prohibit sleep; for in sleep (a state of general relaxation) there is the greatest danger from the unwholesome air. This is a thing so well known at Rome, that, of its many inhabitants, there is scarce to be found one of the better sort, who, during summer or autumn, would venture to sleep a night at Ostia, or in the neighbourhood of the marshes adjoining the city. Persons often recreate in the day, and hunt in the unhealthy parts of the Campagna, but they are sure to return to the city before night; the fatal experience of many having sufficiently taught them the danger of sleeping in those nurseries of disease." (*On Seamen*, p. 75.)

In speaking of miasma, a writer of our own city says: "Exposure in the middle of the night is equally dangerous, and especially during sleep, when the power of resisting noxious agents is diminished. Hence," he continues, "the peculiar danger of sleeping in tents in sickly regions."¹ Turn we to the fever of the African coast?²—we find that there the injurious effects of sleep are recognized; and a reference to the writers on the paludal fevers of Europe will show that these effects are there fully admitted. Monfalcon, in his excellent treatise on marshes, remarks that the action of these is aggravated by the state of sleep which operates by favouring the absorption of effluvia. The same opinion is entertained by Alibert.³ As regards the fever of the Pontine Marshes, Dr. Clark⁴ states that among the inhabitants of Rome who are more prudent, *who never sleep* in the Campagna, but return to the city during the night, the fever is comparatively rare. Eustace, in his classical tour (iii. 148), adverts to the extreme danger of sleeping while passing the marshes, and advises travellers to keep awake during the journey across (p. 149). Folchi, the late able Professor of Materia Medica in the Sapienza of Rome, dwells on the same results.⁵ Brochi affirms the greater disposition to receive the infection during sleep as a fact placed beyond the possibility of doubt, and endeavours to establish on it his theory of the poison being admitted into the system through the pores of the skin.⁶ The writer of an excellent article in the *Edinburgh Review*,⁷ in commenting on the views of Brocchi, is equally positive as to the reality of the fact in question. Nor is the recognition of that fact of recent date. It was dwelt upon long ago by Lancisi, who devotes a separate chapter in his celebrated work on paludal effluvia, to explain the cause. "*Cur juxta palludas noctu præsertim indormientes magis quàm vigilantes lædantur?* Nemo arbitrator de facti veritate dubitabit, qui diu medicæ arti operam dederit." "Nos certè Romana Nosocomia per æstatem et autumnum plena videmus miseris

¹ Wood, i. 44.

² Bryson, p. 218.

³ Hist. Méd. des Marais, pp. 195–6; Fièvre Pernic., p. 235.

⁴ Med. Notes on Italy, p. 80.

⁵ On the Origin of Periodical Fevers in Rome, N. A. J., vii. 258.

⁶ Dello Stato Fisico del Suolo di Roma, &c. Rome, 1820.

⁷ Vol. xxxvi. 546.

agrorum colonis; ac per urbem sæpe dolemus incautos venatores, ac peregrinos, quamquam non longo tempore palustria loca incoluerint; quia tamen *brevem somnium* prope lacunas cœperunt malignis febribus affligari.”¹

But whilst sleep, under certain contingencies, must be viewed in the light of a predisposing or exciting agent of the disease, the deprivation of it—watchfulness, from any cause—gives rise to the same effect. Hence the injurious influence of nursing the sick, which, when added to anxiety of mind, so often attendant on fatigue of body so inseparable from that occupation, is but too often followed by an attack of the fever. To the same cause we may in some measure ascribe the greater prevalence of the disease among seamen and soldiers.

INGESTA.—Intemperance in respect both to food and drink; the use, especially when carried to excess, of aliments of an exciting and nutritious, as well as those of a crude and indigestible character, unripe or acid fruit; perhaps still more particularly the use of ardent spirits, and, indeed, of stimulating liquors of any kind, have almost invariably exhibited a tendency to excite the development of the yellow fever. The danger arising from such indulgences has been fully recognized from the earliest period, and is recorded by almost every writer, ancient and modern, who has treated of the causes of the disease. As regards the fever of tropical climates—Warren (p. 20), Moseley (pp. 86, 432), Hume (p. 237), Lind (p. 176), Hunter (p. 17), Hillary (p. 146), Poissonnier (p. 7), and a host of others,² have dwelt on the subject in a more or less pointed manner; and the descriptions we have of the fevers of the African coast³ and of India⁴—differing, in some measure, as these may do, from the disease more particularly before us—show that the baneful effects resulting in them from the cause under consideration, are similar to those observed in the yellow fever of the West Indies. Nor have these injurious effects been less clearly marked, so far as relates to the yellow fever of temperate climates and to diseases of a kindred nature. Berthe (p. 170),

¹ De Noxiis Paludium Effluviis, pp. 62–3; see also Blane on Dis. of Seamen, p. 230; Celle, Hygiène des Pays Chauds, pp. 298–359, 360; Wallace, Edinb. J., xlv. 273, 282; Balme de la Contagion, p. 227; Watson, Pract. of Med., p. 450; Simons, p. 18; Giannini, i. 228.

² Gillespie, p. 62; R. Jackson, Fev. of Jamaica, p. 398; Outline, p. 249; Hist. and Cure of Fever, pt. i. ch. 2, p. 66; Gilbert, p. 81; Chisholm, i. 303; ii. 154; Manuel, pp. 174, 198; Gillespie, iii. 62; H. McLean, p. 15; Moreau de Jonnes, p. 99, 253; Fever of Cayenne, 1850, pp. 161; Leblond, p. 96; Arnold, p. 28; Fontana, p. 7; Trotter, i. 342, 343; Bancroft, p. 29; Lempriere, ii. 10, 11, 15; Savarésy, pp. 230, 231, 234, 235; Osgood, J., p. 22; Arnold, pp. 28, 34; Heastie, p. 59; Vincent, p. 8; Henderson, pp. 4, 14; Dickenson, pp. 8, 39, 84, 87; Williamson, i. 39; Dariste, pp. 20, 40, 216; Blane, Diseases of Seamen, p. 84; Ralph, ii. 77; Dickson (D. J. H.), Edin. Journ., iv. 456; xiii. 37; Rufz, p. 36; Bally, p. 376; Caillot, p. 136; Ferguson, viii. 148; Blair, p. 60; Good, ii. 198; Pugnet, p. 335; Rochoux, p. 26; Boyle, p. 126; Celle, pp. 235, 241; Gonzales, Tratado de las Enfermedade de la gente de blar., p. 309; Lallemand, Fever of Rio Janeiro, p. 42.

³ Bryson, Edin. Journ., lxi. 115; Burnett, Fever of the Eclair, Edin. Journ., lxxiii. 494.

⁴ Johnson on Tropical Climates, pp. 10, 75, 350.

Pariset (p. 504), Burnett (pp. 17, 142, 172), Audouard (p. 439), Gillkrest (*Cyclop.*, p. 279), Boyle (*Edin. Journ.*, p. 8), Rochoux (p. 115), Palloni (p. 10)—have pointed them out as respects the fever of Europe; while, in this country, from Louisiana to Boston, the same result has been found to obtain.¹ Almost all the first victims of the yellow fever of Baltimore in 1794 were persons habituated to the immoderate use of ardent spirits; “and it is a melancholy truth,” says Dr. Drysdale, “that very few of those unfortunate creatures could be rescued from death by all the powers of medicine” (i. 30).

“I have received, on application,” says Dr. Barton, “an official document from G. W. Powell, Esq., G. W. P. of the Sons of Temperance in this city (N. O.), exhibiting the mortality among the members of that body during the last two most fatal years known to this city, when it has been scourged by the combined influence of yellow fever and cholera with an acknowledged mortality from them, during the period which this document embraces, of 5,653. Of the 2,427 members, there have died in two years, to the 1st of April (that is, from their commencement to that date) but 29—or 1 in 83.41! This most valuable report goes on further to state in detail, that of this mortality, 8 died of yellow fever, and two only of cholera—that about one-third of these deaths were of transient brethren visiting here for their health! And, moreover, that nine-tenths of all these members were of that *very age* (between 20 and 40), most subject to the supposed malign influences of this climate.”²

As might be anticipated, from what precedes, in so far as regards the disease as it has at various periods manifested itself in Philadelphia, the agency of such causes has proved most evidently hurtful. Dr. Rush dwells on the subject with characteristic emphasis, in his history of the epidemic of 1793. “A plentiful meal and a few glasses of wine seldom failed of exciting the fever. But when the body was strongly impregnated with the contagion, even the smallest deviation from the customary stimulus of diet, in respect to quality or quantity, roused the contagion into action. A supper of twelve oysters in one, and only three in another of my patients produced the disease. A half an ounce of meat rendered the contagion active in a lady who had lived by my advice for two weeks upon milk and vegetables. A supper of salad, dressed after the French fashion, excited it in one of Dr. Mease’s patients” (iii. 49). The College of Physicians, in their address to the citizens, 26th August, 1793, recommended temperance as a prophylactic means—fully aware of the danger attending a contrary course (*Carey*, p. 22). Deveze states, that men addicted to the pleasures of the table were most liable

¹ Lining, ii. 427; Addoms, p. 29; Selden and Whitehead, *Med. Reposit.*, iv. 334, 335; Tytler, p. 508; Archer, p. 66; Brown, *Fever of Boston of 1798*, p. 74; *Ibid.*, *Med. Reposit.*, ii. 297; Valentin, p. 90; Drysdale, i. 30; Porter, p. 10; Tooley, p. 16; Barton, p. 20; Cartwright, ix. 15, 17; Gros, p. 8; *Epid. of N. O.*, in 1839, p. 324; Dickson, iii. 257; Wood, i. 307; Ticknor, iii. 218; Forry, pp. 221, 222; Seaman, *Webster’s Collection*, p. 40; E. H. Smith, *ib.*, p. 84.

² E. H. Barton, *Sanitary Report of N. O.*, *Trans. of Am. Med. Assoc.*, iii. 605.

to the disease (p. 114), and Nassy speaks of the fatal tendency of a heating regimen, which, he remarks, "gave additional fire to the disease in those attacked, and thus dug, as it were, their own graves" (p. 9). Carey (p. 74), states that, "to tipplers and drunkards and to men who lived high, and were of a corpulent habit of body, this disorder (in 1793) was very fatal." Of these, many were seized, and the recoveries were very rare; and another writer, speaking of the epidemic of 1798, says that, "of hard drinkers who were attacked, not one in twenty recovered."¹

Dr. Rush further remarks (iii. 202), in his account of the fever of 1794, that "the summer fruits, from being eaten before they were ripe, or in too large a quantity, became frequently exciting causes of this fever. It was awakened in one of my patients by a supper of peaches and milk. Cucumbers, in several instances, gave vigour to the miasmata which had been previously received into the system." In fine, excesses in alimentary stimulation, the use of indigestible food, over repletion of even healthful articles, a resort to alcoholic liquors or strong wines, and in general any irregularity of regimen, have a tendency to predispose the system to the morbid impression of the efficient cause of the disease, or to excite the latter in those already predisposed. It may have been perceived that the baneful effects in question manifest themselves in those in whom such indulgences are of only occasional occurrence as well as in individuals who are habitually intemperate. But though the disease is excited in persons of regular habits, by an occasional excess or an imprudent use of improper articles of diet, the danger of an attack is generally greater, and the risk of an unfavourable issue much more to be feared in those in whom such excesses are habitual.

But while such is habitually the case, instances are found, and not unfrequently, too, in which a contrary result is obtained, habitual drunkards escaping the disease, and experiencing no or little ill effects from their intemperance; while individuals avoiding all excesses, and observing a temperate mode of living, fall victims to the disease. This it is that has led some writers of our own country to affirm that too much stress has been laid on intemperance and high living as causes of the fever.² It is not unreasonable, however, to conclude—such is the weight of authority in support of the contrary opinion—that, if individuals of intemperate habits escape, while others are attacked and perish, the exemption of the former must be viewed in the light of exceptions, not the rule. I am inclined to believe that the greater number of exemptions in question are found to occur in countries where the benefit of acclimatization is obtained, and that the escape of such individuals is due to that cause, or to the protective influence of an attack which had occurred prior to their having acquired the habit of indulgence referred to. Let unacclimatized and unprotected individuals, while indulging habitually in the intemperate use of intoxicating liquors or of stimulating and highly nutritious food, expose themselves in such climates during the sickly period, and

¹ Condie and Folwell, Appendix, p. xii.

² Dickson, *Eclectic Journ. of Med.*, iv. 113.

let persons of temperate habits, newly arrived there, commit excesses of any kind, and their chances of escape will indubitably be far inferior to that of those following a different and more prudent course. Nevertheless, although such is the view I am disposed to take of the subject, it is not to be denied that, under very different circumstances, and in cases of equal exposure and equal want of protection, individuals accustomed to indulgences of the kind mentioned escape with more certainty than those who commit excesses only occasionally—the former deriving their exemption from the power of habit, acquired by the repeated use of articles of a stimulating character, and which render these comparatively less hurtful than they prove to those unaccustomed to them. “It has been observed,” says Dr. McArthur, “and very frequently urged by the *bon vivant* as an excuse for his mode of life, that men who live in the most temperate manner are as liable to fever, if not more so, than those who follow the opposite extreme. There is an appearance of truth in this remark. Often, very often, the temperate and sober are seized with this fever under circumstances where the drunkard escapes. A stranger, on his arrival in the country, unless possessed of more than ordinary resolution, is assailed by so many temptations that he has not the power to follow the plan he may have laid down for his own regulation. He commits an occasional excess, and next morning awakes in a high fever; while the man accustomed to his *mosquito dose* probably feels no uneasiness, or, if he has any, a slight headache, from his last night’s debauch, and flies for relief to his hot punch and sangaree. The more temperate or regular a man has lived, any deviation will become, in a proportionate degree, a strong or exciting cause of fever. But if the drunkard and the sober man shall be attacked with fever, the former has not an equal chance of recovery with the latter.”¹

In this country, where, as we have seen, acclimatization does not obtain, cases of exemption of persons addicted to indulgences of the sort can only be found among those who have passed through the disease at some former period; and, from all the information I have been able to collect on the subject, and the observations I have myself made, I am disposed to conclude that all others are more liable to an attack, and, when so attacked, have a slender chance of recovery.

But while such is the danger arising from the occasional, and especially the habitual, use of stimulating food and spirituous liquors, it is certain that a contrary course—a sudden change from a generous to an abstemious diet—produces an injurious effect, and brings on an attack of the disease in individuals who otherwise might, in all probability, have escaped. Currie (p. 10) and others have noted the circumstance, of the truth of which I have no doubt. Even free livers and free drinkers experience an injurious effect from a sudden adoption of a temperate regimen. But on this point, and on the question as to whether a generous mode of living short of intemperance, as affirmed by Dr. Musgrave,² or a strictly temperate or abstemious one, had best be pursued, I shall have occasion to dwell in a subsequent chapter, and

¹ Johnson on Tropical Climates, p. 350.

² Med.-Chir. Trans., ix. 105.

need not enlarge upon it at present. Suffice it to remark, in conclusion, that, whatever view we may entertain on this subject, the injurious agency of stimulating aliments and drinks presents nothing more than might have been anticipated from their known action on the system. Producing over-excitement in the latter, they exercise, when transient, a local impression on the digestive organs, which they surcharge, fatigue, and stimulate¹ to a degree which would be incompatible, under all circumstances, with a healthy state of the functions, but which becomes doubly injurious when these organs—the stomach particularly—are labouring under a state of increased excitability or irritation, resulting from the action of the high atmospherical heat and other causes of excitement under the influence of which the individual is placed, which debars them from the possibility of resisting the effects of additional and sudden stimulation, and, from the disturbance thus created, opens the way for the morbid impression of the remote cause of the disease. As to the manner in which the habitual use of food to excess, and especially of spirituous liquors carried to the degree of intemperance, operates in facilitating the agency and aggravating the effects of that cause, little need be said. Every one will refer it in part to the indirect debility and weakening of the vital powers of the whole economy; to the semi-disorganization of parts essential to life; to the general disorder of the nutrient process; also to the constant over-excitability of the organs of digestion which attends such practices, as well as to the rapid oxidation of substances thus used to excess, whereby they are themselves eliminated from the blood, and, by robbing the oxygen supplied by the respiratory process, prevent a like change in other materials of which it is one of the special objects of that process to get rid, the whole inducing a change in the system which places it at the mercy of most morbid influences, and deprives it of the degree of reactive power required to enable it to surmount the encroachments of these, and insure their timely elimination.

These remarks relative to the injurious effects of sudden repletion and stimulation, in times of epidemics, of organs already in a state of excitability or irritation, will account for the fact, observed in all countries within the yellow fever zone, of individuals being seized with the disease the day after indulging in an excess, and attributing to a simple indigestion the symptoms under which they labour.² Dr. Rush, in his account of the epidemic of 1794 (iii. 202), calls attention to both the suddenness of such attacks and the mistakes to which they gave rise; not without, however, riding his favourite hobby, about the unity of disease, and the exclusive prevalence of one disease, further than most readers will subscribe to. “It was sometimes unfortunate for patients when the disease was excited by an article of diet, or by any other cause which acted suddenly on the system, for it led them, and in some instances their physicians, to confound those exciting causes with its remote cause, and to view the disease without the least relation to the prevailing epidemic. It was from this mistake that many persons were said to have

¹ Pugnet, p. 335; Bally, p. 365; Rochoux, pp. 26, 27.

² Rochoux, p. 27.

died of intemperance, of eating ice-creams, and of trifling colds, who certainly died of the yellow fever. The rum, the ice-cream, the changes in the air, in all these cases, acted like sparks of fire which set in motion the quiescent particles of tinder or gunpowder."

Immoderate Evacuations.—We have seen that, among the predisposing and exciting causes of the yellow fever may be enumerated a rigid, abstemious diet. Still more injurious will be found a resort to immoderate evacuations, or anything calculated to debilitate considerably the system. Whether moderate bleeding and purging will, as Dr. Rush¹ and others have urged, prove advantageous in warding off the disease, or diminishing its violence, is a question I need not now investigate; but certain it is, that the eminent physician just named, while advocating the practice, acknowledges that some persons, from noticing its success, were led to use those remedies in an excess, which both predisposed them to the disease, and excited it into action. "The morbid effects of these evacuations," he adds, "were much aided by fear—for it was this passion which perverted the judgment in such a manner as to lead to the excessive use of remedies which, to be effectual, should only be used in moderate quantities" (p. 36). Currie (p. 10), in like manner, enumerates among the predisposing causes of the disease "frequent purging and bleeding." Dr. John Clark, in his work on the diseases incident to long voyages (i. 164), entertains similar views. Lempriere (ii. 11), speaks of the injurious effects resulting from violent evacuations of all kinds, whether natural or artificial. Osgood, too, enumerates sudden excess of any of the evacuations among the predisposing causes of the disease. "Irregularity in the discharges from the bowels, the skin, and the urinary bladder, is very often introductory to malignant fevers. These have been occasioned by taking remedies while in health, with the false expectation of preventing them. If the body chance to be a little bound, which is as natural to it as for the mind at times to be dull, it is conceited immediately that a purge is necessary. A dose of jalap and calomel, or some other strong purgative, is accordingly taken, which excites much irritation, and sometimes inflammation on the surface of the intestines; and a flux, vomiting, suppression of urine and of perspiration, or even a malignant fever, takes place in consequence."² Many individuals, indeed, have not only been placed in a condition to be necessarily and strongly impressed by the efficient cause of the disease, but many an attack has been quickly brought about by an imprudent bleeding, by a small dose of purgative medicine, or by a diarrhœa occasioned by ordinary and casual causes.

GESTA. Fatigue.—Fatigue of body, induced by labour, by walking, riding, dancing, watching, or the like—but more especially when combined with exposure to the heat of the sun—has at all times, and in all places, been regarded, when carried to excess, as a fruitful cause of an attack of yellow fever. It hastens and increases the disintegration of the muscles, the particles of which cannot be oxidated and eliminated with sufficient rapidity,

¹ Fever of 1793, p. 35.

² Caution to Seamen, &c., p. 13.

and consequently remain in the blood, the life power of which they impair. Like the other influences noticed, it hence exhausts the power of the system and lessens its capability to eliminate the efficient cause, and resist its baneful impression. "Labor sub sole fervido, vel per se vel cum ratione intemperata vivendi junctus, originem dare possunt huic feбри," says Dr. Veitch,¹ and a like sentiment is entertained on the subject by the best authorities in the West Indies and this country.² As regards the former, it has been remarked, that every forced march of unacclimatized troops is sure to be followed by a large accession of cases. In the same category must be placed all actions which excite the imagination and strongly stimulate the system—excesses of all sorts—gambling, and the like.³ Relative to the fever of this city, we find the College of Physicians, in their Address in 1793, recommending to the citizens to avoid all fatigue of body.⁴ Dr. Currie (p. 10) dwells on the injurious effects of the same cause, and Dr. Rush does not hesitate to say that it was labour which excited the disease so universally among the lower class of people. "A long walk often induced it. Few escaped it after a day, or even a few hours spent in gunning." "Perhaps riding on horseback, and in the sun, was the exciting cause of the disease in most of the citizens and strangers who were affected by it in their flight from the city."⁵

Venereal Excesses.—The physical enervation and nervous disturbance resulting from repeated indulgences in the venereal act, have long been regarded in the light of a fruitful predisposing and exciting cause of epidemic diseases of a febrile character. Centuries ago Hippocrates pointed out the injurious effects of the act during those seasons of the year most prolific of complaints of the kind: *Venus neque æstate, neque autumnō utilis est; æstate in totum, si fieri potest abstinendum.* In more recent times, an author whose writings long ranked among the most prized of text-books, tells us: "Immoderata seminis profuso non tantum utilissimi humoris jactura, sed ipso etiam motu convulsivo que emittitur, frequentius repetito, imprimis lædit."⁶ Writers on the plague and the climate of Egypt have, with few exceptions, noticed the baneful effect of this truly morbid agent.⁷ Dr. Johnson (*Trop. Cl.*, pp. 76–7), Fontana (p. 7), and other writers, have referred to similar

¹ Methodo, &c., p. 27. See also his work on Yellow Fever, p. 157.

² Warren, p. 20; Poissonnier, p. 7; Bancroft, p. 29; Lempriere, ii. 9; Hillary, p. 146; Hunter, pp. 18, 154; Jackson (Outline), p. 248; Good, ii. 195; Osgood, p. 22; Perlee, p. 10; Dariste, p. 217; Barton, p. 20; Bally, p. 375; Chisholm, i. 304; *Ib.*, Manual, p. 198; Pugnet, p. 335; Moseley, p. 434; Girardin, p. 51; Thomas, p. 78; Moreau de Jonnes, p. 253; Dickinson, p. 13, 84; Cartwright, ix. 17; Gros, p. 8; Wood, i. 307; Archer, p. 66; Dickson, Ed. J., iv. 456; Ticknor, iii. 221; Johnson on *Trop. Cl.*, pp. 76, 350; Rochoux, p. 116; Gonzales, *Trattado*, &c., p. 309; Brown, *Fev. of Boston in 1798*, p. 74; Celle, p. 296; Arnold, p. 34; Hume, p. 237; Grant, p. 37; Blair, 61; McArthur, p. 350; Heastie, p. 59.

³ Rochoux, p. 28.

⁴ Carey, p. 22.

⁵ *Fev. of 1793*, p. 29.

⁶ Gaubius, *Pathol.*, sect. 562.

⁷ A. Roche, *Acclimatement des Européens dans les pays Chauds*, *Ann. d'Hyg.*, xxxiii. 48.

results as regards the fever of Bengal, and other parts of India; and if we except Dr. Rufz (p. 34), of Martinique, and a few other writers on the yellow fever, who express some doubts as to the injurious tendencies of repeated coition, there is scarcely a writer on the disease, whether within the tropics, in this country, or in Europe, who has not dwelt, with more or less emphasis, on the reality of those tendencies. "Sexual pleasures," says Dr. Celle, "have certainly killed more men in hot and yellow fever regions than intemperance."¹ Among the first, we have the testimonies of Desportes (i. 23), Poissonnier (p. 7), Lempriere (ii. 10), Pugnet (p. 335), Caillot (p. 138), *Dict. des Sci. Méd.* (xv. 360), Lind (p. 185), Savarésy (p. 228), Dariste (p. 20), Bally (p. 375), Rochoux (p. 28), *Reporters on the Fever of Cayenne in 1850* (p. 161), and more especially of Chervin, who, in his report to the Academy of Paris on the memoirs of Rufz, and in commenting on the statement of the latter says: "Notwithstanding all this, it is a fact that those excesses constitute one of the most powerful causes of the development and severity of the yellow fever. The reporter of your committee could mention several remarkable examples of their fatal effects—which, indeed, have been pointed out by almost every observer" (p. 34). Pariset (p. 511), Rochoux (p. 116), have noticed the same results in Spain; and in this country, Addoms (p. 19), Valentin (p. 90), Thomas (p. 77), Gros (p. 8), Townsend (p. 286), are equally explicit on the subject; while, in reference to the fever of Philadelphia more particularly, Deveze (p. 114) says: "As for the act of coition, it is a well recognized rule, that it must be abstained from, as much as possible, in countries labouring under an epidemic. In those countries it has always been noticed that newly married couples invariably fall victims to the disease."

Professions and Occupations.—In most of the epidemics of yellow fever, the records of which have been preserved, it has been remarked that certain occupations of life have a predisposing or exciting tendency; while others act in a contrary manner, and appear to place the system in a condition best fitted to resist the action of the efficient cause. "It has always been observed," says Deveze (p. 111), "that curriers, tanners, soap-boilers, candle-makers, and, in general, all those who habitually breathe an unwholesome atmosphere, are not liable to the disease." Dr. Rush, in like manner, notices the comparative exemption of butchers, only three of whom out of nearly one hundred died in 1793. The scavengers also escaped almost entirely; a very few grave-diggers were infected—a circumstance already noticed by Dr. Clark; and scarcely an instance was heard of persons taking the disease who were constantly employed in digging cellars (iii. 83, 84).

A similar remark has been made by Dr. Merrill, of Natchez, in reference to the exemption of butchers "who are more accustomed to putrid animal exhalation than almost any occupation." By Valentin (p. 91), Caillot (p. 123), Rochoux (p. 116), and other writers² on the fever of various parts of this country, Europe, and the West Indies, the same preservative influence of

¹ Hyg. des Pays Chauds, p. 218.

² See Med. Repos., ii. 200.

those occupations, and others of a kindred nature, has been noticed so pointedly, that although Bally (p. 307), may doubt the accuracy of the facts mentioned, and place the opinion in question among the dreams of the imagination; it would be hazardous to deny the reality of the exemption. This reality, even were the facts by which it is established less positive than we know them to be, would receive confirmation from the circumstance, that similar exemptions were noticed by Ambrose Paré during the plague of Paris in 1565; at Moscow, as well as at London in 1665.¹ Tanners escaped at Rome, according to Fonsaca, Mindererus, and Schenek.²

But, while the occupations mentioned have a tendency to preserve the system from the agency of the cause giving rise to the yellow fever, others produce a different effect. Sir James Fellowes (p. 61), quoting from Arejula, says "it was observed that few cooks escaped during the epidemic (of 1800), and it was very prejudicial to those who were obliged to remain long near the fire." The same fact was observed by Bally (p. 307), and by Townsend (p. 253), in regard to individuals engaged in sugar-house operations; by Drysdale (*Med. Mus.*, i. 31), as regards blacksmiths. Rochoux (p. 116) alludes to the liability of bakers, locksmiths, and especially cutlers, and remarks that one of the streets of Barcelona, principally inhabited by the latter, had 180 deaths; tailors also, as well as mattress-makers, suffered greatly. Pariset, alluding to the greater liability at Barcelona of cooks, blacksmiths, and especially bakers, says, with his usual exaggeration: "The bakers of Barcelona were so rapidly carried off by the disease, that it was at one time feared that no one would be left to furnish bread" (p. 454). A circumstance so generally noticed elsewhere, and which has been observed to occur in relation to the bubonic plague both in the East and at Moscow,³ could not fail to be noticed in this city. Hence, as Deveze remarks, "locksmiths, bakers, and all those who habitually go near to furnaces are easily attacked by the disease" (p. 111). Dr. Rush, in like manner, notices the greater mortality among bakers, blacksmiths, and hatters (iii. 49).

As was the case at Barcelona in 1821,⁴ and as has been the case in every place—at least in temperate climates, where the sphere of operation of the efficient cause is more circumscribed—the disease in this and other cities of the Union has mostly prevailed among the lower and poorer classes of society. The cause of the greater liability of this portion of the population must be sought in their greater exposure to the action of predisposing and exciting agencies, but especially to their inhabiting localities where the efficient cause of the disease is more extensively diffused and its action most energetic, and to their occupying crowded, ill-ventilated, damp, and filthy houses. The influence of locality, in this respect, is well illustrated by the occurrences of

¹ McLain, i. 271, 272; Wittman, p. 521.

² Plouquet, *Bibliotheca*, vi. 364. See Hancock, p. 183; Thomas's Practice, p. 223; Volney, ch. 17; Webster, ii. 383.

³ Wittman, p. 521; C. Maclean on Epid., i. 264; Samoilowitz, *Mém. sur la Peste*, &c., p. 124.

⁴ Rochoux, p. 117; Pariset, p. 493.

Barcelonette in 1821; for there the disease proved more general and fatal among the rich, who, for various reasons, occupy the streets bordering on the port; while the poor reside in the upper and more healthy portions of the suburb.¹

But, by whatever class of the population such localities may be occupied, and whatever may be the portion of a city where such localities are situated, it is found that there the chances of escape are greatly lessened, and the disease, when it occurs, assumes in general a more malignant and dangerous character. In saying this, I lay aside for the present the question as to the evolution from these localities of the efficient cause of the fever. Admitting, for the sake of argument, that this cause has not its origin in these places, or at least not in all of them, it is impossible to shut our eyes to the fact that the localization of the yellow fever takes place only, or principally, where certain peculiar combinations of materials appertaining to the soil, or which may have found their way there accidentally or otherwise, are discovered; that it takes place also in houses, rooms, yards, &c., where we meet with certain peculiarities and conditions of circumstances connected with the population, or where the objects by which they are surrounded, and which are known to be inimical to health, exist—offensive effluvia, putrescent food, foul water, imperfect ventilation, besides some of the agencies already dwelt upon. Bearing this in mind, we arrive at once at the conclusion, that the real cause, whatsoever it may be, meets there certain agencies which so modify the system as to render it liable to its morbid impress. In a word, what many regard as the active and efficient cause of the yellow fever, may, after all, be only a predisposing agent. If this be correct, the disease is in that respect, on an equal footing with others of a kindred nature. Every one knows that while Asiatic cholera and the febrile exanthemata are never produced by the malarial exhalations evolved from foul localities or marshy surfaces; while typhus and typhoid fevers are, as it is said, seldom the offspring of the former, and certainly never of the latter; while none of these diseases are occasioned by the ingestion of putrescent food, by the use of foul water, by imperfect ventilation, by starvation, by excessive muscular exertions, by the intemperate use of alcoholic liquors, and the like; and while, with the exception, perhaps, of typhus, they do not arise from the effluvia proceeding from the human body—particularly the lungs and skin—and consisting of the effete and highly putrescent matter mingled with the air or perspiration—it is a notorious fact, that they are principally rife in situations where such influences operate, and strike with greater violence, malignancy, and fatality among individuals exposed to their baneful effects. This is true, whether the disease be the product of a zymotic poison floating in the atmosphere, and independent for its development of any organic process, as Asiatic cholera; or whether it arises from a poison formed in the system and transmissible from one individual to another through means of contact, or the medium of the atmosphere; or whether, again, it is due to a particular poison proceeding from external sources of animal or

¹ Rochoux, p. 117.

vegetable decomposition, or from the result of a morbid condition of the system, as is the case, perhaps, with puerperal complaints, erysipelas, and what has been denominated *surgical fever*.

It will only be necessary to open the records of cholera in all the countries it has visited, and especially in England, where the subject has been investigated with greater attention than elsewhere, or of typhus or typhoid fevers, to be perfectly satisfied that, where the predisposing influences in question exist, there these diseases rage with the greatest violence.

The same must be admitted to take place in relation to the yellow fever, which, like autumnal and periodic fevers of all grades—supposing them not to be *produced* by malarial exhalations—are, like other diseases, principally rife among individuals whose systems have been for a greater or less period of time under the depressing influence of such exhalations. These, like the rest of the recognized morbid influences mentioned, must, at the very least, be admitted, in accordance with that view of the matter, to act injuriously by predisposing the system to the action of the disease; and, when examined attentively, the effect will be found to be evidently accomplished, as Dr. Carpenter well remarks, by producing in the blood of the individual exposed to them an excess of those decomposing organic compounds, which, as physiology teaches us, are always present in the circulating current in minute proportion; being conveyed by it from the spots in which they are introduced, or in which they are generated, to the organs through which they are to be eliminated; and an excess of which is manifestly producible, either by the direct introduction of those matters from without, in the food or drink consumed, or in the air respired; or by the production of them within the body, at a rate beyond that at which they are normally eliminated; or, again, by some obstacle to their elimination, which prevents the amount ordinarily originated from escaping at its normal rate through the usual outlets.¹

APPLICATA AND EXCRETA. *Cold and Wet*.—It has ever been found, in all places subject to the yellow fever, that exposure to cold in any way, especially when the body is heated or perspiring through the effect of exercise or otherwise, as also exposure to the coolness and chilliness of night air, to a shower of rain, &c.—it has been found, I say, that these are frequent exciting causes of the disease. Many an individual, who otherwise might have escaped the direful effects of the pestilence, has been stricken down in consequence of an imprudent neglect of proper precautions in respect to such influences, or an accidental exposure to them. Hence, we find that most writers on the yellow fever of tropical or temperate climates dwell on the injurious effects of even a momentary exposure to wet and cold. The Cubans attach great importance to the necessity of avoiding wet clothes; in order to accomplish this, they strip themselves whenever it rains and they cannot shelter themselves, and receive the shower on their bare bodies. The process, they say, is free from danger, and assimilable to a bath.² Pugnet remarks that, in the West Indies, it is not uncommon for the suffocating heat of the day to be tempered by cool

¹ Brit. and For. Med.-Chir. Rev., Jan. 1853, p. 162.

² Ulloa, Mém. Phil., i. 252.

winds of short duration, which, while proving grateful to the feelings, are fraught with danger to those who come within reach of their influence (p. 335); and Savarésy (p. 228), McArthur (p. 350), Ferguson (*Med.-Chir. Trans.*, viii. 148), Hunter (p. 18), Moseley (p. 432), Ruz (p. 36), Hillary (p. 246), Lempriere (ii. 9), Bancroft (pp. 29, 185), Gonzales (p. 309), Ulloa (*Mém. Phil.*, i. 252), *Dict. des Sci. Méd.* (xv. 361), Moreau de Jonnes (pp. 98, 253), Bally (pp. 368–374), Rochoux (p. 29), Dariste (p. 217), Osgood (p. 22), and others, have pointed out the injurious effects of this and other kindred agencies. Authors on the fever of Europe and this country—Girardin (p. 51), Pariset (pp. 477–479), Wood (i. 307), Cartwright (p. 17), Tieknor (iii. 219), Drysdale (i. 32), Areher (p. 66), Brown (p. 74), and Thomas (p. 26)—are equally explicit on the subject. Impressed, from experience, with the conviction of the danger of exposure to cold and wet, the College of Physicians, in their address issued in 1793, recommended to the inhabitants of Philadelphia to avoid standing or sitting in the open air, “to accommodate their dress to the weather, and to exceed rather in warm than in cool clothing.”¹ Other writers on our epidemics are not less positive as to the danger of vicissitudes of temperature and exposure to wet. Entering more fully on the subject, Dr. Rush remarks that in 1793 the fever was brought on by the night air of the warm month of September, when the dress was not accommodated to the great coolness of that period; adding, that the disease was excited in one case by a person only wetting his feet in the month of October, and neglecting afterwards to change his shoes and stockings. “Every change of the weather,” he says, “that was short of producing frost, evidently increased the number of sick people.” “After the body had been heated by violent exercise, a breeze of cool air sometimes excited the disease in those cases where there had been no change in the temperature of the body;”² and in a subsequent publication, he says that while heat has excited the yellow fever in thousands, cold has excited it in tens of thousands.³

From what has just been said, it will readily be inferred that exposure to night air, sleeping in the open air, exposing the body in any way to the refrigeration produced by upper radiation of heat, and the use of cold drinks when the body is heated, will act injuriously, and tend not only to predispose the system to the impression of the cause, but to bring on an attack. Warren long ago noticed the danger accruing upon “drinking large draughts of cool liquors, particularly cold water, upon the aforesaid occasions.” “Sometimes,” says Dr. Blair, “the *determining* cause seemed of the slightest description—the shock on the stomach of an ice-cream or glass of ice-punch, or the indigestion of an unripe orange, would occasionally set the train of symptoms in motion” (p. 60).

So evident and active is the agency of these causes in exciting the disease in individuals exposed to their influence, that there are not wanting those who, as Folehi, Santarelli, Jones and others have done in regard to the fever

¹ Carey, p. 22; Folwell, xi.

² Fev. of 1793, *Med. Inq.*, iii. 50.

³ On the Means of preventing Summer and Autumnal Diseases, *Med. Inq.*, iv. 123.

of the Pontine Marshes, as well as of our ordinary fever, have regarded the refrigeration produced in the system by the effect of cold nights, combined with the common humidity of the atmosphere, the dews of that period, and the upward radiation of heat, on bodies previously heated through the influence of the high temperature of the day, as the sole or efficient cause of the yellow fever. Intending to revert to this subject in a subsequent chapter, where it will find a more appropriate place, I dismiss it for the present.

That such agencies should give rise to the effect mentioned, cannot be a matter of surprise. Dr. Rush, in accordance with the peculiar views he entertained relative to the nature of fever, thought that the action of wet and cold in exciting the disease "depended upon the diminution of the necessary and natural heat of the body, and thereby so far destroying the equilibrium of the system as to enable the miasmata to produce excessive or convulsive motions of the bloodvessels." Without stopping to examine the correctness or erroneousness of this explanation, it will be sufficient to remark that any external agency capable of disturbing, even momentarily, the healthy tone of the system, of driving excitement from the surface to the central organs, and causing a sudden abstraction of cutaneous caloric and a suppression of perspiration, will necessarily occasion morbid changes in the functions, and place the system in a condition to be affected more surely than it would otherwise be by the efficient cause of the fever. The maintenance of health under the influence of high temperature, atmospheric or artificial, has even been considered as depending in a marked degree on the non-interruption of cutaneous excitement and of the perspirable process. Carried beyond certain bounds, this excitement, it is true, induces a diseased state, which, among other effects, renders the system susceptible to the impression of morbid influences, whether of a contagious or other character; but I need scarcely call the attention of the reader to the fact that the sudden diminution of that excitement, and the suppression of the perspiration attending it, are, under all circumstances, fruitful sources of disease, and must therefore readily excite into play the injurious influences of morbid causes amidst which systems so affected are, as it were, immersed; but which, so long as the functions remain unimpaired and the healthy equilibrium is preserved, are comparatively innocuous. The baneful effects of such suppressions are not limited to the peculiar form of disease under consideration. They are observed in regard to all complaints which assume the epidemic character, whether these be of a malarious origin or depend on a contagious principle.¹

In conclusion, it may be remarked that the suppression of other natural or artificial excretions, as well as the sudden diminution or removal of irritations to which the system has become in some measure accustomed—issues, blisters, alvine evacuations, ulcers, chronic cutaneous eruptions, discharges, or the like—have been found to produce an injurious effect, and prove the harbinger of an attack of the fever.²

¹ Bally, p. 374; Lefort, p. 12; Rochoux, *Typhus Amaril*, pp. 29, 118; Blair, p. 61.

² Bally, p. 371; Savarésy, p. 176; Rochoux, *op. cit.*, p. 30; Heastie, p. 59; Blair, *loc. cit.*

CHAPTER V.

CIRCUMFUSA.

HAVING now disposed of those causes which, having reference to the particular disposition or condition, natural or acquired, of persons exposed to the disease, I have, in imitation of some preceding writers, denominated organic or *individual*, I pass to the consideration of the *circumfusa*, under which title are included the various conditions of the atmosphere, such as temperature, the electric and hygrometric states, and the nature of the aerial currents. To the influence of each of these I shall, in this and some subsequent chapters, call the attention of the reader.

Temperature.—The yellow fever is undeniably a disease of hot climates and hot seasons. Everywhere a high range of thermometric heat has been found necessary to insure its production; for, whatever be the condition of the localities where it generally breaks out and prevails, the occurrence fails unless the thermometer marks a high degree of temperature, and continues to give a certain average during some weeks or months. We all know the average summer temperature of the tropics, the proper soil of the disease. Nowhere, whether in Africa, the West Indies, or on the South American coast, does it fall under 80° . At Sierra Leone, where the fever has prevailed several times epidemically, the thermometer varies from $77\frac{1}{2}^{\circ}$ to $86\frac{1}{2}^{\circ}$, giving an average of about 80° . In the West Indies and on the eastern coast of tropical America, where the disease is, as it were, endemic, and prevails almost annually, the average range of the thermometer, during the summer months, does not fall much below 80° . In the French islands, it reaches rarely higher than 90° , and presents an average, during the middle of the day, of about 85° . But there it remains nearly stationary, the variation from the hotter to the cooler part of the year not amounting to more than from four to six degrees, the average in the former being about 87° , and during the latter season, 80° .¹ Major Tullock, in his admirable *Reports on the Sickness and Mortality of the British Army*, confirms these statements, remarking that in the windward and leeward command (which includes British Guiana, Trinidad, Tobago, Grenada, St. Vincent, Barbadoes, St. Lucia, Dominica, Antigua, Montserrat, St. Kitts, Tortola, and Nevis) the mean height of the thermometer is $80\frac{1}{2}^{\circ}$. In none of the islands is it above 82° , or under 79° . The difference between the highest and lowest mean range is, even in the most variable of the islands, only 13° , and in some it is not more than 4° throughout the year (p. 3). In that part of the island of Jamaica

¹ Cassan, Mém. de la Société d'Émul., v. 154–5; Savarésy, p. 241; Moreau de Jonnes, p. 352; Bally, p. 320.

where the fever is rife—the north side—the heat is even greater, the thermometer varying from 83° to 92° throughout the year.¹

Dr. N. Diekenson,² in a work already cited, says: “By examination of a table drawn up from a daily register, it is shown that the greatest range of temperature has not exceeded six degrees on Fahrenheit’s thermometer in the different years or different seasons of the same year; while the greatest difference observed, at any time within the twenty-four hours, has seldom been more than fourteen degrees; the thermometer upon these occasions being exposed in the shade to a northern aspect in a large room, through which there passed a current of air.

“The medium height, thus observed in the neighbourhood of the coast, and about 700 feet above the level of the sea, has been from 82° to 84° . The greatest elevation, about 96° and the lowest 72° . Exposed to the direct impression of solar radiation, the mercury in the thermometer has risen to 140° .”

In Vera Cruz, according to Humboldt, the mean temperature of the year, during a period of fourteen years, was 77 (25 Cent.); that of the sickly season, from April to October, inclusive, was about 80° (27.09 Centigrade), whereas in the winter, November to March, inclusive, it did not attain to more than about 72° (p. 767).

It will be useless to cite more facts in proof of the connection of high atmospheric temperature with the prevalence of yellow fever in tropical regions; for this connection is universally recognized not as regards one portion of those regions alone, but all. In every locality where the yellow fever prevails in an endemic or an epidemic form, the thermometer gives us an average heat of 80° , or thereabout. In all these places, the disease shows itself only at the period of the year when the heat is greatest.³ In all it usually occurs with particular severity in seasons of the highest temperature; while it seldom attains its greatest degree of severity before the heat has continued at its maximum average for some time.⁴

In our southern States, where the winter season is characterized by cool weather, the summer heat approaches that of tropical regions;⁵ and when the fever, which, as is known, is not of annual occurrence, breaks out, it does so generally in seasons when the range of the thermometer equals that of the West Indies and of Vera Cruz, to say nothing of Puerto Bello, La Guayra, and Carthagená; while the disease, as in the Antilles—and, indeed, in every part of tropical regions, appears only after this elevated temperature has continued some time. Such has been the usual course of events at New Orleans, Mobile, Natchez, Charleston, Savannah, Galveston,

¹ Tullock, p. 42.

² Observations on the Inflammatory Endemic, or Yellow Fever, pp. 51, 52.

³ Maher, p. 831.

⁴ Humboldt, p. 765; Maher, p. 831; Belcher, Edin. Journ., xxiii. 248; Ralph, Ed. Med. Ch.-Tr., ii. 52; Tullock, Rep., p. 76; Arnold, p. 26; Boyle, p. 46; Winterbottom, Facts and Obs., viii. 72; Rufz, p. 29; Imray, Ed. Journ., lxx. 255, 257; J. Clark, p. 50.

⁵ Chalmers, pp. 42, 47; Girardin, p. 25; Ramsay, ii. 50.

Key West, and other places in the Gulf of Mexico and in the southern section of the Mississippi Valley, where heat is universally regarded in the light of an important agent in the production and diffusion of the disease.¹

Nor is this less the case in our Middle States, where the summer temperature likewise approximates closely to that of the torrid zone, for in them the fever has scarcely ever appeared, unless the mean temperature has reached, or approached to, 80°. This has been observed at Norfolk, Baltimore, New York, Providence, Boston, and New London. All this the reader will easily find by referring to the writings of Valentin,² Whitehead and Selden,³ Archer,⁴ Dalmás,⁵ Miller,⁶ Seaman,⁷ Bayley,⁸ Townsends,⁹ Drysdale,¹⁰ Wheaton,¹¹ Brown,¹² Channing, and others.¹³ Sir Gilbert Blane has called attention to the same fact, in relation to the yellow fever of Europe,¹⁴ and his observations are fully confirmed by those of every writer on the epidemics of Cadiz, Barcelona, Gibraltar, Leghorn, Xeres, Seville, Carthagena, Marseilles, &c., where the appearance and prevalence of the disease has, as elsewhere, invariably been associated with a mean temperature, in every way equal to that observed during sickly seasons within the tropics.¹⁵

¹ Gros, p. 5; Thomas, pp. 62, 78, 109; Barton, *Fev. of N. O.* in 1833, pp. 4, 5; Baxter, *Med. Repository*, xxi. 3; Rept. on Fever at N. O. in 1819, p. 6; *Ib.* for 1839, *Journ. of Med. Soc.*, p. 156; *Ib.* for *Fev. of 1820*, p. 4; Shecut, *Med. Essays*, pp. 77, 93, 103; Tooley, *Fever of Natchez*, in 1823, p. 7; Girardin, p. 51; Merrill, *N. A. J.*, ii. 237; *Ib.*, *Med. and Phys. Journ.*, ix. p. 233; Monnett, pp. 11, 35; Chabert, p. 23; Perlee, *Med. and Phys. Journ.*, iii. 17; *Amer. Almanac*, 1846, p. 82; Allen, *Vital Stat. of Baton Rouge*, *N. O. J.*, viii. 69; Barton, Rept. to State Med. Soc., p. 14; *Charleston Med. Journ.*, iv. p. 803; Cartwright, *Med. Recorder*, ix. 6; Dickson, *Med. and Phys. Journ.*, iii. 251; *Ib.*, *Eclectic Journ.*, iv. 112; Simon's Address, p. 3; *Ib.*, Rept., p. 10; Chalmers, *Climate of S. C.*, i. 164; Moultrie, p. 2; Ramsay, *Hist. of S. C.*, ii. 83; Lining, *Edin. Essays*, ii. 409; Dupré, *Am. Journ. (N. S.)*, ii. 382; Waring, p. 20; Tucker, *Barton's Med. and Phys. Journ.*, ii. 22; Fenner, *N. O. Journ.*, Sept. 1848, p. 194; Fenner, *Fev. of N. O.* in 1853, pp. 8, 9; *Ib.*, *Trans. of Am. Med. Assoc.*, vii. 427; Drake, ii. 193, &c.; Barton, Rept. on Sanit. Condition of N. O., 1853, p. 229, &c.

² *Fièvre Jaune*, pp. 85, 86.

⁴ *Med. Recorder*, v. 61.

⁶ *Works*, pp. 118, 119, 433.

⁸ *Fever of N. Y.* in 1795, pp. 51-2, 124.

¹⁰ *Med. Museum*, i. 31.

¹² *Med. Repos.*, ii. 360, 467; *Treatise on Fever of Boston*, in 1798, p. 26.

¹³ *Med. Repos.*, ii. 406; Watts, *N. Y. Med. Reg.*, p. 30; *Med. Repos.*, ii. 206; Smith, on Epidemics, p. 78; Hill, *Fevers of Wilmington*, *N. C. Recorder*, v. 87; Hardie, *Fever of N. Y.*, in 1798, end of vol.; Revere, *Fever of Baltimore*, in 1819, *Recorder*, iii. 217.

¹⁴ *Dissertations*, ii. 155.

¹⁵ Fellows, pp. 33, 244, 565; Amiel, in Johnson, *Trop. Climates*, p. 250; Pariset, *Fièvre de Barcelone*, pp. 12, 185; Robert, *Guide Sanit.*, pp. 110, 111, 742; O'Halloran, p. 122; Burnett, *Fever of Medit.*, p. 205; Tommasini, sect. 161, ii. 483; Bally, p. 323; Rayer, p. 23; Rochoux, pp. 110, 111; Audouard, p. 44; Arejula, pp. 132, 133, 134; Lind, p. 91; Berthe, pp. 154, 324. Vance, in Pym, p. 64; Tullock, *Rep. on Sickness of Troops in Medit.*, p. 4, D.; Caisergues, pp. 18, 19; Pierquin, *Trad. de Mém.*, &c., sur la *Fièvre Jaune de Barcelonne*, p. 40; Blin, *Trad. du Rapport sur la Mal. Epid.*, &c., Cadiz, 1800, p. 4; Doughty, *Observ. on Yellow Fever*, pp. 88, 180; Gillkrest, *Cyclop.*, ii. p. 279, &c.

³ *Med. Repos.*, iv. 129.

⁵ *Fièvre Jaune*, pp. 30, 38.

⁷ Webster's Collection, pp. 1, 2.

⁹ *Fever of N. Y.* in 1822, p. 259.

¹¹ *Med. Repos.*, x. 329.

In the city of Philadelphia, the connection between high atmospherical temperature and the prevalence of yellow fever has been found to hold good. It was early pointed out, and has been noticed in all the epidemics that have prevailed here from 1699, to the present year. We have already seen that our summer climate approximates closely to that of the tropics, and we may now add that on no occasion has the disease prevailed where the mean temperature of the warm months has remained below the standard of those regions. On this point, our several epidemics, from the earliest to the latest, leave no doubt. Dr. Caldwell has long ago shown that the fever has never appeared among us except during our hottest summers. In other words, we do not experience the ravages of that disease, except during those summers which, in point of atmospherical temperature, bear the closest resemblance to tropical regions, and on the other hand, the fever seldom fails to appear when the thermometer remains some time at that point. This venerable physician, who seems to have been the first on this side of the Atlantic to call attention to this fact, shows, from observations kept during several years, from 1796 to 1806, inclusive, that the yellow fever did not make its appearance¹ unless the mean temperature of the summer months reached 80°, and remained so forty days or upwards. Since his publication, a more extended series of observations made by Mr. Cadwallader Evans, from 1793 to 1817, inclusive, have shown that the fever, during that long period, has never prevailed "at all, or so as to create alarm when the mean heat," at 3 P. M., "of all June and July, had been lower than 79°." The only exception to this was in 1802, when the thermometer indicated a mean of 78°. These observations show that "in every summer, where it has been above 79°, it (the disease) has prevailed more or less, and the mortality has been regulated very much by the heat being higher or lower. In 1793 and 1798, which were the hottest summers in all the twenty-five years, it prevailed most, and was attended by the most extreme mortality. In 1797, 1799, 1803, and 1805, when lower degrees of heat prevailed, the mortality was less. In all the other years (except a small mortality in 1802), when the mean heat of those two months was below 79°, at the hour mentioned, we have had no alarm of yellow fever."²

The same connection has been noticed and dwelt upon by others in this city in reference to some of the epidemics mentioned, and those which have since occurred—Deveze,³ Chapman,⁴ Folwell,⁵ Ffirth,⁶ Rush,⁷ Currie,⁸ Emilen,⁹ Carey,¹⁰ Jackson,¹¹ and Jewell.¹² In all, we find that the fever has only

¹ Fever of 1805, p. 15; Med. Mem., pp. 140, 143; Ib. (1826), p. 87.

² Cadwallader Evans in Eclectic Repert., vii. 425; Med. Rec., i. 139.

³ Op. cit., p. 116.

⁴ Med. and Phys. Journ., viii. 356.

⁵ Fever of Phila. in 1798, pp. 13, 22; Ib., 1797, p. 48.

⁶ Treatise on Mal. Fev., pp. 21, 22, 23, 24.

⁷ Works, iii. 41, 49, 120.

⁸ Currie, Fever of 1798, pp. 2, 4, 5, 37, 38; Ib., Fever of 1793, p. 15; Fev. of 1799, p. 29.

⁹ N. A. Journ., v. 329.

¹⁰ Fev. of 1793, Meteorol. Tables.

¹¹ Fev. of 1820, pp. 12, 13.

¹² Fev. of 1853, pp. 10, 40.

occurred in seasons marked by a degree of heat equivalent to that of the tropics.

With these facts, as to the necessity of a tropical temperature of some continuance for the development of yellow fever, we find ample reason for regarding heat as playing an important part in the production of the yellow fever, whether we view it as a necessary, exciting, or predisposing cause, or as aiding in the development of agencies of a more efficient nature.

Here, as in the South, and in the West Indies, the disease is almost inevitably brought on by exposure, on the part of those unacclimatized to the heated atmosphere of a sickly locality—especially to the action of the sun—the danger of an attack being proportioned to the extent of that exposure. The nature of the agency of heat has been so considered by Dr. Rush, who says: “Heat from every cause, but more especially the heat of the sun, is a very common exciting cause of this disease (of 1793). The register of the weather during the latter end of August, the whole of September, and the first two weeks in October, will show how much the heat of the sun must have contributed to excite the disease, more especially among labouring people. The heat of common fires likewise became a frequent cause of the activity of the miasmata where they had been received into the body; hence, the greater mortality of the disease among bakers, blacksmiths, and hatters, than among any other class of people” (iii. 49). Deveze, in like manner, dwells on the efficiency of heat as a cause of yellow fever (p. 116), and, after enumerating exposure to the direct rays of the sun among the exciting causes (p. 113), remarks, that everywhere the intensity of the disease and the number of persons attacked are in direct ratio to the elevation of the thermometer (p. 116). Others among our writers have been equally explicit relative to the agency of solar heat, whether as a predisposing or an exciting cause of the disease—Chapman, Emlen, Caldwell, Folwell, &c.—and in various places on this and the other side of the Atlantic sentiments of kindred nature have been expressed. “Exposure to the sun,” says Dr. Osborn, “is a circumstance which must, in some degree, be submitted to, but should be avoided as much as possible, for there is no one exposure so dangerous as this; it is never safe to stand or move in the sunshine without a hat either of fur or wool” (p. 12). In fact, insolation has been regarded at all times and in all places as a fruitful agency in promoting the development of an attack.¹

It may be remarked in confirmation of what precedes, that other diseases, bearing more or less analogy to the yellow fever, require for their development,

¹ Makittrick, p. 115; Williams, p. 18; Addoms, p. 19; Moultrie, p. 3; Gonzales, p. 319; S. Brown, p. 74; Savarésy, p. 235; Bally, p. 373; Rochoux (F. J.), p. 262; *Ib.*, *Obs. sur les Diff. Maladies*, p. 27; A. Hosack, p. 21; Caillot, p. 159; E. H. Smith, pp. 88, 92; Dickenson, pp. 13, 49, 87; Pariset, p. 479; Kéraudren, p. 61; Valentin, p. 90; Drysdale, *Med. Mus.*, i. 31; Thomas (2d ed.), p. 78; Gros, p. 8; J. Clark, p. 45; Arnold, p. 34; Blanc, *Dis. of Seamen*, p. 426; Trotter, i. 348; Grant, pp. 37, 58; McArthur, p. 350; Hillary, p. 146; Vincent, pp. 6, 42; Rouppe, p. 296; *Diet. des Sci. Méd.*, xv. 361; Moreau de Jonnes, pp. 98, 253; Lallemant, *Fev. of Rio Janeiro*, p. 41; Doughty, pp. 71–2; H. McLean, p. 15; Dancer, p. 84; Veitch, pp. 53, 77; Chisholm, i. 124; Henderson, p. 4.

and are under the control of, a certain amount of atmospheric heat, and that, like this fever, they are brought into action by exposure to the influence of the atmosphere of an overheated and unsheltered locality. The connection of a southerly constitution of air with pestilence was long since recognized by Hippocrates, Aristotle, Galen, Celsus, and other physicians and philosophers of antiquity. It was pointed out by Rhazes and Avicenna, and, at a less remote period, by Lomnius, Diemerbroeck, Mercuriales, Forestus, Baynard, Mead, and many modern writers. Our bilious remittent requires for its development a continuance of hot weather. The researches of Dr. Drake, in reference to the Valley of the Mississippi, show that, with the decrease of yearly and summer heat, other conditions continuing unchanged, there is an abatement of the fever; that a summer temperature of 60° is necessary to the production of fever, and the latter will not prevail as an epidemic when the temperature of that season falls below 65° ; finally, that if the other conditions favouring its production are deficient, it will cease before those reductions of temperature have been reached.

The necessity of a high degree of atmospheric heat for the extrication or production of the febrile cause out of the materials from which it is usually found to be obtained, is every day illustrated in Italy; for there, as elsewhere, marshes or collections of putrescent substances, which are, to all external appearances, in a fit state to evolve the poison, and even to emit vaporous effluvia offensive to the olfactory nerves, remain innocuous so long as the thermometer does not reach a high point.¹

“If the temperature,” says Dr. McCormack, “prove habitually below 80° , whatever be the exuberance of vegetation or the quantity of marsh land, malaria is never generated. Malaria does not subsist in the north of England, nor in Scotland, nor in Ireland, nor in Sweden, Denmark, or Norway, nor in the colder parts of Europe, Asia, North and South America; in fine, nowhere, so long as the temperature—a few degrees more or less—keeps below 80° . Let it rise above this, and thereupon we have malaria, and periodic fever in all its forms.”² The same remarks are applicable to the fevers of France, Spain, of India, and of Africa; while in every region where periodic fevers of various types prevail, exposure to great solar heat is often—in some places almost inevitably—followed by an attack. Every instance of fever that occurred in the river Niger, Dr. Pritchett assures us, could be traced to solar exposure, which was therefore, he adds, the obvious immediate exciting cause.³

That the elevated and prolonged temperature which, as we shall see, accompanies the manifestation of yellow fever, is rightly viewed as a fruitful cause of the latter, might indeed have been inferred *à priori* from the known effect of that agency on a system unaccustomed to its action. By itself, and independently of other causes, it produces—whether through the electrical

¹ Thouvenel, *Climat de l'Italie*, iv. 212.

² McCormack on Malaria, *Edinb. Med. J.*, lv. 372.

³ *Some Account of the African Remittent Fever*, pp. 137–8, 145.

changes elicited by it, the greater dilution of the air, and the consequent less oxygen contained in a given bulk of the latter, matters not—disease even of a febrile character, or deranges the visceræ to such a degree as to predispose them to the impression, or aggravate the effect of other morbid causes. Heat produces a direct excitement on the nervous force; contraction when applied to motor nerves, sensation, both special and common, when applied to sensorial nerves. Conversely, it is directly generated by the excitement of that force—showing a relation between these two agencies in some respects similar to that which exists between the nervous force and electricity, though less intimate in degree. As an agent of great power, in these respects, it has long been recognized. The medical literature of all countries exhibits many proofs of the fact, that a burning atmosphere and devastating diseases are often associated together. One of our own writers¹ reminds us that even the poets, who, though styled “the sons of fancy, and the fathers of fable,” are “indisputably among the most accurate observers of nature, have not less uniformly connected the rage of the dog star with the ravages of pestilence.”

It is remarked of those who, from a cold or temperate climate, arrive in tropical regions, that even when they avoid exposure to the direct rays of the sun, they experience a truly distressing sensation of heat, a marked acceleration of the pulse, and, towards the warmest part of the day, an irresistible disposition to sleep, attended with a feeling of lassitude, and sometimes fullness and heaviness of the head. The nervous power is impaired, and, as a consequence, the muscular is enfeebled. The cutaneous surface becomes, as it were, puffed up, the face and eyes are red, and perspiration—the natural vent which nature has established for the escape of superabundant heat—is excited by the least muscular exertion. During the night the individual is restless and excited. Sleep is often interrupted, the tongue dry, the thirst great, and the mouth somewhat bitter. Towards morning he feels better, and continues so until eight or ten o’clock, when the symptoms of the previous day are renewed.

The effect is more severe when the individual is obliged to expose himself during several hours to the influence of the sun. The sensation of heat, under these circumstances, soon becomes extremely distressing. The perspiration is not as copious as before—for we know that excessive cutaneous heat is a bar to the secretion—but is replaced by a sensation of ardor and tingling of the skin. Before long, the thirst becomes excessive, the pulse is febrile, and the head and loins painful. The blood becomes affected, florid, and thin. Epistaxis not unfrequently attends. This truly morbid state is mitigated, though not always completely relieved, by sleep and the coolness of early morning, and is almost certain to be converted into positive disease on frequent repetition of a similar exposure. The mucous membrane of the alimentary canal becomes implicated; irritation, amounting often to the degree constituting inflammation, ensues, and affects the stomach and bowels. The

¹ Chapman, Med. and Phys. J., viii. 355.

hepatic organs, also, are deranged, the secretion of bile being often greatly increased, and the fluid finding vent in choleric or diarrhœtic discharges, with or without dysenteric symptoms. The lungs themselves, though less frequently affected, suffer sometimes from irritation arising from the direct application of hot air to their lining membrane. Nor is it less true that, in those who avoid such exposure, the protracted and excessive heat of the climate produces an undermining effect, which, in a large number of cases, finally leads to an attack of disease.¹

The picture thus offered of the morbid effects resulting from a high atmospheric temperature in hot climates, is not very different from that noticed not only in our southern States on strangers, but in our own latitude, on all classes, as the observations of each of our summers may fully demonstrate. To the action of heat during this season our systems cannot, for reasons already stated, become completely inured; and though we may not suffer from it as much as natives of colder climates, we experience its effects to a marked extent. At each yearly return of the warm months, few are found who do not present some of the symptoms enumerated, and nothing but the greatest care on the part of individuals thus exposed can shield the majority from experiencing the effects in a more serious degree. The great medical reformer of modern times, speaking of enteritis, remarks, that he found the climate of Italy to exercise on the French a stimulating action to which all individuals do not easily accustom themselves.² We know what the effect of the climate of India and other hot countries is on the unacclimatized northerner. Everywhere, as here, heat acts as a direct stimulus, exciting the system at first to a febrile condition, and even occasionally producing inflammation; or leaving it in a state of extreme lassitude or torpor, which renders it vulnerable to the attack of various diseases, either purely febrile, or blended with gastric, hepatic, or intestinal disturbances.³

In connection with this subject, it may be stated that in those steamers in which the fever breaks out in tropical climates, it has been found that, instead of occupying, as it commonly does on board of sailing vessels, one, two, or three months, it not unfrequently embraces, though not equally, the greater part of the year. Such was the case on board the British steamers, the *Dec* and *Rhadamanthus*, in 1835. The reader must bear in mind, also, the obstinacy with which the disease elung to the *Eclair* in 1845. Dr. Wilson, from whom I derive the reference to the two first named vessels, remarks that more time and a larger field of observation are required to settle the point; but that, if such diffusion as to time in the operation of the cause of fever were found to hold generally in steamers, it would be interesting to inquire whether it was connected with the steam, or rather with the heat which produces it.⁴

¹ Rollo, *Obs.*, p. 42; Fontana, pp. 8, 10; Pugnet, pp. 333; Johnson, on *Trop. Cl.*, p. 10, ed. of 1819; Savarésy, pp. 175, &c.; Rochoux, p. 7; Leblond, p. 86; Bally, p. 319; Dickinson, p. 95, &c.; Evans, p. 41; Girardin, p. 46; Thomas, p. 78.

² Broussais, *Phlegmasies Chroniques*, ii. 629–30.

³ Chapman, *op. cit.*, viii. 356.

⁴ Wilson, *Statistical Rep. on the Navy*, fol. p. 110. See also Arnold on *Bil. Remittent Fever*, &c., pp. 302–3.

But are we, while admitting the agency of heat as an exciting and predisposing cause of the yellow fever, to regard it as of itself, and independently of other morbid influences, sufficient to produce the disease? Are we to view it in the light of the efficient cause? This opinion has found many advocates, so far as regards the fever of the tropics,¹ some of whom regard heat as the sole cause of the fever, or of some forms of it; while others view it as acting conjointly, in that respect, with humidity. As regards the fever of Europe and this country, the same opinion has been warmly supported, as may be seen in the writings of Tommasini (§ 84), Robert (*Guide Sanitaire*, i. 17, 86, 145), Girardin (p. 43), *Rep. on Epid. of 1819 at New Orleans* (pp. 37-39), Diekson (*Eclectic Journal*, iv. 112; *Med. and Phys. Journ.*, iii. 265), Simons (pp. 17, 18), Bell (*Med. and Phys. Journ.*, xi. 282), and Jones (*Boston Med. Journ.*, ii. 378), who, like the authorities mentioned in relation to the West Indian fever, refer the disease, with more or less limitation, to heat alone, or associated with humidity.

But, however highly we may value these authorities, and whatever weight we may attach to the facts and reasonings adduced by them, it is impossible to deny that the opinion in question is open to very serious, indeed insuperable objections. That in the West Indies, or other places within the yellow fever zone, that form of disease denominated seasoning or climate fever, and the inflammatory remittent, or eausus, may be due at times, or usually, solely to the morbid action of heat, or of heat associated with simple moisture, is a point I am not here called upon or disposed to examine; for, after all, it is, to me, doubtful that that fever, though having some features in common with the yellow fever, deserves, any more than the inflammatory and bilious remittents of this country, to be regarded as identical with that disease, or that it is usually the result of the agent in question. Our business is with the true yellow fever, or that form of it which is regarded as such here, as also by the majority of writers elsewhere. Now, in reference to this disease, it is difficult to admit the propriety of ascribing it to the action of heat alone, unaided by a more efficient agent, when we take the following facts and circumstances into consideration:—

A. In some countries as hot as, and hotter than, those subject to the yellow fever—where the average tropical heat is of longer duration—the disease has never been known to occur; while in others it has only done so occasionally and to a limited extent. The summer heat of the southern portion of the great desert is very great; but, as Dr. Drake remarks, those who traverse it, and keep at a distance from its watereourses, pass the season unaffected. The same writer calls attention to the fact that the sandy banks of Pensacola Bay,

¹ Towne p. 11; Poissonnier, p. 50; Blane, *Dis. of Seamen*, pp. 403, 607; Vatable, p. 330; Bertaud, p. 7; Bourdon, p. 7; Veitch, pp. 127, 141; Béguerie, p. 21; Hosack, *Practice*, p. 390; Caillot, p. 117; Madrid, pt. ii. p. 1; Davidson, *Rep.*, viii. 248; Stevens, pp. 195, 213; Lefort, *Arch. Gén.*, xiii. 318, *Ib.*, *De la Contagion, &c.*, p. 12; Wallace, *Edinb. Journ.*, xlv. 276; Rochoux, p. 374; Moseley, p. 434; Peixotto, *N. Journ.*, i. 406; Catel, pp. 7, 8; Gilbert, pp. 67-70; Chambolle, *Broussais's Journ.*, xiii. 199; Monson, p. 21; Dickinson, pp. 6, 8, 58, 84, 110.

from its entrance up to the town of Pensacola, suffer but little; while at the head of the bay, where extensive alluvial deposits have been made, fever has been so constant and fatal as to prevent permanent settlement. Yet the temperature of both localities is the same, for they are but ten miles apart (i. 53).

In some localities, as the Antilles generally, fevers are said to be now more rife than they were formerly, though the temperature was not lower then than it is at the present day. The savannas or natural prairies of French Guiana, the arid deserts of Peru, and a large portion of Spanish Guiana, furnish us with examples of the sort.¹ Thermometrical observations made in the plains of Meta, situated on the east side of the Oriental Cordilleras and in the valley of the Magnaline on the west of the same ridge, exhibit great similarity of temperature, yet the former is remarkable for insalubrity, while the latter is free from fever.²

The range of the thermometer in Antigua and Barbadoes is rather higher than in Dominica, Tobago, Jamaica, or the Bahamas; yet we find that the troops in the latter stations suffer nearly three times as much as those in the former.³ The yellow fever does not prevail on Pigeon Island, but is of common occurrence in the town of Castries,⁴ though the temperature is much the same in both places. While common in some parts of the coast of Africa, the same disease is rare in Senegal.⁵ It is rare also in Cayenne, which is, to say the least, as hot as any place within the tropics.⁶ Panuco, and the plains of Coro and Cumana, and the coast of Coromandel and of Onixa, cited by Humboldt,⁷ as well as the coast of Malabar, the deserts of Arabia, and of the Diabakie,⁸ are rarely the seat of malarial fevers, especially of those of a malignant character; never of true yellow fever. We do not hear of yellow fever in Manilla, Mauritania, Antongil (Madagascar), Egypt, Bassora, Aleppo, Smyrna, and New Holland, where the heat exceeds, or at least equals, that of the West Indies or this country.⁹ The same may be said of Lima, Brazil, Peru,¹⁰ the savannas or natural prairies of French and a large portion of Spanish Guiana,¹¹ where, in spite of excessive and long-continued heat, this fever had, until lately, never or seldom prevailed. This fact had not escaped the attention of some of the older physicians. "The excessive heats of the torrid zone," says Dazille,¹² "which have been wrongly regarded as the remote cause (*première*) of the diseases of its inhabitants, and principally of those experienced by new-comers from Europe, do not act otherwise than in developing those causes, and imparting to them

¹ Leblond, pp. 6, 62-65.

² Boussingault, *Annales de Chimie*, lvii. 153.

³ Report on Mortality in British Army, p. 101.

⁴ Pugnet, p. 342.

⁵ Thevenot, p. 244.

⁶ Bajon, *Mém. sur Cayenne*, pp. 29, 59.

⁷ Volume i. 62.

⁸ Bonneau et Sulpici, *Recherches sur la Cont. de la Fièvre Jaune*, p. 16.

⁹ Pouillet, *Elem. de Physique*, iv. 637.

¹⁰ Vicaire, *Annales Maritimes*, Oct. 1831, pp. 298-9.

¹¹ Leblond, pp. 6, 62-65.

¹² *Malad. des Nègres*, p. 8; *Malad. des Climats Chauds*, p. 4, by the same.

more or less activity. Thus, the inhabitants of the island of St. Domingo, situated between the seventeenth and twentieth degrees of north latitude, have been, and continue still to be, subject to violent diseases; whilst those of Pondicherry, situated about the twelfth degree of the same latitude, are almost exempt from such diseases, though the heat is there much greater, owing to that city being situated nearer the line, and, besides, being built on a sandy soil, which concentrates and reflects the rays of the sun." Major Tullock remarks that sickness and mortality from fever vary considerably in stations where the mean temperature is nearly alike; and he instances the range of the thermometer in Antigua and Barbadoes, which is rather higher than in Dominica, Tobago, Jamaica, or the Bahamas. "Yet," he continues, "we find that the troops in the latter station suffer nearly three times as much as those in the former."¹ To this we may add, that the temperature of Sicily, where the true yellow fever does not prevail, ranges pretty steadily, in the shade, from June to September, between eighty-two and eighty-six degrees;² and Dr. John Wilson, in his excellent work on the yellow fever, remarks that if excessive heat could alone produce the disease, it ought to do so in Canada and Russia, where, after a cold winter, the heat suddenly becomes equal to that of the tropics, and continues to that pitch during many weeks (pp. 66, 67). But it is not necessary to pursue this subject, for this want of necessary connection, as cause and effect, between high atmospherical heat and fever—common autumnal and yellow—has been pointed out by a large number of writers on these diseases, as they show themselves in various parts of the globe.³

The yellow fever, it has been remarked by M. Vicaire, who has given an interesting account of the geographical limits of the disease, does not indeed propagate itself in the southern hemisphere. "The equator seems to offer an insurmountable barrier to the pestilence (*fléau*), and yet all the physical constitutions of the air and soil proper for its development are in some sort combined at Rio Janeiro (Brazil), at Guilca and Arica (Peru), and in several southern portions of the African and Asiatic coasts. This exemption," M. Vicaire adds, "is not attributable to greater heat in the northern hemisphere, for Brazil and Peru are much hotter than the United States of America."⁴ Since the date of M. V.'s publication, the fever has appeared and prevailed epidemically in both Brazil and Peru. But the occurrence cannot be attributed to a high degree of atmospheric heat, inasmuch as the accession of the disease was not preceded by or accompanied with any notable increase of temperature,

¹ Rep., p. 101.

² Boyle, Edinb. Journ., viii. 178.

³ Copland, iii. 151; Bryson, p. 197; Warren, p. 8; Dariste, p. 32; Osgood, p. 18; Good, ii. 168; Chisholm, ii. 264; Dalmas, p. 38; Bally, p. 327; Gillkrest, p. 279; Musgrave, Med.-Chir. Trans., ix. 121; Imray, Edinb. Journ., liii. 92; Chalmers, i. 22; Lining, ii. 407; Townsend, p. 377; Hosack, ii. 29; Chervin, translation of Wilson on Fever of Gibraltar, p. 9; Sir J. Fellowes, p. 417; Hunter, p. 13; Ferguson, Med.-Chir. Trans., viii. 142; Evans, p. 45; Davy, Notes to Blair, p. 52; Drake, pp. 712-13.

⁴ Annales Maritimes, Oct. 1831, pp. 298-9. In the London Medical Gazette (vol. ix. 804) this is attributed to M. Sper, R. N. of France. In the work from which I have derived it, the essay is signed by M. Vicaire, D. M. Chir. Maj. de la Marine.

which always has reached, and sometimes has exceeded, the degree noticed in the West Indies. Guayaquil, situated a few degrees north, on the western coast of America, suffers from the yellow fever, and so does Panama; whilst Lima, in fourteen degrees of south latitude (corresponding in that respect with Martinique, which is fourteen degrees north), is healthy, and was, until the last few years, perfectly free from the disease.

B. The prolonged action of heat on the system is not necessary for the production of the disease, which often breaks out when the individual has been exposed but a few weeks, a few days, or even only a few hours to the atmosphere of an infected spot. To say nothing of new arrivars in tropical climates, who are seized with the disease immediately on landing, we may remind the reader of the facts mentioned by Humboldt (pp. 773-4) and others, of persons residing in the city of Mexico, or the mountainous parts of the country, where the oak and pine grow, and where the disease was never seen, and who are attacked after passing a short time—even a single night—at Vera Cruz. Many an individual residing in the country, or in some salubrious spot, has taken the fever after a visit of a few moments in an infected city. Facts of the kind have frequently been noticed in this and other cities of this country, as also in Spain and the West Indies. Dr. Wilson, who notices them, very properly remarks: "It will be observed that the inhabitant of Canada, as regards change from cold to heat, is in a very similar condition, when his summer succeeds his winter, to the native of the Orkneys, who migrates to Jamaica; while in the diurnal vicissitudes to which he is exposed, he is placed on a footing similar to the inhabitant of Upper Mexico, who descends in a day from the moderate temperature of his healthy habitation, where the thermometer indicates a medium heat of sixty degrees, to the burning soil of Vera Cruz, where, in summer, it seldom falls below ninety degrees" (pp. 66, 67).

So true is this, that some writers who attribute the disease to the effect of change of climate alone, without reference to other agencies, urge the necessity of a *sudden* change from a moderate or low to a high temperature, and not of the *long-continued* influence of atmospheric heat. "The reason," as Dr. Dickinson remarks, "why strangers, on their arrival in the East, suffer less from the same disease than new comers to the West, is in great part accounted for from the protracted period of the voyage." "A material difference has also been observed among West India arrivals, as they left Europe or North America in a cool season or during the summer months" (p. 58). Veitch also accounts in the same way for the greater exemption from fevers on the part of arrivars; and Humboldt, after mentioning the fact of persons living in the elevated inland sections of Mexico suffering more from the fever than strangers who arrive by sea, supposes this to arise from the former experiencing a more sudden change of temperature, and passing in a few days from a temperate region to the torrid zone (p. 773). Johnson also (pp. 78, 79, &c.), explains it in that way, calling attention to the fact that persons become gradually acclimatized before arriving, while those who arrive in the West Indies have not, from the shortness of the passage, that advantage.

However true the facts may be for which this explanation is given, they cannot be admitted to prove the exclusive agency of sudden change in producing the disease. Humboldt mentions that every care imaginable was taken to shield the young soldiers enlisted in the salubrious parts of Mexico from the air of Vera Cruz. They were kept some time at Jalapa, to "acclimatize them gradually to a more elevated temperature." They were conducted to Vera Cruz, and made to cross the arid plains of Antigua by night, so as to avoid the effects of exposure to the sun, and at Vera Cruz they were lodged in well-ventilated apartments; but all to no purpose.

The change of climate which the inhabitants of the country in the immediate vicinity of our cities, or of those of many of the West India Islands, experience when they visit infected districts, is so trifling as to deserve little attention; and nevertheless they take the fever when they pay such visits, and are exempt when they remain at home. Again, it is admitted, as a general rule, that individuals who arrive in yellow fever countries—the West Indies for example—require a residence of several years before they become safely acclimatized, unless they go through an attack of the disease, or have remained free during a severe epidemic. All others may be attacked two, three, or more years after their arrival. During that period, when the thermometer has not descended much below eighty degrees, they must have had a better opportunity to become accustomed to the action of heat alone than those who pass from Europe to India. If sudden change produced the disease, they should have been seized soon after their arrival, not several years after, when such a change would not be felt.

c. In countries subject to the yellow fever—where it prevails endemically, or at long intervals—seasons occur in which, though the temperature is as high as, or higher than, during sickly periods, the disease either does not show itself at all, or does so to a limited extent or in a mitigated form. Dr. Henry Warren, who wrote on the fever of Barbadoes more than a century ago, remarks: "Neither the alteration of the weather or winds, nor the different seasons of the year, have ever of themselves been able to produce this contagious disease among us. Many years (I may safely say seven or eight, or more, successively) have, to my certain knowledge, passed over, when *the sultry heats* and long, intolerable droughts of some, the almost incessant rains of others, or the tempestuous weather of many, and that from uncommon points of the compass too, must surely, in some degree or other, have given rise to such an epidemical malignity, if it could possibly be derived from such causes, and yet no footsteps of the fever did appear all that time" (p. 8). Remarks of similar import were made by Dariste (p. 32), Osgood (p. 18), Good (ii. 165), Chisholm (ii. 264), Dalmas (p. 38), Bally (p. 327), &c.

Prior to 1794 the yellow fever had not, for many years, prevailed in the city of Vera Cruz. In that year it assumed the character of a wide-spreading and fatal epidemic, and yet the mean temperature at that season and the next, was not so high by near one degree (centigrade) as it had been the two preceding years when the city was healthy—when vessels arrived in large number without being subjected to quarantine restrictions,

and brought many unacclimatized Europeans, and when the disease prevailed extensively at the Havana and other West India Islands.¹

In his account of the fever of Antigua, in 1817, Dr. Musgrave informs us that the heat was by no means unusually intense.² The epidemic of 1793, at Grenada, occurred under circumstances of like nature; for the season was little, if at all, warmer than it had been in the years 1784–5–6, when the island was healthy.³ A similar observation was made relative to the epidemic of Martinique, in 1838, when the heat was no higher than it had been in other seasons exempt from the disease.⁴

It has been before remarked, that the yellow fever is a rare disease at Cayenne. Bajon, already cited in reference to that subject, alludes to the epidemic visitations of 1763 and 1764. In 1802, the same disease appeared and prevailed extensively among the troops recently arrived from Europe, and other unacclimatized individuals.⁵ From that time to 1850 and 1851, when the disease again broke out in an epidemic form,⁶ the yellow fever was unknown in the colony; and yet the heat during the intervals remained the same, and troops, uninured to its morbid effects, annually arrived to renew the garrison. The same may be said of the coast of Brazil, where, since the epidemic of Pernambuco, in 1688 (the yellow fever nature, let it be said *en passant*, is somewhat problematical), the disease was not seen except sporadically, and that too very seldom. It broke out epidemically in 1850, although the temperature, during this long interval, did not differ considerably, if at all, from what it has been since.

The yellow fever, though frequently encountered in some parts of the western coast of Africa, is seldom seen in others where the range of the thermometer, from the Gambia on the north to Benguela on the south of the equator, is as high as, and probably higher than, in many portions of the yellow fever zone. In Senegal, it has but very seldom shown itself, having, so far as I can ascertain, prevailed but twice in very many years at Goree, though the heat was not greater then than at other periods.⁷ At Aseension, at Sierra Leone, at Fernando Po, and at the Gambia, it has only appeared occasionally; and we all know that at Boa Vista, where it committed such ravages in 1845, it had not been observed to any extent for many a long year, the heat, nevertheless, continuing much the same.

In 1838, when the fever broke out in Dominica after an almost complete exemption of seventeen years, the heat was not any greater than it had been at any time during that period.⁸ The mortality among the troops in the West Indies, as it appears from the tabular statements in the medical reports, is sometimes in one year twenty times as high as in another, without any perceptible difference in the range of temperature. "This fact has already attracted the notice of some medical authors, who, in treating of

¹ Humboldt, p. 785.

² Med.-Ch. Tr., ix. 121.

³ Chisholm, i. 92.

⁴ Rufz, Chervin's Rept., p. 30.

⁵ Leblond, p. 227.

⁶ Report on that fever, Ann. Mar., N. S., viii. 153. 1852.

⁷ Thevenot, pp. 45, 244, 254; Chev , Relation de deux Epidemics, &c.

⁸ Imray, Edin. J., liii. 92.

yellow fever, adduce instances of various epidemics, both within and beyond the tropics, during which the temperature was not above the average, and was sometimes even a little below it, and ordinarily when the existence of a high temperature was not attended with the prevalence of fever."¹

It is to be remarked, also, that in the West Indies instances have occurred when epidemic fever has made its appearance, and raged with the utmost violence during the winter months—a circumstance not likely to have taken place had that disease owed its origin to increased temperature. It will be found that the epidemics of Grenada, in 1793, and of St. Christopher, in 1812—two of the most fatal that ever appeared—commenced, the former in March, and the latter in February, and continued with unabated violence during the whole of the cool season.²

The range of the thermometer at Charleston, in 1752, appears to have been extremely high, and to have continued so a long while. The mercury often rose above the 90th degree throughout the months of May, June, July, and August; and for twenty successive days—excepting three in June and July—the temperature of the shaded air varied between the 90th and the 100th degree, and sometimes it must have been 30 degrees warmer in the open sunshine, to which great numbers of people were daily exposed for many hours together. And yet, as Dr. Chalmers, from whose work I have extracted the above statement, remarks, a more healthy season had never been known than this, so long as the weather continued steadily warm and fair.³ Lining, writing in 1752, says that, within the preceding twenty-five years, the disease was only four times epidemical in Charleston, “though none of those years were warmer—and some of them less so—than the summer and autumn were in several other years in which there was not one instance of the fever.”⁴ Prior to the epidemic of 1817 at Natchez, the heat, during the summer months, attained the usual tropical degree, and yet those cities remained free from the disease. In 1824, the thermometer, during June and July, gave at 3 P. M. an average of near 90° (89.4). In August, it gave a mean of 87.2. “The mean temperature of Natchez in summer, in 1819,” says Dr. Drake, “was 78.66, which was 1.03 higher than the mean of the nine preceding years, but it was less by 2.25 than the mean of three years just before, when the fever did not prevail; less by 3.03 than one of those years” (ii. 264). In New Orleans, the average temperature of the summer months during the epidemic year of 1847 was somewhat higher than usual, being 87.7; but we find at the same time that the averages in 1819, 1833, 1841, 1853—all epidemic seasons—were 83.40, 79.56, 83.9, and 80.07, while in 1840 and 1842, when there was no yellow fever, the averages were 84.4 and 82.3.⁵

It results also from thermometrical observations, conducted at New York by W. Leigh, during a period of thirty-four years—from 1789 to August

¹ Tullock's Rept., p. 101.

² Ibid.

³ Chalmers, *Climate of S. C.*, i. 22.

⁴ *Essays and Obs.*, ii. 407.

⁵ Rept. on Fev. of 1819, p. 6; Barton, *Epid. of 1833*, p. 9; Fenner, *Southern Reports*, i. 29, 30; Rept. of San. Commis. 1853, p. 270.

1822—that in some years the yellow fever did not appear, though the mass of heat was greater than in other seasons when the city suffered from the pestilence. Thus, in 1793 and 1820, the mass of heat was greater than in any yellow fever season, except two. In 1809, the fever prevailed at Brooklyn, opposite to New York, although the mass of heat was less than any of the thirty-four years, except one.¹ Occurrences of a recent date at Charleston lead to the same conclusion, for, if we compare the degree of heat during the months of July, August, and September, in 1846, 1847, 1848, 1850, and 1851, when the fever did not prevail with that of 1849 and 1852, when it spread epidemically, we shall see that the averages were often higher in the former than the latter category:—

NON-SICKLY SEASONS.

	1846.	1847.	1848.	1850.	1851.	1853.
July	79	78	81.09	83.74	82.84	81.78
August . . .	80.96	79.45	79.25	82.64	82.19	80.28
September . .	79.5	75.33	75.66	77.33	74.23	77.33

SICKLY SEASONS.

	1849.	1852.	1854.
July	78.42	81.74	81.35
August	80.54	79.16	82.14
September	74.23	75.33	78.53

Dr. P. Wilson, in his account of the fever of Gibraltar² in 1828, states, that the summer of that year preceding the development of the disease was a very moderate one, and Gillkrest says expressly the heat was not greater (*Cycl.*, ii. 279). The summers of 1799 and 1803, at Cadiz, when the city was perfectly healthy, were hotter than was that of 1800, when the fever raged extensively. The same season in 1789 was exactly, and those of 1791, 1792, and 1794, nearly, as hot as in 1800, and nevertheless they also remained free from the pestilence.³ The same remark applies to Barcelona, where the heat in 1821 was more moderate than it had been during the non-sickly summer of 1820.⁴

¹ Townsend, p. 377; Hosack, Med. Ep., ii. 29; Med. and Phil. Reg., i. 293; ii. 220; iv. 296.

The average heat of June and July, in these tables, during the yellow fever seasons, is generally, if not usually, below 79°. It is doubtful whether this degree should be assumed as the most probable one, though Dr. Townsend thinks it ought to be so considered. The average for 1822, as given by that physician in his account of the epidemic of that year, were furnished from observations made at the New York City Hospital, and are higher by eight degrees than those marked in Mr. Leigh's tables. Now, if we add eight degrees to all the averages of sickly seasons, as given by Mr. L., we shall find that the mean temperature exceeded 79°. Nevertheless, this would not affect the conclusions mentioned in the text; for, by applying the same process to the healthy seasons, we would find, that in some of them the mean temperature would not cease to be higher. In any way, whether we adopt the averages given by Mr. L., or make the addition in question, we derive the proof that a mean temperature of 79° is not necessarily followed by yellow fever.

² Chervin's Tr., p. 9. The summer of 1821 less warm than 1820 at Barcelona, Pariset, p. 478.

³ Fellowes, p. 417, from Arejula.

⁴ Pariset, p. 478.

The following statements, derived from a report recently published by Mr. Lorin Blodget, who had charge of the meteorological department of the Smithsonian Institution at Washington, may be appropriately placed here.¹

Few summers in this country have been characterized by greater heat than the one we passed through in 1853. The excess prevailed from New Hampshire to Savannah in Georgia. The first general high temperatures of the season occurred on the 3d to the 5th of June, extending from Montreal to Florida, but sparing the West, generally. At the South, its maximum, from Chapel Hill, N. C., to Savannah, Ga., was 92° ; and at the North, from Montreal to New York, 83° . From the 14th to the 18th, the heat was excessive and general. It commenced at the extreme west on the 12th and 13th, and did not extend beyond Camden, S. C. It rose from 90° to 94° in Ohio, Kentucky, and westward in the same latitudes. From the 20th to the 23d, there was another general excess of temperature—less than the preceding in the extreme north, and with a considerable fall there on the 22d; but quite unusual and long-continued at almost every other part of the country. The maximum varied from the 20th to the 23d, and ranged from 90° to 97° . The maximum of 95° , was probably general from New York to Savannah on the 23d. Lastly, a most extraordinary extreme of heat occurred on the 29th and 30th. The extreme was central in the latitude of Washington, and was limited at Savannah, on the south, and Burlington, Vt., on the north. It attained 96° to 98° in Tennessee, Kentucky, and Southern Ohio, and 77.5° to 102° at Washington, and Eastern Virginia, and North Carolina. This is without any parallel in the records of temperature at Washington, and is several degrees above any recorded temperature at New Orleans, Mobile, and Savannah.

The temperature of July was also high, and slightly above the normal mean in most parts of the United States. The excessive heat of the last days of June was prolonged through the 1st and 2d of July at 94° in Virginia, at the south, and the range was generally high in this city and south, where it was again at 92° to 84° . The temperature was at or above 90° after the middle of the month only in the central part of Georgia and Alabama, and west in the latitude of Washington to Texas, for two or three days about the 20th, and again about the close of the month.

In August, a period of general excessive heat occurred, beginning, as usual, at the west, and reaching 90° in several places on the 7th and 8th. The maxima in Illinois and the adjacent States were 70° to 90° from the 8th to the 13th; in Ohio and Kentucky, nearly the same; and passing eastward a little later through Pennsylvania, the district of greatest excess was central at New York, from the 12th to the 14th. The temperature at one place south reached 90° . Later in the month, from the 25th to the 31st, the heat was unusually great in the southwest, Texas, the Cherokee Territory, and Mississippi, with an extraordinary reverse in Iowa and the adjoining States.

Indeed, the summer of 1853 was remarkable for its climatic conditions,

¹ On the Climatic Conditions of the Summer of 1853, most directly affecting its sanitary character.—*New York Journ. of Med.*, Nov. 1853, p. 313, &c.

and the extreme of temperature was much more striking than usual. Yet, though epidemics of yellow fever, which require a continuance of high temperature, prevailed in more places than has been the case in this country for the last thirty years, many other places that have suffered from the disease on former occasions, as Savannah, Baltimore, and New York, for example, and where the temperature was unusually high, and even exceeded that of Philadelphia, escaped.

In the latter city, too, though the mean temperature of the three summer months was 76.76, or nearly four above the common average, such a heat could not be considered as alone the cause of the disease from which we suffered; for it has been exceeded, according to the records of the last sixty-four years, on two occasions—in 1798, when the fever prevailed, and in 1838, when the city was entirely free from it. It must be added that, notwithstanding the unusual heat of the season, we do not find that ordinary autumnal fevers were anywhere more rife, or that they extended over a wider expanse of country than usual; indeed, many localities remained healthier than in cooler seasons.

While such were the results obtained in the places mentioned, the sanitary condition of New Orleans in 1853, when the fever prevailed extensively and with great malignity, completely disproves the idea of the sole agency of heat in the production of yellow fever. June gave a maximum of 91, a minimum of 73, and an average (out doors) of 80.73. July gave 89, 71, 79, 88; August 91, 72, 81.25; and September 86, 60, and 76.23.¹ The fourth week of June gave, according to the statement of Mr. Blodget, a maximum heat from New York to Savannah of 95°. The corresponding week in New Orleans, when the disease began to spread, gave, according to Lillie's tables, a mean maximum of 92°, and throughout the whole epidemic the average never, for any week, equalled that of the above-mentioned zone, where little or no yellow fever prevailed. "The average maximum temperature of the week ending August 26, in New Orleans, was 91°, while the mortality was the greatest, amounting to 1.667, or more than 238 as the average per day, the temperature averaging one degree more than that of the week ending September 15, during which the mortality was only 411, averaging less than 59 per day. The average maximum temperature of the week ending October 22, was 82°, two degrees more than that of the preceding week, though the number of deaths did not differ more than three for that period."² To this it may be added, that many places, where yellow fever did not prevail, gave a maximum temperature much higher than others where it did so. The last week in June, in New Orleans, as we have seen, gave an average maxima of 92°. Now, on and about the same day, the maxima temperatures were—at Alexandria, Va., 95°; Knoxville, Tenn., 94.4°; Oberlin, Ohio, 95°; Baltimore, 92.3°; Camden, S. C., 97.6°; Sparta, Ga., 97°; Eutaw, Ala., 101°; Lebanon, Tenn., 95.90°; New Harmony, Ind., 97.5°; Bloomfield, N. Y., 99.5°; Brooklyn, Michigan, 97°; Poultney, Iowa, 97°. "On the other hand,

¹ Barton, Rept. of Sanitary Commission (1853), pp. 232-237.

² Dowler, p. 40.

many southern towns were comparatively cool—those which escaped as well as those which suffered from yellow fever.”¹

The summer heat—June, July, August—in places where the yellow fever prevailed, will be found to have closely corresponded in 1853, with that of other localities, within and without the sickly zone, where the disease did not show itself.

I have said that in countries subject to the yellow fever, whether endemically or epidemically at distant intervals, some localities are more or less severely visited; while neighbouring cities—or districts and situations—even at no considerable distance, where the temperature is equally high, are exempted—these at times becoming affected while the others remain healthy. “The heat of tropical climates,” says Dr. Hunter (p. 13), “though generally reported the cause of their unhealthiness, will not alone produce fevers, as is strongly exemplified in those living on board of ships, who remain free from fevers; and also in the inhabitants of certain dry sandy spots along the coast, in which the heat is uncommonly great, yet the situations are healthy, as Fort Augusta, Port Royal, and others.” In 1816, while the fever visited Guadaloupe, Barbadoes, and Antigua, it spared the equally or more unhealthy countries of Trinidad, Tobago, St. Lucia,² and when introduced into St. Vincent it could not remain there. In other years, the reverse has been the case. And yet, the temperature in such places—in those affected and those spared—is much the same. When the fever has prevailed in this or in any other of our cities, other places whose temperature was not at the time lower, remained free from the pestilence.

Dr. Ramsay says, that the degree of heat in Charleston is considered less than in the interior western country. In the summer of 1808, at Columbia, it was frequently at 96 and 97, and sometimes at 98; while at Charleston it did not exceed 91.

Furthermore, in all our pestilences, the disease, as remarked—and the same holds good in most, if not all places—is circumscribed in its sphere of action. Some street, some portion of the city is infected—such sickly parts varying in different visitations—while the rest, where the temperature is equally high, remains free from the disease. The history offered in the preceding chapters of our various epidemics, proves this beyond all doubt. The same observation has been made in all parts of the United States, in Europe, and to a certain extent in the West Indies, and when the fever breaks out in a ship and spares others close by, or when it ceases in a vessel on the latter’s shifting its position, we cannot presume that a difference of temperature can have had anything to do with the difference of the results observed.

It is to be observed also, that were the localities thus infected hotter than others that remain healthy, the circumstance could not be adduced in proof of heat being the efficient cause of the disease, for individuals who are unaffected so long as they remain aloof from the sickly spot, and take it by visiting the latter, are not so from the greater heat they there encounter, inas-

¹ Dowler, p. 41.

² Ferguson, Med.-Ch. Tr., viii. 142.

much as the risk of infection is not as great in the hottest as in the coolest part of the twenty-four hours. To this subject I shall return by and by.

Were the heat the principal or efficient cause of the disease, we ought to find the prevalence and severity of the latter, during a sickly season, to be proportioned to the elevation of the temperature. So far from this being the case, it is a well known fact, that when our cities are visited epidemically with the yellow fever, the disease is not always more particularly rife during the hottest month of the season, but some time after, when the average temperature has lowered in a notable degree; and that so far from its diminishing as the weather becomes cooler, cases are often multiplied daily till the temperature reaches the freezing point, when the occurrence of new cases ceases at once. Take the epidemic of 1793 as an example. The deaths in August, when the mean temperature at midday was 82.55 (69.03 in the morning), amounted to 325; in September, with a mean heat of 76.48 (60.93 in the morning), the mortality reached 1,442; and in October, with a temperature of only 62.57 (44.06 in the morning), the deaths fell but little short of 2,000.

Cool weather, therefore, when the disease is fairly established, so far from diminishing its extension, actually increases it. These facts have been noticed in our southern States as well as here, and at Barcelona in 1821.¹ Humboldt informs us that the disease is more prevalent at the commencement and close of the rainy season (p. 765). The former takes place in June, when the mean temperature is 27.5 (Cent.), and the latter in September, with the thermometer at 27.4. In July and August, when the thermometer is as high, and in May when it is higher, the disease is not so prevalent, and the latter continues in October, though the heat is already reduced (pp. 382, 765, 767).

Williamson (i. 210, 211) tells us that, in Jamaica, the fever does not produce the greatest havoc during the hot season: "On the contrary, the fall of the year, when the air is colder, and from the north—when vegetation is declining, and probably a corresponding decline of the animal creation—then the formidable endemic diseases of Europeans more commonly do discover themselves." Similar statements are made by Pinckard.²

It may, moreover, be mentioned that long-continued and excessive heat has occasionally put a stop to the epidemical extension of febrile diseases; at any rate, that they cease long before the cessation of hot weather—when, indeed, the temperature has reached its highest point. We know that such is the case in relation to the Egyptian plague, and that it has occurred also at Dantzic, Toulon, and Stockholm.³ "There broke out a plague in Venice," says Matth. Villani, an eye-witness (*Lib. i. Historiar.*), "in the year 1348, in the month of Mareh. It was at its height in April and May. It began to decline in July, and ended in August; so that a plague will end in hot

¹ Lining, *op. cit.*, ii. 410; Tullock's Report on Sickness, &c. in the Mediterranean Station, p. 65; Emlen, *N. O. Journ.*, v. 328; Carey, p. 71; Rochoux, p. 110; Pariset, p. 475; Williamson, *Med. and Miscel. Obs. on West Indies*, i. 210, 211; Pinckard, *Notes on the West Indies*, ii. 485; R. Jackson, *Sketch*, pp. 9, 10, 11.

² *Notes on the West Indies*, ii. 485; Jackson, *Sketch*, pp. 9, 10, 11.

³ *A Treatise on the Plague and Pestilential Fever*, pp. 10, 11. Lond. 1751.

weather.”¹ The occurrence has been noted, in reference to the yellow fever, by Rush (iv. 155), Devcze (p. 117), Emlen (*op. cit.*, p. 329), and others.

This could not be the case did the disease owe its origin to the action of a high atmospheric temperature on the systems of those exposed to it. The famous fever of Grenada, in 1793, which broke out in March, with a medium heat of 83°, disappeared about the middle of September, when the mercury stood at 86°. ² The same fact was observed in this city in 1803, as also in Mobile and New Orleans in 1848, in the latter city in 1853, and in Leghorn in 1804.

Heat, in a word, may be, and is, to a certain extent, requisite to promote the formation of other agencies; it is doubtless essential to farther the evolution of the poison which gives rise to the disease, as everything connected with the appearance of the latter proves—its production in hot weather—its absence in cold, and its disappearance on the accession of frost. Heat may act, besides, as an exciting cause; but alone, it cannot occasion the peculiar form of fever under consideration. It requires materials to act upon, and from which, aided by other influences, it may extricate an efficient cause. That high and long-continued heat may, and does often, by its action on individuals unaccustomed to its effects, produce fever, is doubtless true, and perfectly well known to all physicians acquainted with the complaints of hot climates; but the disease thus produced is different from true malarial fevers, and especially from genuine yellow fever. More frequently it gives rise to other groups of morbid phenomena more or less distressing, violent, and dangerous—cerebral inflammations, or congestions, visceral inflammations, inflammatory angiotenic fevers; but these phenomena, I repeat, are in no way analogous to those characterizing the regular and specific pyrexia under consideration.

CHAPTER VI.

CIRCUMFUSA, CONTINUED.—LIGHT—ELECTRICITY—ATMOSPHERIC PRESSURE.

THE electrical and luminous fluids are, as every one knows, distributed differently, as respects quantity, in various sections of the globe. In the torrid zone, where the inclination of the terrestrial axis is always equal and parallel, these fluids are abundantly supplied; at the poles, on the other hand, where a contrary inclination exists, they are deficient in quantity, while temperate regions are characterized by a moderate degree of both, except during those seasons in which solar action is most intense. Such being the extended diffusion of those elements in countries and seasons most liable to

¹ A Treatise on the Plague and Pestilential Fever, pp. 10, 11. Lond. 1751.

² Chisholm, i. 294.

the yellow fever, it may not be uninteresting to inquire how far they may exercise an agency in the production of the disease.

A. *Light*.—That light, especially when combined with heat, produces a potent action on organized beings, whether vegetable or animal, is a fact too well attested to need an enlarged notice in this place. Applied in a moderate quantity, it conduces to the healthful performance of the most important functions. On the other hand, if carried beyond certain bounds, it may prove highly injurious. By Rumford it was thought that the effects produced on bodies exposed to the action of the solar rays were due exclusively to the heat conveyed by these, and not to the light which attends; but however true this may be in many instances, there can be little doubt that light alone, and independent of heat, exercises the powerful influence to which I have alluded, and hence, that in those cases where the effects have been obtained from solar rays, they may, in part, be ascribed to the agency of the former. We all know how influential are the solar beams in modifying the physical phenomena which make up the character of climate.¹ They produce, in the mineral kingdom, combinations which cannot be brought about at the same low temperature without them, and unassisted by light. Plants would scarcely produce green matter. To the same agency we must refer, not only the etiolation of such plants, but the vigour of their growth. Indeed, without light, independently of its accompanying heat, there would scarcely exist a trace of vegetation.

In the animal creation, light is necessary to the full development of many species and to the maintenance of health and life.² In all, it acts as a healthful stimulus to the skin and to the organs of vision, and, through the instrumentality of these, to the system at large; whilst it promotes, even in the human species, a favourable temperature of body, as exemplified in the natives of countries where nudity, and the exposure of the whole surface of the body, are not injurious to health.³ On the other hand, a privation of that stimulus is incompatible with health and a proper and vigorous development of body—witness the paleness and feebleness of persons excluded from its influence, the stunted condition of the body, the incomplete development of certain parts, those deviations of form noticed in children of a scrofulous constitution compelled to reside in confined and dark situations,⁴ as well as the diminished excitability of the frame, and the frequency and feebleness of pulse, noticed in gorges of mountains or deep dells.

Nor is it less certain that, when in excess, light acts as a strong stimulus, and produces the effect of over-excitement—giving rise, unaided by other causes, to diseased actions in various tissues and organs, or aiding in the elicitation of morbid derangements in the system at large. In illustration of this stimulating agency, it will be sufficient to remind the reader of the fact that the symptoms of *coup de soleil*—the usual effect of a burning sun—have

¹ Forry, *Climate of the United States*, p. 63.

² Edwards, *Influence of Physical Agents*, p. 125, Am. ed.

³ Edwards, p. 127.

⁴ Dunglison, *Human Health, or the Influence of Atmosphere, &c.*, p. 52.

been known to be produced by a strong daylight, when the sun had little force, and could not, therefore, give rise to the result. We know, too, that the impression of even a moderate light upon the organ of vision produces, in several acute diseases, a general exacerbation of symptoms.¹ So evident, indeed, is this stimulus, that there is every reason to believe that light is largely associated with heat in the production of those sudden deaths which occur in hot climates, and are usually ascribed to the latter alone.

Such being the exciting effects of light, it is natural to conclude that they will be more marked in situations characterized by a large share of that element than in others differently circumstanced. We may therefore expect to observe them in the West Indies, and other parts of tropical regions where the fluid is so abundantly supplied that, in clear weather, the stars emit rays of sufficient vividness to enable travellers to see their way with perfect ease; where the finest print can be read by moonlight, and where, during the day, the glare is overpowering to the eyes, and imparts to the atmosphere a glowing and fiery appearance, attention to which has been called in all descriptions of those regions.

To the natives of such countries, and to the acclimatized and creolized inhabitants, this abundant supply of light, and the stimulation accruing therefrom, may prove innocuous. Their systems have become habituated to the impression of that agent, and they can support its effects with impunity. But to new comers the case must be different; and we may readily presume that, for some time after their arrival, and until their constitutions have undergone a suitable change from the operation of the influences mentioned in a former chapter, they experience from this excess of light a degree of stimulation incompatible with perfect health. In this country, and in other portions of temperate regions, as already stated, the supply of light is not, by far, so abundant throughout the year; but, during the summer months, it is still sufficiently great to act as a strong stimulus to the economy, and the effects resulting therefrom are of a character similar to those mentioned. Nevertheless, disposed as we may be to view light in tropical regions, and, at some seasons, in temperate climates, as a fruitful source of disease—in the former among the unacclimatized, in the latter among every one—it is impossible to ascribe to it more than an accessory or secondary agency in the production of the yellow fever, whether in the West Indies or elsewhere. It may contribute to place the individual exposed to its influence in a condition favourable to receive the impression of the efficient cause of the disease, or it may hasten and excite an attack; but it cannot be regarded as the efficient or remote cause itself.

The abundant supply of light in other countries—in Africa, the East Indies, and South America—where the yellow fever has, notwithstanding, never existed; its greater diffusion there and elsewhere than in some of the southern portions of temperate climates where that disease has prevailed, and continues still to prevail almost annually; its still greater diffusion there than even in the hot seasons of more northern regions, where that fever occasion-

¹ Edwards, pp. 127-8.

ally reigns to a great extent; and more especially the fact that, in the West Indies, as everywhere else, the supply of light during healthy seasons is as great as during the prevalence of the disease, and that when the latter occurs among us, it does so most extensively and severely during the month of September, when the sun has lost most of its power, and the light has sensibly diminished, all go to prove the impropriety of assigning to the latter a greater sphere of agency than has just been mentioned.

B. *Electricity*.—As regards the degree of agency exercised by the electrical fluid pervading the atmosphere, in the production of the yellow fever, little is as yet known. That its power, as a meteorological cause, is exceedingly limited—that it may be altogether left out of the account as productive of meteorological effects of importance, and that whatever part it plays in phenomena of the kind is rather that of an effect than a cause; and again, that the influence its development may have on organic life has been greatly over-rated, are propositions advanced by respectable authorities.¹ But, I have neither the time nor the inclination to investigate the subject in all its bearings, and must content myself with the remark that, from the conviction to which every day's experience leads us respecting the potency of the action of that fluid in the natural world, we cannot doubt that, directly or indirectly, atmospheric electricity plays an important part in bringing about, or influencing the development or diffusion of the yellow fever, and other diseases of kindred character. If, while recollecting the almost universal agency of electricity over the laws of inorganic matter, we take into consideration the diffusion of this fluid in the atmosphere, both in its tranquil or disturbed states; if we bear in mind the close analogy or correlation existing between it and the cause presiding over certain phenomena of innervation—an analogy for the discovery and elucidation of which we are indebted to Wilson, Philip, Breschet, Prevost, Dumas,² and, as it would appear, confirmed by later experimenters, Longet, Person, Matteucci, Dubois-Raymond; if, besides, we consider that the nervous system derives from the atmosphere a portion, at least, of the electrical properties requisite for the performance of its functions, and if, with these facts before us, we recollect that electricity is elicited in the processes of the living economy—increased under peculiar circumstances—and modified by cold, lassitude, or sudden motion, we shall discover, in the absence of other and more positive proofs, a strong argument in favour of the conclusion, that from the excess, paucity, or difference of condition of atmospheric electricity, will result certain modifications, for good or for evil, in the play of the functions. We may conclude, too, that from the same influences will result various changes in the ambient air and in the chemical combinations of surrounding objects calculated to impart to these deleterious properties, or to aid in the diffusion of morbid causes. This will appear the more natural when it is remembered that a certain proportion of electricity is indispensable to

¹ Edinb. Rev., Jan. 1848, pp. 218, 224.

² Arch. Génér., Aug. 1823, p. 185; Journ. de Physiol., Oct. 1823.

our existence, and that living bodies exhibit all the conditions which, in inorganic substances are attended with the development of electric phenomena, evaporation of liquids, changes in the state of aggregation and alterations of composition, as in assimilation, respiration, nutrition, and secretion. But we are not left, on this subject, to mere inferences. Facts may be appealed to. Every physician knows the effects of electricity on the animal economy; its influence in promoting the play of the functions; the modification it imparts to the general and capillary circulation, to muscular action, animal heat, absorption, endosmosis, and exosmosis; its agency in producing certain nervous diseases or functional disturbances, &c. We all know this, and have, besides, no reason to reject the opinion of Humboldt, that the depressing action of certain winds—the Catia, the Chamsin, Harmattan, and Sirocco—is due to the probable abstraction by them of the electricity of our bodies. We may even go so far as to view favourably his conjecture respecting the etiology of cretinism and bronchocele; everything conspiring, in places where these diseases are endemic, to diminish the quantity of electricity, and thereby lower the tone of the vital forces. To the same cause the effects of the east wind, in some parts of this country, have been ascribed;¹ and we may believe that such is the case elsewhere on this side of the Atlantic and in Europe, in some parts of which the Levanter produces the same effect as the Chamsin. So evident, indeed, are the effects mentioned, and their connection with electrical states of atmosphere, that while unwilling to believe with Foster² that it is not the heat, nor cold, nor dampness, nor draughts of the air, which are chiefly concerned in producing disorders, nor the sudden transitions from one to another of those conditions, but some inexplicable peculiarity in its electric state, we cannot but admit that, in the instances referred to, and others of the same kind, the effects must be sought in the influences assigned, and not, as has been done, in the hygrometrical changes which accompany alterations of weather, though these, as well as barometrical and thermometrical variations, contribute to the result. (*Dunghison*, p. 56.)

But, while all must recognize the modifying influence of atmospheric electricity, and its agency in the production of disease, some difference of opinion exists as to whether the injury it produces in that respect depends on its excess or its paucity. According to some, there is reason to believe that the theory which ascribes to malarial combinations the development of electrical phenomena of sufficient energy to affect the system injuriously is not destitute of truth; and we know there are not wanting those who think, with Pallas, that, by their geographical constitution, and their influence upon the human organization, marshes present the clearest analogy to the galvanic battery. We know, too, that some are disposed to adopt the views of Eisenmann and Hirsch,³ that the main cause of the modifications of atmospheric electricity is to be sought in the dispositions of the several strata of the globe; the deleterious effects increasing in proportion to the quantity of saline and organic

¹ Barton, Fever of N. O. in 1833, p. 9.

² Researches about Atmospheric Phenomena, pp. 165, 166.

³ Hirsch, Rech. sur l'Étiologie de la Fièvre Interm., Gaz. Méd., 1850, p. 640.

matter contained in the water subjected to the process of evaporation during which electrical phenomena are necessarily developed.¹

Not very different are the views entertained by a distinguished medical philosopher, Sir James Murray,² who considers that what is called malaria is not bad air at all, as the name implies, and that marsh miasms, gases, effluvia of vegeto-animal matters, or putrid emanations are not (as commonly supposed) the exciting causes of agues or other diseases, called malarious. But he holds that in spots long noted for insalubrity, there is emitted from the earth's surface an untoward emanation of electro-galvanism, set free by causes operating within the earth of that locality, either by the juxtaposition of strata of dissimilar materials, acting electrically upon each other, or by the infiltration of subterranean streams, or mineral waters, by internal heat, and consequent liberation of steam electricity, or by some other agents operating on materials contained in the ground, analogous to the manner in which we operate upon artificial materials in a galvanic apparatus.

In some of the thousand ways in which electro-galvanism is produced in the earth, or air, its undue influence (under certain circumstances), disturbs, as he thinks, the natural electricity of human beings, particularly when recumbent, in contact with the ground. Again: he maintains that this disturbance, either in the relative quantity of electricity itself, or in the due proportion of the positive or negative (fluids), alters the condition and functions of

¹ 1. The greater number of diseases, and more particularly those which involve the nervous system (belonging to the class of neuroses), are caused by the excessive influence of general electricity, of which stormy clouds and marshy localities are the most abundant sources.

2. Marshes, by their geographical constitution, and their effects on the animal economy, offer the greatest analogy to the galvanic pile. Indeed, their noxious action is by so much the more formidable as the water which exists in them holds in solution organic or saline matters; and this explains why salt-water marshes, and those near the sea-coast, are most especially injurious to the health. The drying up of marshes, or their submersion, presents us with the same conditions as when a galvanic pile is deprived of its liquid, or totally immersed; in either way its action being rendered insignificant, or being quite annulled.

3. The works of physicians and physiologists have demonstrated that electricity produced by machines, has a special action on the nervous system. Experience and rigorous observation of facts prove that the maladies which are developed in the vicinity of marshes are always, primarily, of a nervous type; and that when they become inflammatory, it is by the reaction of the nervous system on the heart and entire vascular system, whence arise, consecutively, local or general inflammations.

4. Nervous maladies and intermittent fevers being occasioned, not by the action of a miasm, which has never been detected either in the air or in the water of marshes, but by the excessive exaggerated influence of electricity, some means which have in view the modification of this morbid influence should naturally and rationally be preferred.

5. Electrical insulation happily fulfils this indication. The insulation may readily be effected by fitting on to the ordinary forms of beds, feet made of glass, or of resin. A great number of observations have demonstrated to M. Pallas, that all the sick who have been insulated, have been cured or relieved of their severe diseases, many of which had previously resisted all known means of cure.—(*Pallas, Reflexions sur l'Intermittence, &c.*)

² Dublin Med. Press, Nov. 27, 1844; Am. Journ., ix. (N. S.) 458.

the human nerves, and probably the relative states of the particles, and the polar relations of the atoms or corpuscular molecules, and, at all events, is capable of exciting or depressing the vital functions, and of acting chemically on the circulating animal fluids. Somewhat similar are the views of Heyne,¹ who derives this excess of electricity, so far as relates to the fever of some parts of India, from certain iron hornblende, which forms a large ingredient in the composition of the soil.

If such is the case with respect to malarial combinations generally, or the electrical development from certain minerals, we can find no reason for denying that the same result may obtain in relation to those combinations or agencies which give rise to the yellow fever.

From all that has been said regarding the effects of atmospheric electricity, and its probable influence on the development and diffusion of disease, it will be natural to foresee that in those regions of the globe where, and in seasons of the year when the fluid is more extensively diffused and frequently modified, the effects upon individuals unaccustomed to its agency will be more sensibly felt. Such, it is said, is the case in the West Indies, where, while the electricity is very abundantly diffused, and more apt to be modified, diseases of a febrile character—the yellow fever especially—are very rife. Such also is the case, to a certain, though less extent, in the summer seasons of those parts of temperate regions where the same electrical results are observed.

The difficulty of eliciting electrical phenomena in the West Indies, by means of even the strongest and most perfect apparatuses—a fact noted by many experimenters²—is not to be viewed as indicative of any deficiency of atmospheric electricity. That this result may be accounted for by the extreme humidity which exists in those islands, and impedes the play of the batteries, is shown by the fact that effects of a different kind are obtained in very dry weather, as likewise in elevated situations where the humidity is not felt; while the deficiency in question is further disproved by the frequency and violence of storms, attended by intense electrical discharges which are observed in most parts of those regions at the commencement and close of the rains, as well as in all places and seasons characterized by intense solar action, and during a state of atmospheric humidity favouring the passage of the fluid from the earth to the clouds.³

From a review of the aforesaid effects of electricity on the human system, and the abundant diffusion of that agent in the atmosphere of the Antilles, it has been inferred, that to constitutions unhabituated to the excessive stimulation resulting from that condition of air, injurious effects are produced, and that such individuals are thereby rendered prone to the yellow fever (*Rochoux*, p. 20). Should this be the case in these islands, we could have no reason to doubt the propriety of applying the same principle to the hot season of such

¹ Med.-Ch. Rev., July, 1842, pp. 141, 144.

² Moreau de St. Méry, i. 522; Cassan, *Mémoire sur le Climat des Antilles*, &c.; *Mém. de la Soc. Méd. d'Emulation*, v. 29; Savarésy, p. 165; Rochoux, p. 19; Lefoulon, pp. 72-3.

³ Tullock, *Sickness and Mortality in British Army*, p. 4; Forry, p. 63; Rochoux, p. 19.

places of the temperate zones where the fever prevails. Hence, some of the writers on the fever of the West Indies ascribe to electricity considerable agency as an exciting and predisposing cause in epidemics of yellow fever—Belot, Maher, Rufz, Bertullus—an opinion predicated on the frequency of thunderstorms during some epidemic seasons, the increase in the number of cases observed after the occurrence of such storms, and the fatal results on the sick which follow.

The late Dr. Caldwell was not blind to the fact that the matter of electricity plays an important part in furthering the operation of the cause of yellow fever and other epidemic diseases. Following in the footsteps of Mr. Webster, and anticipating in some measure Dr. Parkin, who, in a late ingenious and learned essay,¹ has endeavoured to show that all epidemic complaints—yellow fever among them—are due to volcanic action, Dr. C. remarks: “During several of the yellow fever calamities in Philadelphia and the other Atlantic cities, electrical phenomena were unusually irregular. Shooting stars were at times abundant and brilliant in a degree far beyond what is common. Throughout some seasons, especially the summer of 1793, scarcely a gleam of lightning was seen; while in others, thunderstorms were inordinately frequent and severe. In 1799, the shooting stars were most abundant.”²

But, while many respectable authorities are found who favour the idea that epidemic diseases are due to, or connected with the operation of an excess of atmospheric electricity, and while facts may be gathered of a nature calculated to justify the belief, other facts may be adduced to show, and other authorities entertain the opinion, that the development of febrile and other epidemic complaints must be attributed to a deficiency of that fluid in the atmosphere. Restricting myself to the subject of the form of fever which constitutes the object of our inquiries, it is to be remarked, that in most places where the latter is endemic, and, to a certain extent, in those where it occurs only at special times, circumstances present themselves which tend to modify not only the electrical impression on the system in regard to intensity of action—thereby rendering it more or less open to the inroad of disease—but also the condition in which that electricity exists in the atmosphere. I shall not stop to point out the nature of those influencing circumstances, and the modifications produced. They are well known to the reader. Suffice it here to say, that all the variations and modifications thereby produced which are of frequent occurrence in the West Indies, and, in the summer months of some parts of temperate climates, must, in view of the effects of electricity on the system, give rise to modifications in the economy and in surrounding objects of a kind to predispose the former to the impression of morbid influences, as well as to promote in the latter changes detrimental to health.

It is known that the positive electricity of a clear atmosphere is much more powerful in winter than in summer, and that it varies in a regular manner in the intervals of the two seasons. Basing his calculations on the results

¹ The Remote Cause of Epidemic Diseases, &c., Part II. 10. London, 1853.

² Boylston Prize on Quarantine, 1834, pp. 50, &c.

obtained during observations continued throughout a period of four years, Dr. Turley found that the mean variations of the monthly electricity were—

January	605	July	49
February	378	August	78
March	200	September	82
April	141	October	188
May	84	November	282
June	47	December	669

Hence the atmospheric electricity attains the highest point in the winter season, when, from the depression of thermometrical heat, the exercise of the organic functions is rendered more difficult and less active.¹ Let it be remembered, that it is during the period when electricity is thus shown to be in least proportion, that fever is more prevalent. Whether we judge of the extent of atmospheric electricity by the frequency or absence of thunderstorms, or by other means, in different years during epidemic seasons, we arrive at the same conclusion relative to the connection of fever with deficiency of the fluid.

Dr. James Clarke (pp. 55–57), of Dominica, remarked, that when there was much thunder and heavy rains in June and July, the season was not sickly; but that if the weather was fine in those months, it was followed by a hurricane, or considerable sickness. Influenced by these and kindred facts, he attributed the fever to a deficiency of thunder (together with heavy rains and violent gales of wind), which he regarded as correctives to impure air. Lempriere (i. 12, 19), like Clark, regarded such storms as clearers of the atmosphere, and therefore as beneficial agents. “Thunder and lightning,” he says, “certainly seem to contribute to render the air more healthy, for in those seasons which have proved the most sickly, this phenomenon has been less prevalent, and, perhaps, the diseases at the latter end of the year are, in some degree, rendered worse, by the more frequent failure of lightning in the autumnal rains.”

In confirmation of this, we find that in later times the infrequency of thunderstorms during and before some epidemics, and the disappearance of these through the effect of such storms or hurricanes, have been similarly noticed.²

How matters stood in Brazil at the time of the breaking out of the epidemic of 1850, we learn from Dr. Lallemand: “No inhabitant of Rio de Janeiro can forget the regularity with which, until four or five years ago, thunderstorms occurred almost daily during the hot weather. When, in the month of November, the sun returned from hyperborean latitudes to pass over our heads, and to culminate over the shores of the province of Rio de Janeiro, there were various agents to mitigate the burning heat. One of the most powerful agents of this nature was, without doubt, the thunderstorms. When, in the hot months, the morning hours had exhausted the physical forces in general, and especially those of labouring men, by two or three

¹ Foissac, i. 142, 258; Fleury, p. 78.

² Imray, Edin. Journ., lxiv. 331; Arnold, on Bilious Fever, p. 212.

o'clock in the afternoon, the tops of the mountains have become hidden within the thick thunderclouds. At a distance, the bright glare of the lightning broke through the celestial mantle, and very far off was heard the rolling of the thunder. At five o'clock, the storm, in general, very rapidly left the mountains; to a strong wind succeeded a fierce combat among the elements. The air trembled with continuous claps of thunder; a very copious rain ended the strife, frequently leaving the long streets of the city impassable for more than an hour. Thus, the air underwent a most violent agitation; thus, afterwards, everything that was suspended with it was precipitated by the rain; thus, whatever had passed into a state of putrefaction, was swept away by the very copious torrents; thus, was the heat checked; thus, was all nature reorganized. It is four or five years since this change, so peculiar to these useful elements for the diminishing of the geographical predisposition to diseases, became sensibly lessened. Rarer and rarer became the thunderstorms; and, in the hot season of the years 1849-50, they had nearly altogether disappeared. It is true, that the mountain tops were frequently hidden by thunderclouds; it is true, that lightning flashes sometimes reached as far as us, and that we heard the very distant rollings of thunder; but an impenetrable barrier seemed to have been raised on the plains on the other side of the bay; and, however heavy thunder there was on the mountain tops—however many whole weeks of copious rain there were up there, the city and the vicinity were in the greatest apparent tranquillity of nature; the apparent tranquillity of a cemetery. No wind preceeding an electric discharge; no bursting out of a thunderstorm; no copious rain; no interruption of the intertropical heat; even the south-southeast breeze, formerly so regular and so strong, was, in this year, rarer and slighter. Under Phæton's ear, the proximity of which once more burned the world, *domibus negata*, and, under the above-mentioned conditions, the vital forces of the universe were exhausted; it was impossible that humanity either could longer resist that general fermentation; the human race sickened on a grand scale; and where the organism was not accustomed to resist these influences, when it was not acclimatized, it followed the immutable laws of nature; the organism was dissolved into inorganic matter. If, indeed, we find an indubitable predisposition to the yellow fever in the geographical conditions of the bay of Rio de Janeiro, these were completed by the want of electric action, by the want of thunder and rain, and by the heat necessarily increased by these indicated deficiencies."¹

If, in this country, instances may be pointed out, as at Natchez in 1837, for example, when in the course of the year there occurred sixty-three storms attended with thunder and lightning; if, I say, in such cases, the electricity was in greater excess than usual, or if, from peculiar circumstances, it became more frequently manifest, in other and more numerous instances, the fluid has been found deficient in quantity, or its manifestation less frequent in

¹ Lallemant, Observações acerca da Epidemia de Febre amarella do anno de 1850, &c., pp. 33, 34.

times of epidemics. Such was the case in New York during the fever of 1795 and 1822;¹ in New Haven² and New London in 1798;³ in Savannah in 1820;⁴ in Charleston in 1817,⁵ as also in other sickly years,⁶ and in Philadelphia during the fatal season of 1798.⁷

In Charleston, no fever occurred for forty-four years after 1748. During that period of exemption, there was a frequent recurrence of showers and thundergusts. In the latter part of that time, a thundergust was of almost daily occurrence. After 1792, a change took place—light showers were succeeded by heavy falls of rain, and not so generally attended by lightning—yellow fever was more common. Under frequent occurrences of showers and thunderstorms “yellow fever did not appear in 1798, 1806, 1811, and 1821.”⁸ By Dr. Shecut, of Charleston,⁹ greater stress has been laid upon this meteorological condition of atmosphere than by any one else. During the prevalence of the yellow fever of 1817, he discovered that operations on his electrical machine varied with the rise, progress, and declination of the fever; that when the latter first appeared in the city that summer, and, indeed, for some time previous to its occurrence, the electric power of the machine had begun to decline; that when the fever raged with its utmost violence and fatality, sparks were not elicited—the fluid being almost totally inert; but that as soon as the disease began to decline, a visible change also took place in the effects of the machine, from which sparks were with some little difficulty produced; and that, on the entire cessation of the epidemic, which took place shortly after the restoration of an electrical equilibrium, produced by the heavy concussion of thunder and lightning on the 14th of October, 1817, operations on the machine were performed as usual with an evident increase of electrical power, as in the other healthy states of the atmosphere of Charleston. Dr. Shecut further remarked, that the years in which the yellow fever prevailed most extensively in Charleston—1732, 1739, 1745, and 1748—were, with some exceptions, either excessively hot and dry, or excessively hot and moist, and always attended with little thunder and lightning (p. 97). From these facts he draws the inference, that to the deficiency in question must be ascribed the cause of the yellow fever; and that it acts by diminishing the vital influence of the air, and promoting thereby the generation of a gaseous poison (pp. 60, 61, 92, 93, 99, 100).

In the present state of our knowledge on the subject, and in the absence of more numerous and positive facts, it would be hazarding too much to either adopt the views entertained by Dr. Shecut, or to reject them as erroneous. Still more improper would it be, after all we have seen, to deny that the electricity of the atmosphere, by its excess or deficiency, exercises on this continent

¹ Townsend, p. 262; Bayley, pp. 54, 123; J. Smith, pp. 76, 92.

² Monson, p. 179.

³ Med. Repos., ii. 403.

⁴ Waring, p. 21.

⁵ Dickson, Chapman's Journ., iii. 251.

⁶ Simons, p. 18.

⁷ Condie and Folwell, p. 13.

⁸ J. Johnson, Charleston Med. Journal and Rev., iv. 154.

⁹ Essays, Preface, i. 92, 105.

and Europe an agency, as an exciting or predisposing cause of the disease. Its agency in promoting the play of the functions, its enlivening and invigorating power, the sensibility to its impression in affections of a purely nervous character, are subjects of common observation. "Every physician," says our countryman, Dr. Littel,¹ "must have noticed the great frequency of asthmatic attacks before a change of weather. The epileptic paroxysm occurs also most frequently in the night; and while this may perhaps be explained, in part, by the temporary suspension of the will in sleep, it is not irrational to attribute it in some degree to electrical changes, which are both more common and more prejudicial at that period, especially as we know that some persons, subject to this malady, are affected only at the vernal and autumnal equinoxes, when these fluctuations are greater than at other times. We have all experienced the feeling of energy and elasticity which is imparted when the electrical fluid is present in due proportion in the atmosphere—as in clear cold weather; and more strongly still the sensations of chilliness and discomfort occasioned by its deficiency under opposite conditions. Rheumatic persons, and those who have recently suffered from sprained or fractured limbs, can predict with unerring certainty an approaching atmospherical change, though to others there may be no sensible indications of its occurrence."

On the other hand, the prostration, oppression, dulness, listlessness, heaviness, and languid sensations of mind and body consequent on an abstraction and paucity of electricity, are too notorious to occupy us here. But while recognizing this, it would be unsafe to adopt either view to the exclusion of the other, and to regard it in the light of the proximate cause of epidemic diseases generally, and of the yellow fever among these.

In most of the cases adduced in proof of the influence of such deficiency, the latter has been inferred from the difficulty of eliciting shocks from electrical batteries and the infrequency of thunderstorms. As regards the former of these phenomena, it may be remarked that the failure of the trial arose necessarily from the great humidity of the atmosphere prevailing at the time—for humidity, under all circumstances, offers an insuperable obstacle to the elicitation of electrical effects; that even when the same results are noticed at periods when the atmosphere is apparently dry, it may nevertheless have been really otherwise—such a condition of atmosphere being demonstrated by the high state of the dew-point, and that in no circumstances do such failures indicate an absence of electricity, but only greater power of conduction in surrounding objects. The failure of Dr. Shcut and others may therefore, after all, have been due to causes inappreciable to his observation, but different from the one he adduces. With reference to those sickly seasons when thunder and lightning occur less frequently than usual, it may be remarked that, from this deficiency no conclusion can be drawn in favour of the doctrine, because, in most of them, nothing appears to have been done to ascertain by other means the electrical condition of the atmo-

¹ Medical Examiner, x. (N. S.) 387, 388.

sphere, and because some of those seasons were characterized by excessive dryness, when, though there may be few storms, the electricity does not, for that, prove deficient in quantity, but remains accumulated in terrestrial objects.

That storms may act beneficially in the way mentioned by Clark, Lempriere, and others, *i. e.*, by cleansing the atmosphere of noxious exhalations, and imparting to it greater elasticity and purity we can readily believe. Every one in our latitudes has observed that such storms are followed by an improved condition of the air, a change of wind, and a pleasant depression in the thermometer; and it is well known that the cessation of epidemics has not unfrequently been traced to the occurrence of violent electrical discharges and atmospherical commotions. This, as we have seen, was the case here in 1741 (see *ante*, p. 56); and many other instances of the same kind might be adduced. Dr. Arnold informs us that, in 1815, at Jamaica, the hurricane of October had a powerful influence in cleansing the atmosphere of impurities. After it there was scarcely a case of remittent fever in the parishes it visited (p. 212.) Nor is this all. The results of scientific inquiries show the occasional production of ozone—the most powerfully bleaching and oxidating substance in nature—by electric discharges, and it is natural to conclude that, to this agent, found during the discharges of atmospheric electricity, we may owe the disinfection of the air from a variety of noxious miasmata, and from the active cause of yellow fever. Dr. Schönbein, to whom we owe the discovery of ozone, remarks on the subject of its effects: “It is a very old popular opinion that thunderstorms are capable of purifying the atmosphere, and I think there are some grounds for entertaining that notion. As we know that during a thunderstorm comparatively large quantities of ozone are formed, we can easily conceive in what manner such a purification may be brought about. The deterioration of atmospheric air which is supposed to take place in the hot season, may possibly consist only in an accumulation of miasmatic gases—principally resulting from the putrefaction of organic matters—in the lower regions of the atmosphere; and the purification of the air can be effected only by the destruction of those miasmatic gases. Now, as ozone is abundantly produced by thunderstorms, that principle will act like chlorine, and will purify the air in which those electrical phenomena take place.”¹

¹ This substance, which at one time was supposed to constitute the efficient cause of epidemic disease, because it is sometimes found in the atmosphere during the prevalence of these, may now, if the experiments of Dr. Schönbein¹ are to be relied upon—and, so far, no doubts have been expressed on the subject—be considered as the most powerful disinfectant, and the great purifier of the atmosphere. It destroys, quietly and effectually, the miasma disengaged from putrid flesh; and there is every reason to believe that it acts as efficiently in regard to the cause of fever, as to atmospheric poisons, artificially produced; whilst its effects in respect to these and its virtues as a destroyer of the cause in question, lend a strong support to the opinion which ascribes fevers to the existence, in the air, of peculiar poisonous exhalations. Ozone is abundantly found during thunderstorms, and we know that these purify the atmosphere, mitigating or arresting the spread of epidemic fever. If it is diminished in volume, by the presence of impurities artificially

¹ Med.-Chir. Trans., xxxiv. 212.

Further than that I cannot go. Electricity may, and no doubt does, act as an exciting cause by its excess, and as a predisposing one, sometimes, by this excess, and more frequently by its deficiency and modifications. It is possible, also, that, by a derangement of the electric force, or its deficiency, and by the absorbing effects of certain winds, which, as we shall have occasion to notice soon, often prevail in epidemic times, the formation and diffusion of the remote cause of the disease may be promoted; for it is not without reason that Dr. Turley and others ascribe the degree of vitality of animals and vegetables to the greater or less degree of electricity. Diseases of vegetables, and blights in general, have been traced to a deficiency of terrestrial electricity; and we know that while a supply of that fluid prevents or retards putrefaction, an excess or deficiency of it accelerates the process. In a word, electricity may, by its excess or deficiency, operate on the system in a twofold manner—as an exciting and as a predisposing agent; and may, besides, under particular circumstances, promote the development of the efficient cause of the disease, which an excess tends to neutralize. To all this no one can object. But when we find medical writers, while rejecting the idea of recognizing the existence of separate and distinct poisons for the several exanthemata, for influenza, for cholera, for each of the different kinds of fever, for whooping-cough, mumps, &c., and while maintaining that an etiology so manifold cannot be true, refer all these different and dissimilar diseases to various modifications of a single principle—electricity; when we find that fluid accused of producing, on some occasions, scarlet fever, or smallpox, or measles, or typhoid, typhus, remittent, bilious, or yellow fever, or influenza, and at other times ordinary phlegmasiæ—the only reason of the difference being diversity of predispositions, “arising from a variety of circumstances, existing in countless combinations and involving whole communities, or affecting individuals only”—we must pause. The idea of referring scarlet fever, smallpox, and yellow fever to a little more or less electricity, can scarcely be acceptable to sound pathologists. The body, as has been remarked long ago, may be made the

produced, and whose presence in the atmosphere cannot be doubted, it is, in like manner, found in less quantity in hot seasons and fever regions, where malaria may be supposed to exist in greater abundance; and if, in the former case, the diminution in question is produced by the action of the disinfectant, in neutralizing or destroying the existing impurity, we cannot greatly err in referring the diminution, in the latter case, to a like action of that substance on a kindred poison, and its consequent consumption. Ozone, on the other hand, exists in greater abundance in winter; and, as this is precisely the season at which miasmatic fevers do not prevail, and when the atmosphere is in the greatest state of purity, we are justifiable in the conclusion that its accumulation, at that period, arises from a less demand of it for the decomposition of oxidable miasmatic matter or poison. Again, it has been found that the higher stratas of the atmosphere are more ozoniferous than the lower ones, an effect easily accounted for by the circumstance that those strata contain a less quantity of that oxidable miasmatic matter than those portions of the atmosphere which are nearer the surface of the earth, and that hence a smaller quantity of the disinfectant substance is consumed. In a word, ozone acts like chlorine, by destroying impurities existing in the atmosphere; and, if it purifies the air of infected localities, and destroys the cause of fever, it can only do so by destroying or neutralizing a kindred impurity, or poisonous exhalation, floating in the atmosphere of such localities.

medium of very energetic electrical or electro-motive action, without injury being inflicted on it; and it is only when a very powerful and very manifest current of either the negative or positive electricities strikes or passes through it, that life is thereby in any way affected.¹ It would require many a shock to produce smallpox or yellow fever; and without being accused of undue skepticism, we may doubt the possibility of producing the same effects by abstracting a large sum of the fluid from the body, be the predisposition what it may. Whatever may be the case with respect to other zymotic diseases, the idea of looking to electricity for the remote or effective cause of the yellow fever is not tenable. The sphere of prevalence of the disease is often circumscribed within such narrow and well-defined boundaries, that it is difficult to perceive the propriety of regarding it as due to any modification in an agent of that general and wide-spread character necessarily possessed by the one under consideration. This modification, whatever it may be, whether consisting in an excess or deficiency of the fluid, extends far and wide, and cannot fail to exercise its influence, as well at a distance from, as within the precincts of, the sickly district; and, were the theory correct, we should not hear of, and every day see, instances of fever being arrested by a street, a wall, or the like; or restricting its ravages to a very limited surface of ground, to a single ship or side of a ship, to one side of a street, &c. The same modification in the electricity of the atmosphere must have existed beyond those narrow limits; and if it were the sole cause of the fever on one side of the barrier, it would in all probability have produced a similar effect on the other. Some other agent, then, must have been at work in the former, which did not exist in the latter. It matters not whether the sickly locality covers a small area of ground, or extends over a wide surface, the necessity of that peculiar agent must be admitted. The disease is always the same, and must be produced everywhere by the same cause. It is different from other diseases, and must be produced by causes different from those which give rise to these. It cannot, therefore, be the product of a morbid agent, which can by no possibility produce it artificially, and which, supposing the assertion of the advocates of its agency to be correct, produces disease of a dissimilar kind. Add to this, that this agent is always associated with modifications of heat, humidity, &c., each of which is entitled to our regard in estimating the degree of influence of febriferous causes.

c. *Atmospheric Pressure*.—So far as I can ascertain, no observations of importance have been made in regard to the barometrical changes that take place during or just before the prevalence of the yellow fever, or any attempt made to connect, as cause and effect, the particular degrees of pressure of the atmosphere with the production and diffusion of that disease. Dr. Prout² remarks that at the time the Asiatic cholera first made its appearance in England, and during the prevalence of east winds—which at that period displaced those from a western direction—there was a positive increase in the weight of the atmosphere (rise in the barometer), which continued during the prevalence of

¹ Copland, i. 770, Am. ed.

² Bridgewater Treatise, p. 197, Am. ed.

the disease. This was ascribed by Prout to the diffusion of some gaseous and poisonous body, analogous to malaria, and considerably heavier than the air it displaced; and which, in consequence, kept close to the surface of the earth, creeping along this, and especially in low and humid places. The observations of Dr. E. Barton,¹ of Louisiana, on the barometer and east wind (which he regards as a fruitful source of epidemic diseases), correspond with and confirm, as he thinks, so far as the yellow fever of New Orleans is concerned, those of Dr. Prout. But Dr. Barton would seem to stand alone in this matter; and in the present state of our knowledge, and considering the paucity of materials within our reach on the subject, it would be unsafe to confirm or disprove the opinion that may be entertained respecting the connection existing between the state of the barometer and the manifestation of the fever in question. All that can and need be said on the subject is, that a comparison of the state of atmospheric pressure here and elsewhere, during sickly seasons, with the results of observations made at periods when the disease does not show itself, does not lend much assistance to the belief in the reality of any such connection, so far, at least, as relates to the production of the efficient cause. In the West Indies, where the fever is common, the diurnal fluctuation of the pressure pursues a march so regular, that we may infer the hour of the day from the height of the mercurial column, without an error, on the average, exceeding fifteen or seventeen minutes. Humboldt says: "In the torrid zone of the new continent, I have found the regularity of this ebb and flow of the aerial ocean undisturbed, either by storm, tempest, rain, or earthquake, both on the coasts and at elevations of nearly 13,000 feet above the sea." The total diurnal oscillation amounts, under the equator, to 0.117 inches, diminishing gradually as the latitude increases.

The average at Demerara, for four years, 1843, 1844, 1845, 1846, and two months, January and February, of 1847, was 30.076; the range between the extremes of those four years varying from 0.101 to 0.306, and presenting an average of 0.178. In the colony here mentioned, the mean pressure, in 1843, was 30.218; in 1844, 30.233; in 1845, 30.082; in 1846, 29.950; in January, 1847, 29.952; and in February, 1847, 30.007 (range .383). In the first of this series, the pressure varied from 30.014 to 30.287 (range .178); in the second, from 30.151 to 30.280 (range .129); in the third year, from 29.859 to 30.165 (range .306); and in 1846, from 29.902 to 30.003 (range .101). The yellow fever, which had prevailed in 1843 and 1844, ceased in 1845, when the pressure was least, and the range greatest. These results correspond closely with those obtained usually throughout the whole of the West Indies, where, as just remarked, the barometer remains almost stationary, and where, unlike what takes place in some parts of this country, and more particularly in England (the range of which is from two to three inches), the extreme range does not exceed from one-fourth to one-half an inch in the year, even during the most violent hurricane. At Martinique, the usual height

¹ Fever of 1833, p. 9.

of the barometer, according to Moreau de Jonnès,¹ varied from 28 inches and 4 lines to $27.10\frac{1}{2}$, with a medium of $28.1\frac{1}{4}$. This, reduced to English measurement, gives $30.\frac{947}{1000}$ for the greatest pressure, $30.\frac{447}{1000}$ for the least, and $30.\frac{739}{1000}$ as a medium. Reduced, again, to the present French measurement, we have a variation of from 784 to $771\frac{1}{2}$ millimetres, and a medium of 778. The range, therefore, is about $5\frac{1}{2}$ French lines, or $\frac{1}{2}$ of an English inch, $12\frac{1}{2}$ millimetres. At St. Domingo, Moreau de St. Méry² tells us, the greatest pressure is 28 inches and 3 lines (French); the lowest, 27 inches and 10 lines.

In this city, the average pressure of the atmosphere in 1793, 1797, 1798, 1799, 1802, 1803, 1805, and 1853, yellow fever years, as also 1800, 1801, and 1854, when the fever existed sporadically, was as follows :—

	June.	July.	August.	September.	October.	Range.
1793		29.35	30.40	29.88	29.85	1.05
1797			29.26	29.95	30.03	.77
1798	29.80	29.84	29.96	29.29	30.01	.72 during the year.
1799	29.76	29.73	29.72	29.94	29.96	.24 “
1800	29.92	30.09	30.04	30.07	30.04	.17 “
1801	30.01	30.07	29.99	30.15	30.13	.02 “
1802	29.98	30.00	30.05	29.98	30.10	.18 “
1803	30.03	30.03	30.07	30.06	30.02	.05 “
1805	30.07	30.23	30.23	30.29	30.18	.22 ³ “
1853	29.987	29.955	29.913	29.976	29.974	.074
1854	29.824	29.926	29.946	30.020	29.994 ⁴	.196

The following table gives the average of nine healthy years, during which the yellow fever did not prevail :—

	June.	July.	August.	September.	October.
1841	29.09	30.1	30	30.1	30
1842	30	30	30	30	29.90
1843	29.95	30.05	30	30	29.90
1844	30	29.9	29.8	29.8	29.70
1845	29.95	29.8	29.9	29.8	30
1846	30.01	29.9	29.9	30	29.90
1847	29.89	29.9	30	30	30.25
1848	28.5	29.9	30	29.92	29.94
1852	29.848	29.903	29.952	30.034	29.976

If we direct our attention to the difference of pressure in sickly and healthy years, both here and elsewhere, we find the difference so small that it is difficult to ascribe to it any agency. In the West Indies, scarcely any difference is noticed from year to year—whether the disease exist or not. Take Martinique as an example. We have seen that the ordinary pressure never varies more than a few millimetres—the medium being $.771\frac{1}{2}$. In 1838–39, the fever, after an interval of ten years, prevailed extensively. During that time, the pressure varied from .766 to .760 ($30.\frac{236}{1000}$ inches, English, to 30 or $\frac{236}{1000}$ of an inch—3 lines), the difference being between that period and ordinary times 8 millimetres (*Rufz*, p. 29). In 1843, the fever again pre-

¹ Climat des Antilles, p. 60.

² Description de St. Domingue, i. 522.

³ Coxe's Med. Mus., i. 201 ; ib., ii. 436.

⁴ Medical Examiner, 1854.

vailed, and during the sickly season the barometer marked .762 to .765 in July; .763 to .766 in August, and .762 to .765 in September, the range being for the three months from 762 to 766 (30.236 to 30.079 inches, English).¹

In Demerara, as we have seen, the pressure in 1843–44, yellow fever years, was respectively 30.218 and 30.233; while in 1845, when the epidemic ceased, and in 1846 when there was no fever, 30.082 and 29.950. The average of the first two years was 30.225, and that of the last two 30.016, the difference being $\frac{209}{1000}$ of an inch, or less than three lines.

In Cadiz, during the awful epidemic of 1800, the barometer marked in July, 28.14 (French); in August, 28.14; in September, 28.15, and in October, 27.70. Now, when with this we inquire how matters stood in 1799, when there was no yellow fever, we find that July gave 28.10; August, 28.16; September, 28.16; and October, 27.93. In Charleston, during a series of eleven years, extending from 1844 to 1854, inclusive, and embracing several yellow fever seasons, and many more in which the disease did not make its appearance, except perhaps sporadically, the differences and fluctuations in the pressure from May to October, were so trifling and irregularly distributed as to merit but little attention in an inquiry as to the causes of the disease. The following table will show this at one glance. The fever prevailed severely in 1849, '52, '54; one death is reported in 1844; and no mention is made of the disease in the register's reports for 1845, 1846, 1847, 1848, 1850, 1851, and 1853.

	1844.		1845.		1846.		1847.		1848.		1849.	
	Max.	Min.	Max.	Min.	Max.	Min.	Max.	Min.	Max.	Min.	Max.	Min.
May	30.14	29.30	30.50	29.80	29.96	29.33	29.83	29.30	29.94	29.48	30.05	29.38
June	29.90	29.50	30.45	29.94	29.90	29.55	29.89	29.53	29.86	29.59	29.95	29.56
July	29.85	29.40	30.36	29.92	29.94	29.58	29.97	29.58	29.97	29.50	29.98	29.56
August . . .	29.92	29.25	30.34	30.02	29.90	29.60	29.93	29.37	29.86	29.54	29.87	29.62
September . .	29.99	29.40	30.33	29.81	29.95	29.44	29.82	29.43	29.86	29.48	29.95	29.43
October . . .	30.05	29.04	30.50	29.75	30.00	29.05	30.13	29.45	29.87	29.20	30.07	29.44

	1850.		1851.		1852.		1853.		1854.	
	Max.	Min.	Max.	Min.	Max.	Min.	Max.	Min.	Max.	Min.
May	29.83	29.38	30.50	29.82	30.51	29.95	30.41	29.94	30.32	29.91
June	30.05	29.44	30.35	29.88	30.45	29.80	30.37	30.11	30.30	29.90
July	29.86	29.50	30.30	29.99	30.33	29.95	30.37	30.04	30.39	30.08
August . . .	29.90	29.49	30.33	29.81	30.34	29.87	30.31	29.90	30.29	30.05
September . .	29.87	28.60	30.46	29.87	30.38	29.60	30.34	29.99	30.48	29.68
October . . .	29.97	29.49	30.35	29.69	30.46	29.78	30.38	29.80	30.48	29.85

In New Orleans, there was no yellow fever in 1845, a mild epidemic in 1849, a severe one in 1853, and a most severe one in 1853. The barometer marked as follows in these years during the months of June, July, August, September, and October.

¹ Catel, Annales Maritimes, 1844, ii. 221; Amic, On Fev. of Martinique, Report of Sanit. Com. of New Orleans, for 1853, p. 208.

	1833.		1845.		1849.		1853.	
	Max.	Min.	Max.	Min.	Max.	Min.	Max.	Min.
June	29.75	29.32	30.25	29.98	30.15	29.80	30.37	30.12
July	29.82	29.57	30.31	30.04½	30.16	29.89	30.37	30.11
August . . .	20.79	29.52	30.20	30.02	30.14	29.92	30.29	30.04
September . .	29.75	29.50	30.13	29.96	30.17	29.76	30.33	30.02
October . . .	30.03	29.60	30.24	30.04	30.35	29.72	30.18	29.74

Dr. Ed. H. Barton, of New Orleans, to whom the scientific world is indebted for much valuable information on the connection of meteorology with etiology, has pointed out that the mean pressure of the atmosphere—basing his calculations on an examination of fifteen of the epidemics which have occurred in that city, was, at the commencement, 30.108, at the maximum of the epidemic 30.024, and at the declination 30.074. The difference between the pressure at the commencement and the maximum of the epidemics is .084, and that between the maximum at the declination, .050. Whether these differences will be found to hold in other places must be determined by future observations; and whether they will be regarded as an essential element in the causation of the disease, I must leave to the decision of the reader.

In this city, the mean pressure of the atmosphere, in June, of seven yellow fever seasons, varied from 29.76 to 30.07; July, in eight, varied from 29.35 to 30.23; August, in nine, varied from 29.26 to 30.21; September, in nine, from 29.29 to 30.29, and October, in seven, from 29.85 to 30.18. On the other hand, in nine yellow fever years, the barometer in June marked from 29.84 to 30.1; July, from 29.003 to 30.1; August, from 29.8 to 30; September, from 29.8 to 30.1, while October, in eight of those years, gave a difference of from 29.70 to 30.25. The average of June, in sickly years, is 29.920—that of non-sickly seasons, 29.785. The average of July in the former, is 29.880; in the latter, 29.730. That of August in the former, 29.770; in the latter, 29.690. The average of September is 29.930 in the former, and 29.950 in the other. The one in October is 30.020 in the former, and 29.945 in the latter. If we examine the difference between the pressure in 1793 and 1798, years of violent epidemics on one hand, and that in 1800 and 1801, when there were but a few cases, if any, on the other, we shall perceive these to be very slight. July in the first two years gave a pressure of 29.35 and 29.84; in the latter two 30.09 and 30.07, difference 00.97. August, in the first two, gave 30.40 and 29.96; in the latter, 30.04 and 29.99, difference 00.33. In September, the pressure in 1793 was 29.88; in 1798, 29.29; in 1800, it was 30.07, and the next year 30.15, difference 1.08. In October, the first two gave us 29.85 and 30.01; the other two 30.04 and 30.13, difference 00.32. Finally, in November, the first two presented a pressure of 29.64 and 30.04, and the last two 29.97 and 30.13, difference 00.54. I am unable to state the pressure in June, 1793; but in 1798 it was 29.80; in 1800, 29.92, and in the next year, 30.01. The average of these two years was 29.96,

giving us a difference from 1793 of 00.16. Thus, we perceive that in June and July—the most important of the series, so far as relates to the question of the origin of the disease, the difference was smaller than in September, when the epidemic was already established. The pressure was greater in all the months of the last two years of the series except August, when the difference was in favour of 1793 and 1798.

As to the range of the pressure, we can scarcely admit that it produced any effect in relation to the subject before us. The largest in this city amounted to 1.80, and the lowest to 0.5.

	June.	July.	August.	September.	October.
1793			1.27	1.35	1.15
1797			1.77	1.19	1.80
179853	.29	.48	.23	.73
179978	.49	.55	.35	.67
180227	.40	.54	.46	.57
180353	.40	.48	.53	.75
180569	.45	.47	.42	.82
185481	.74	.82	1.30	

In New Orleans, the ranges in June, July, August, September, and October, amounted, in 1833, to .43— .25— .27— .25— .43. In 1845, the ranges were, .27— .27— .18— .17— .20. In 1849, .35— .27— .22— .41— .61—, and 1853, .25— .26— .25— .31— .34. These differences are too small to have exercised much influence. Besides, we must recollect that in the West Indies, where the fever prevails frequently, the oscillations are trifling.

I am far from denying the influence which a difference of pressure of the atmosphere exercises on the system in health and disease. Nor shall I attempt to ignore the fact, that an undue increase of it produces unpleasant effects, and leads even to diseased manifestations, and that other results, of an equally deleterious effect, attend an extreme in the opposite condition of the air. Every one knows the effects on the feelings of opposite conditions of the barometer—that when it is low, and smoke and vapour and fogs linger near the surface of the earth, the body feels languid, that fatigue soon follows on exertion, and the intellectual functions lose their activity. On the other hand, we know that when the barometer ranges high, there is a feeling of vigour and activity of mind, which, as the late Dr. Drake well remarks, is disproportionate to the mere reduction of temperature which is generally connected with that change. Every one knows this, and knows also the extreme lassitude, uneasiness, vertigo, nausea, and vomiting, dyspnœa and hemorrhage, experienced by those who have ascended to a great height—phenomena due, no doubt, to the diminished weight and density of the atmosphere. Nor can any one be ignorant of the injurious tendencies of those violent commotions of the barometer, which sometimes occur. But there is nothing in all this calculated to make us believe that it can do more than place the system in such a condition as will predispose it to the deleterious impression of some more efficient cause, especially when we find that the same condition of the barometer exists, as well when the yellow fever prevails as when it does not.

CHAPTER VII.

CIRCUMFUSA, CONTINUED.—HUMIDITY.

WE are next to examine the agency of atmospheric and terrestrial humidity in the production of the yellow fever. In doing so, my remarks will apply as well to the dampness diffused in a visible or in a latent state in the air, and resulting from the evaporation of adjacent or distant permanent sheets of water, as to that produced in the atmosphere and on the surface of the earth by rain.

The connection of yellow fever with humidity has been, from an early period, a subject of observation and a fruitful theme of comment. Sure it is, that in many countries where the disease is endemic, or where it prevails frequently in an epidemic or sporadic form, the moisture of the atmosphere, as indicated by the hygrometer, and by the repeated fall and large amount of rain, as well as the humidity, usual or occasional, of surrounding localities, have been noted. It is well known that, all things being equal, the degree of dampness indicated by the hygrometer increases in proportion as we approach the equator, where, as a general rule, we find localities most subject to the fever; and that the absolute proportion of dampness in the atmosphere diminishes as we advance towards either of the poles, where the yellow fever never prevails. It is well known, also, that the variations in the instrument in question are rarer and more feeble as we approach the line, where, in consequence of the active evaporation going on in those regions, amounting to some two lines a day, and increased by the prevalence of east winds, the hygrometer exhibits almost constantly a state of complete saturation.¹ In fact, with high temperature we have a high dew point, with little variation during the several months of the year. In the West Indies, judging from the few data I have before me, the dew point is high. In St. Vincent, the mean point is very near 70 (68.8); the highest point being in July (70.2), and the lowest in February (67.1). In Demerara, the mean of 1843 was, according to Blair (pp. 120–1), 73°.8—July, August and November reaching 74°.9. The mean in 1844 was 75°.1—May and July rising to 76°. The next year, when the mean could not be ascertained, April gave 76. In 1846, the mean was 74.4, May and June giving 76.2. These islands, like tropical regions generally, have, properly speaking, only two seasons, the wet and the dry; the former being commonly of about four months' continuance,² and characterized

¹ Cotte, *Leçons Élément de Physique, d'Astronomie, &c.*, p. 164; *Mém. de la Soc. Roy. de Médecine*, Année 1784–'5, p. 204; Péron, *Voy. aux Terres Australes*, 8th ed., ii. 112; Caillot, pp. 120–1; Cassan, *Mém. de la Soc. Méd. d'Emulation*, v. 27, 29, 163.

² Lind, p. 44.

by heavy and frequent showers of rain. This wet or rainy season is not composed of a continued series of rainy days; so far from this, a rainy spell of some twelve hours is generally followed by twenty-four or thirty-six hours of fair weather; and at times many successive days occur of dry weather, chiefly in August and September. But, during these intervals, the weather is distinguished by an almost insupportable sultriness and closeness; thick black clouds form, and discharge their contents in torrents, accompanied with thunder and lightning; and at all times, even during the dry season, the dampness is exhibited, not only by the hygrometer, but by the oxidation of metals, the rapid putrefaction of flesh, as well as by the quantity of dew which, in most places, falls during the greater part of the time. Towne calls attention to the fact of the existence of this humidity. "This is evident," he says, "from the great difficulty there is, in all the Caribbee Islands, to preserve their iron and steel from rust, their brass and copper from turning green, and their silver from tarnishing; all which mutations are seen upon the respective metals in a proportion not much short of that which they would have suffered by being exposed to bilge-water, as the sailors term it" (p. 10). Others have noted the fact.¹ "During the hours of seven to eleven, during a clear, calm night, I have known," says Evans, "the dew to fall so heavily as to have been sensible to the feeling, to have given to the pavement the appearance of a slight shower having fallen, and to have drenched the brushwood with moisture." The mean annual quantity of rain in those islands amounts to not less than 120 inches.

In our southern States, the humidity, though less than in the West Indies, is still very considerable. So far, hygrometrical observations are wanting to indicate the dew point of many of the localities where the fever usually shows itself; but the humidity is cognizable to the senses. The dews are heavy, even in winter, and mould and fungi are easily formed. This state of atmosphere exists principally during the summer and autumnal seasons, the winter being comparatively dry and serene. The rain there, as in tropical climates, falls at a particular season of the year; but the quantity in a given time is less abundant, and the number of fair days during the wet period is comparatively small.² The humidity of New Orleans, of Charleston, and other localities along the southern portion of our sea-board, is proverbial, and rendered greater from the peculiarity of their location; while at Natchez, and other points somewhat distant from the coast, and less surrounded by sources of evaporation, the indications of great moisture present themselves in a striking manner.³

The quantity of rain which fell in the first of these cities, in the four years, 1833–1836, inclusive, amounted, on an average, to $47\frac{35}{100}$ inches;⁴ and that of

¹ Maher, p. 835; Gilbert, p. 27; Bally, p. 349; Savarésy, pp. 165–7; Cassan, v. 31; Evans, p. 6.

² Climate of U. S., p. 62.

³ Gros, p. 5; Forry, p. 62; Thomas, p. 61; Dickson, Eclectic Journ., iv. pp. 10, 11; Simons, p. 16; Cartwright, ix. 5; Ramsay, ii. 56, 57; Girardin, pp. 43, 44.

⁴ Barton, Trans. of Med. Assoc., 1849, p. 608.

the years anterior to 1833, to $58\frac{6.54}{1000}$. The average quantity which fell in that city, according to the measurements of Dr. Lillie, quoted by Dr. Drake,¹ may be put down as $51\frac{2.04}{1000}$; but in the ten years, 1840–1849, inclusive, the average was $69\frac{6.32}{1000}$ —the extremes being 46.439 and 127.247. In 1853, the quantity was 52.62. Of this, the greatest quantity falls in summer and winter. In the afore-mentioned four years, 1833–1836, June gave an average of 6.17; July, 5.63; August, 5.24; and September, 5.79. The next greatest quantity fell in January, April, and November. In the other series, 1840–1849, the summer was the wettest season during six years, the winter during three years, and the spring during one year.²

The average dew point, at different periods of the day, in a series of eight years, was 62.56, viz: at sunrise, 61.16; at midday, 63.56; and at 9 P. M., 62.95. In 1850, the average was 62.58 (60.20, 64.46, and 62.75). In 1853, the average was 62.12. The amount is higher in June, July, August, and September.³ The quantity of moisture in the atmosphere—saturation being 1000—is, at sunrise, .905; at midday, .703; and at 9 P. M., .826. Dr. Barton well remarks that few countries have more elements of humidity than Louisiana, from large inland bodies of water. The whole area of the State is 48,972 square miles. Of these, 21,370, including flat lakes, are always more or less under water, and constantly subject to it. Of the whole alluvion, there is uncultivable more than one-half—say, 12,000 square miles—including shallow lakes. One-eighth of the State is constantly under water, and more than two-fifths subject to inundation.⁴

What has been said of New Orleans, may be said of all places around the Gulf of Mexico, where there is not only an impregnation of the atmosphere nearly up to the point of saturation, but where the absolute quantity of vapour is great. The dew point is always high, and its complement small. “In every season of the year,” Dr. Drake states, “the loss of a few degrees of temperature is sufficient to cause the condensation of vapour, and render the air moist” (i. 604). At Mobile, the average quantity of rain which falls annually is estimated at $66\frac{9.15}{1000}$ inches. In 1841, it amounted to 74.883. At Natchez, the quantity in 1840 was 48.48; in 1845, 53; in 1846, 61.79; and in 1847, 75.32; the average, according to Dr. Tooley, being 60.120 annually.⁵ At Baton Rouge, the quantity is 76.28.⁶ In Charleston, the mean quantity of rain, according to Ramsay, varies from 41.75 to 49.3 inches.⁷ From 1738 to 1742, Lining states it to have been 48.6 inches. In the ten years, 1750–1759, Chalmers found it to be 41.75.⁸ From 1797 to 1807, the observations of the Medical Society make the amount 49.3—varying from 38.6 (1800) to 83.4 (1799). In 1841, the quantity was 53.94; in 1844, 36.39;

¹ Works, i. 588.

² Fenner, Southern Rep., i. 28–31.

³ Barton, Rep. to State Med. Soc. of Louisiana, p. 48; Fenner, ii. 148; *Ib.*, Feb. of 1853, pp. 8, 9; *Ib.*, Trans. of Med. Assoc., vii. 426.

⁴ Rep. to State Med. Soc., p. 10.

⁵ See Amer. Alm., and Drake, i. p. 588.

⁶ Allen, Vital Statistics of East Baton Rouge, N. O. Journ., viii. p. 69.

⁷ Hist. of S. Carol., ii. 62, 63.

⁸ Climate of S. C., plate at p. 42. In the text, Chalmers states it to have been 42.03.

in 1845, 46.44; in 1846, 44.33; in 1847, 47.83; in 1848, 43.40; in 1849, 30.69; in 1850, 23.89; and in 1851, 33.14. As regards the dew point, it stood as follows in the last-mentioned seven years:—

1845	55.22 $\frac{1}{3}$	1849	57.89 $\frac{8}{12}$
1846	56.76	1850	58.79 $\frac{3}{12}$
1847	57.78 $\frac{7}{12}$	1851	58.07 $\frac{3}{12}$
1848	57.89 $\frac{2}{12}$						

We have seen, in a former chapter, that in our climate the humidity, as indicated by the hygrometer and the frequency and quantity of the falling rain, is, as might indeed be expected from the position of the country, much inferior to that of equatorial regions; for while the mean dew point of St. Vincent is 68.8, that of New York is only 38.7; and while the mean quantity of rain in the West Indies is 120 inches, that of Philadelphia is only about 43.

But although, as stated, the dew-point in our climate is many degrees below the temperature of the atmosphere—the two rarely coinciding entirely or nearly—the daily range with us is much greater, varying in some places in June from 23 to 71°, and approaching to within a few degrees of the temperature. The coincidence, during the summer months of some years, is nearly if not quite complete, and continues long, when the weather becomes extremely humid, and, assuming the peculiar character denominated *close, sultry*, or *muggy*, gives rise to that oppressive influence which is well known in the country.¹ These observations coincide with those made in those portions of Europe where the fever has prevailed, for there the humidity, though far less striking than in tropical regions, is nevertheless considerable during the hot summer months; and, in some places—as Gibraltar—the atmosphere during certain months becomes, under the influence of peculiar winds, as much saturated with moisture, as in almost any portion of the globe. The mean quantity of rain in the same place has varied, in fourteen years, from 15.3 inches; (September 1800, to September 1801), to 73.63; (1796–1797) giving an average for the fourteen years of 34.72.²

From an early period in the history of the yellow fever, a tendency, arising from a consideration of the preceding facts, has existed in the minds of some writers to ascribe to atmospheric humidity a large share of agency in the production of the disease. By not a few it is considered, when combined or alternating with high atmospheric heat, as the principal or efficient cause of the disease. Such will be found to be the views entertained by Poissonnier (p. 50), Lefort (p. 9), Catel (pp. 7, 8), Gros (p. 5), Dickson (*Ed. J.*, iv. 10, 11), Simons (p. 16), Jones (*Boston J.*, ii. 376), as well as by the Academy of Medicine of Barcelona (*Rayer*, p. 50), the reporters on the epidemics of New Orleans in 1819 and 1839, &c.

Lind, also, in some passages of his work on hot climates, appears to have entertained similar sentiments; but elsewhere he speaks of an impure air as giving rise to the disease (p. 9). This opinion is not exclusively applied to the particular form of fever under consideration, but is entertained by some

¹ Forry, *Climate of United States*, p. 112.

² Sir James Fellowes, p. 466.

as regards the paludal fevers of Italy, and has been advanced by some writers on the African fever,¹ and applied by Fordice (pp. 146, 151)—not without some subsequent qualifications—to fevers generally. Others, again, regard the humidity indicated by the dew-point or otherwise, as the efficient cause *par excellence*—the disease resulting from the deficient evaporation from the body produced by a high dew-point, by which the vapour which should escape, as well as the heat necessary for its evaporation, is retained. By them, malaria is identified with a high dew-point.²

By a different set, atmospheric humidity, though not viewed, even when conjoined with heat, as the efficient cause of the disease, is nevertheless regarded as an active agent in its production, aiding in the evolution of the efficient cause, and placing the system in a condition to be morbidly affected by it.³

These views—even that which recognizes in atmospheric humidity, combined with heat, the essential cause of the yellow fever—derive support from the connection which we have found to exist generally between a moist climate and the prevalence of the disease in latitudes and localities where that condition of atmosphere exists either throughout the year or at particular seasons; for, if the fever affects more particularly such latitudes and localities, while it spares others differently conditioned, the inference is natural, that the humidity thus existing, and the fever which so usually attends it, stand in the relation of cause and effect. They derive support also from the fact that, in the West Indies and elsewhere, the fever usually breaks out during the dampest portion of the year, or after the fall of considerable rain, and also in seasons of great humidity; and that everywhere it so truly requires for its development some degree of rain or humidity, as not to prevail when these fail to show themselves. A hot and humid atmosphere has long been recognized as a prolific source of febrile complaints. Hippocrates has somewhere remarked, that the constitution of the air that preceded the pestilential fever was marked by great heats, much rain, and southerly winds. Galen takes notice “that no other than a moist air brings the plague.” Lucretius is of the same opinion, maintaining that these diseases “either come from the air, or rise from the earth.”

. . . Ubi putrorem humida nacta 'st
Intempestivis pluviisque et solibus icta.⁴

Whatever views we may adopt respecting the etiology of the yellow fever, no one can feel disposed to contest the reality of the connection in question, at least under ordinary circumstances. In the West Indies, and other portions of the tropical regions visited by the disease, the rainy and humid season has generally been found that at which it prevails; and there are not

¹ Edinb. J., lxix. 117; Rept. on Sickness of Brit. Army, p. 26.

² Hopkins, Lond. and Edinb. Philos. Mag., 3d series, No. 86.

³ Savarésy, p. 237; Desportes, i. 17; Pugnet, pp. 332, 342; Waring, p. 21; Valentin, p. 86; Barton, Rept. of San. Commis. 1853, p. 292, &c.

⁴ Luc., lib. 6, ver. 1098.

wanting facts to give countenance to the opinion of those who maintain that the fever seldom appears in an epidemic form during the dry season.¹

The epidemic at Stony Hill broke out about the middle of February, after a long continuance of dry sultry weather, when the ground was excessively parched, and as the drought increased so did the disease; whereas, at Up-Park Camp, it broke out in the month of June, after the rains had commenced, and continued with unabated severity during a period when more rain fell than had been witnessed for twenty years previous—and as the rain ceased so did the disease. (*Arnold*, p. 149.)

The same occurs as regards the fever of the coast of Africa. “When it was attempted to colonize the Bulam Islands under Captain Beaver, in 1792, the mortal and destroying fever which prevailed among the emigrants took place in the wet season; and the same coincidence has been noticed at subsequent periods.”² In general, on the African coast, when the rains commence, sickness also commences; and the smokes which take place during or after the rains are always more or less productive of fever. So closely do these dense vapours appear to be connected with fever, that it is supposed by common observers, that when carried seaward, which they occasionally are, they bear with them the active exciting principle of fever. Of this conveying power, Dr. Bryson thinks there is no proof. But the fact of coincidence is, we believe, generally, if not universally recognized.³

The following remarks of Lind, relative to the African coast, will apply to many other places: “The large rivers in the dry season being confined within narrow bounds, leave a great part of their channels uncovered, which, having their moisture totally exhaled, become a solid hard crust; no sooner do the rains fall than this long-parched crust of earth and clay gradually softens, and the ground, which before had not the least smell, begins to emit a stench, which in four or five weeks becomes exceedingly noisome. At this time, the sickness is generally most violent.”⁴

In tropical regions, the sickly or fever season corresponds with that of the rains. In the French colonies, it is denominated *hivernage*. The latter is the period in which the sun heats the portion of the zodiac situated on the side of the equinoxial line where the rains prevail. The *hivernage* is consequently in reality the summer of such sections of tropical regions where it is observed. When the sun crosses the line, the rainy season necessarily changes sides; and with the accession of wet weather we have the advent of fever.⁵

In Rio Janeiro, in 1853, it was observed, according to Dr. Candido, that the force of the epidemic was greatest in those months in which humidity

¹ Savarésy, pp. 236, 257, 292; Pugnet, pp. 341–2; Moseley, pp. 10, 11; Chisholm, i. 146–7; Belcher, Edinb. J., xxiii. 248; Dariste, p. 33; Davidson, *Reposit.*, viii. 248–9; Catel, p. 7; Desportes, i. 17; Dazelle, p. 10; Humboldt, p. 765; Pugnet, p. 342.

² Edinb. J., lxiii. 448.

³ *Climate and principal Diseases of the African Station*, p. 198. See also Edinb. Journ.

⁴ *Hot Climates*, p. 54.

⁵ Becquerel, *Des Climats et de l'Influence qu'exercent les Sols Boisés, &c.*, p. 124.

was most prevalent and marked.¹ At Puerto Cabello, Dr. Lacombe states that "it is a constant and general rule that the place becomes entirely free from disease, and the healthiest in the world when strong heat, combined with total absence of rain and dampness, prevails, the atmosphere then being entirely dry." On the contrary, "during the last two years, 1852-53, the weather was very hot, and very damp, with frequent small rains—during all this period yellow fever prevailed."²

The connection of humidity with fever—the necessity of the former for the production of the latter—is exemplified by the occurrences at Tampico in 1836. The rains commence there in July, and are followed by intense heat. This is the period of yellow fever. In the above-mentioned year the rainy season commenced two months later than usual, and there was a corresponding delay in the appearance of the disease.³

In Bengal, the rainy season commences in June, and continues until October; the remainder of the year is healthy and pleasant. During the rains, the rich and fertile country is almost quite covered by the overflowing of the Ganges, and converted, as it were, into a large pool of water. Diseases rage among the Europeans in the months of July, August, September, and October, consequently during the rainy or wet season.⁴

The fever of Key West has, in like manner, a predilection for the wet season.⁵ The epidemic of Savannah, in 1820, arose, and for a time prevailed, during extreme moisture of atmosphere,⁶ as did also that of ordinary autumnal fever of 1817. Dr. Dickson, of Charleston,⁷ informs us that the yellow fever of that city appears more frequently in very wet than in dry summers; while some at least of the epidemics of our Atlantic cities have broken out during the prevalence of excessive humidity. Dr. E. H. Smith,⁸ and Dr. Bayley (pp. 52, 123), in their accounts of the fever of New York in 1795, dwell on the extreme dampness and sultriness of the weather during that season. The epidemic of New Orleans, in 1853, came on and continued during wet weather (*Barton and Fenner*). Dr. Barton concludes from his meteorological observations, that it has never been otherwise in that city (pp. 294-5).⁹ The

¹ Rept. of Sanit. Commission of N. O. 1853, p. 186.

² *Ibid.*, p. 294.

³ Goupilleau, *Bulletin de l'Aead.*, i. 456; *Ibid.*, iii. 306.

⁴ Lind, *Hot Climates*, p. 91; Shannon, *Practical Obs., &c., on Diseases of Hot Climates*, p. 74; J. Johnson on *Tropical Climates*, p. 59.

⁵ Duprès, *Am. J.*, iii. 280, N. S.

⁶ Waring, pp. 20-1; Daniel, p. 39.

⁷ *Philad. Med. and Phys. J.*, iii. 265.

⁸ Webster's Coll., p. 75.

⁹ Professor Blodget, in a highly interesting communication to Dr. Barton relative to the hygrometrical peculiarities of portions of the yellow fever zone of 1853, remarks that South Florida only was profusely rainy in June, except for the last half of the month, when New Orleans became remarkable for frequency of rains. In July, Texas was very dry—New Orleans was the reverse, with tropical frequency of rains. In Northern Florida and South Carolina the rains were heavy, though not unusually frequent. In August, the tropical rains of New Orleans continued, and began at the close of the month in lower Texas. In September they spread over the Gulf coast, east and west of New Orleans, and diminished at that point. In October they were continued on the Rio Grande, and at Bermuda, and other islands, and over most of the Gulf coast also. The

humidity experienced at Leghorn, in 1804, would appear to have been very great;¹ and it should be remembered that the Levanter wind, under the influence of which the disease usually, if not always, prevails at Gibraltar and Malaga, occasions a considerable moisture in the atmosphere.²

It may be adduced in farther support of the action or essential agency of humidity in the production of the yellow fever, that the disease appears to possess a decided affinity to such localities as are, from their position, more prone to dampness—prevailing, as it does, in a more especial manner along the sea-coast, at the outlet of rivers, along water courses, and lagunes, and in the vicinity of swampy, low, and level ground. Instances of this predilection for localities of the kind are frequent in tropical climates, and are not rare in this country. The cities of New Orleans and Charleston are much to the point, and whenever the disease has broken out in other cities differently located, it has almost invariably done so in the vicinity of the water.

We have seen that in countries which, during the dry season, remain free from the disease, the latter appears soon after the advent of rain, and continues to prevail until the return of dry weather. We may now mention that excessive droughts at the season when the fever usually appears, have at times been found to prevent its development. Such was the case in Charleston in the year 1752, when, during the months of May, June, July, and August, the heat was excessive, but attended with a remarkable deficiency of humidity. The season remained very healthy so long as the weather was hot and fair.³ In 1818, heat and dryness existed in excess in the same city, yet no fever occurred, except sporadically.⁴ Macarthur remarks that, at Barbadoes, there is little or no fever during very dry seasons.⁵ Humboldt states that at Vera Cruz, excessive dryness puts a stop to the fever (p. 765); Desportes (i. 121), speaks of the almost complete exemption of the Cape (St. Domingo), in 1742, as being caused by the *extreme* dryness of the season. Similar statements are made by Ferguson,⁶ and Savarésy (p. 236). In Demerara, in

yellow fever began on the Rio Grande with these rains in August, and continued till they ceased in October. It began in other parts of Texas with the same conditions, and so at Mobile, “continuing with their unusual continuance.” Prof. Blodget found also that the mean humidity, or percentage of saturation, was very large in New Orleans, as also in all parts visited by the fever. Dr. Barton remarks, in reference to the observations of Prof. Blodget, that they perfectly correspond with the variable outbreak of the epidemic in every part of this extended region (S. W.). In every instance where the facts are known, great heat and high saturation were the predominant conditions for the prevalence of the disease; and it was often remarked, that the return of these conditions reproduced the fever two or three times.¹

¹ Tommasini, § 162.

² At Rome, it is found that if the season is dry, there are comparatively few cases of fever; if, on the other hand, much rain falls, the hospitals are much crowded; Clark, *Med. Notes on the Climate, &c., of Italy*, p. 80; Bailly, *Fièvres Interm.*, pp. 130-1.

³ Chalmers, i. 19, 22.

⁴ Sheut, p. 104.

⁵ Fever of Barbadoes (quoted by Diekson), *Edinb. J.*, xiii. 47.

⁶ *Med.-Ch. Tr.*, viii. 130; Lind, p. 47.

¹ *Rept. of Sanit. Cond. of N. O. in 1853*, pp. 268, 273.

1825, a most severe drought prevailed—not a drop of rain fell for ten months—everything was parched up, and water was very scarce, occasioning great mortality among cattle—yet the season was healthy, and no fever occurred.¹

Towne, a long time ago, remarked that when rain succeeds to a long and intense drought, and pours down in an impetuous and excessive degree, we are then to apprehend a terrible invasion from the whole tribe of acute and inflammatory diseases—among which he includes putrid, burning, malignant fevers (pp. 7, 8); but that when rain happens to fall moderately, and at the usual time, we may expect that the following dry months will prove salubrious and healthy (p. 7). Warren, in like manner, called attention to the necessity of rain for the production of the yellow fever, and to the correcting effect of long and severe droughts—remarking, that “a continuation of dry and sultry weather, has been so far from giving any aggravation to it, that it has seemed to repress it, and make it lie more lulled and dormant, until the returning rains and a moist atmosphere had set it at liberty to exert its rage anew” (p. 8).

These remarks of Towne and Warren, who both practised at Barbadoes, are confirmed by the result of experience in other West India Islands, and on the west coast of Africa.² Similar results have been obtained in this country and Europe. During the year 1801, the quantity of rain that fell at Gibraltar amounted to little more than half the usual average in Great Britain, and yet the fever did not prevail.³

In 1810, Dr. Doughty states, there was a continuance of drought and heat for three months, and the rains set in about the autumnal equinox; then, and not till then, did fever, of the order we are speaking of, develop itself, both at Cadiz and Gibraltar.⁴

At New Orleans, in 1832, the summer was very dry, but healthy. The fever (and soon after the cholera) broke out in September, after the fall of much rain.⁵ Speaking of Mobile, Dr. Lewis says it is usual for forty days of very dry, cloudless weather, to put an end to yellow fever epidemics. If the atmosphere continues laden with the same amount of moisture, or anything approaching to it, which invariably exists at the time the disease makes its appearance, there is no cessation of it until after a decided frost.⁶

These facts cannot fail to lead to the inference that atmospheric and terrestrial humidity, arising from active evaporation or the fall of rain, is an efficient agent in the production of the yellow fever. They show that the fever frequently occurs during rainy seasons, and is, indeed, ordinarily encountered in damp localities, where rain is common and falls abundantly; where the

¹ Davy, Notes on Blair, p. 52.

² Lind, p. 150; Davidson, as quoted by Rush (iv. 155), for Fever of Constitution Hill, Barbadoes, in 1794; Humboldt, p. 765; Ralph, ii. 60; Tullock, Sickness and Mortality in British Army in West Indies, p. 53.

³ Report on Sickness of British Army, p. 4.

⁴ Doughty, pp. 187, 188.

⁵ Halphen, pp. 42, 43.

⁶ Lewis, Fever of Mobile in 1847, N. O. Journ., July, 1848, vol. v. 38.

soil, previously dry, has been rendered wet by some of the causes referred to, rain, freshets, overflows, &c. ; or where the dew-point is high, and vesicular humidity generally or often noticed, or is considerable at the time. That it often makes its appearance at the first set in of the rains, the country prior to this having been dry and healthy ; and that hence a certain degree of humidity appears to be necessary to the development of the disease—the *wetting*, in contradistinction to the *drying* process, proving injurious to health by exciting the fevers in question—are facts which no one need be told. It is true, also, that in many instances, if not in all, long-continued and thorough terrestrial humidity, or saturation of the soil, has been found to precede epidemic manifestations of yellow and some other forms of malarial fevers ; and, on the other hand, that a complete absence of such humidity, a thorough desiccation of the soil to a great depth, as well as an excessive dryness of the atmosphere, are inimical to the production and continued prevalence of those diseases. Facts to that effect have been observed and recorded in this country, in South America, in the West Indies, in Africa, Europe, and Asia. They are true as regards both ordinary autumnal or periodic fevers, and malignant yellow fever. In a word, the humidity of the West Indies, of most parts of our southern States and of the coast of Africa, is proverbial ; and we know, not only that these are all fever regions, but that the outbreak of the disease coincides often with a wet season, and with the manifestation of increased moisture.

But, before we can regard moisture, as some have done and continue to do, as the essential or necessary cause when combined with high atmospheric heat, it should be shown that without it the disease cannot exist epidemically or sporadically, that the disease always attends whenever it prevails to a considerable extent, and that the risk of the danger will be proportioned to the extent at which it exists. Now, it will be found that so far from this being the case, the disease, on some occasions, in other localities, and, at other times in the same place, has appeared under circumstances which preclude the possibility of viewing humidity in the light above mentioned ; for facts are far from being wanting to show that the fever spares places remarkable for humidity, and, on the other hand, prevails at times—and extensively too—under hygrometrical conditions of atmosphere very different from that above mentioned ; and though the complete desiccation of the soil puts a bar to the disease, the latter may exist under any condition short of such dryness. It is sometimes arrested in its epidemic course by the very means that are supposed to be by themselves, and without concurrent aid from other conditions of atmosphere, instrumental in its production.

Heberden, many years since, remarked that the air is often fully saturated with moisture, and could not be more filled by the vapours arising from a chamber covered with water ; and yet, he adds, neither is any epidemical distemper produced by it, nor are those remarkably aggravated, with which the sick happen at that time to be afflicted. The air from rivers and from the sea is probably more replenished with vapours than inland countries cleared of their woods—yet they are generally healthier.

This statement of the celebrated London physician, which applies to the climate of Europe, and is fully borne out by the result of experience in Holland, where fevers cease when the soil is completely submerged,¹ has been confirmed in tropical climates and such other places where the yellow fever prevails. Dr. Ferguson has shown, from undeniable facts, that mere humidity from fresh water is not productive of fever.² He remarks that water kept in stone tanks, or anywhere, so that it can be preserved in bulk without being absorbed by the surrounding soil, is not productive of disease. One of the healthiest quarters in the West Indies, according to that able physician (p. 342, 343), is that of the field officers on Berkshire Hill, St. Vincent, the bedroom of which is placed immediately over a deep stone reservoir of water. A block-house in Demerara, reported to be one of the healthiest quarters there, is similarly situated; and it is known to all that the fresh water laid in for a ship's crew, however much in contact with their sleeping-places, produces nothing like marsh fever amongst them. Similar statements relative to the innocuousness of fresh water will be found in the works of H. McLean,³ Lempriere,⁴ Bancroft,⁵ Belcher,⁶ R. Jackson,⁷ Pugnet,⁸ Dickson,⁹ Drake, and others¹⁰

Whatever may have been the case in former days, it would appear from the reports of Dr. Cornuel, that, since 1838, the yellow fever has, on several occasions, broken out or prevailed at Basseterre (Guadaloupe), during the driest and coolest season of the year, and ceased during the wettest.¹¹

Major Tullock, in his excellent Report on the sickness of the British troops in the West Indies, after remarking that the inference of the connection as cause and effect between humidity and disease, derives plausibility from various facts in the history of tropical fevers, especially its great prevalence along the sea-coast, at the outlet of rivers, and in the vicinity of swampy level ground, adds that this hypothesis seems at variance with the facts noticed: "For, if the mortality of the troops depended materially on the influence of moisture, we might expect it to attain its maximum in those stations where the fall of rain was the greatest; whereas, the average mortality of the troops in Jamaica is at least double that which prevails among those in British Guiana, though the quantity of rain which falls in that island is little more than half as great; and in the preceding pages there are adduced

¹ Ferrus, in Pallas, pp. 328-9.

² Med.-Ch. Tr, viii. 129.

³ An Inquiry into the Nature and Causes of the great Mortality among the Troops at St. Domingo. London, 1797, pp. 24, 25.

⁴ Practical Observations on the Diseases of the Army in Jamaica in 1792, '97, ii. 5, 6.

⁵ An Essay on the Disease called Yellow Fever, p. 243.

⁶ Edinb. Med. and Surg. Journal, xxiii. 47.

⁷ A Sketch of the Hist. and Cure of Febrile Diseases, p. 11.

⁸ Mémoire sur les Fièvres de Mauvais Caractère, pp. 342, 343.

⁹ New York Med. Journal, Sept. 1841, p. 175.

¹⁰ Diseases of the Valley of the Mississippi, &c., i. 610, 711.

¹¹ Cornuel, Rapport sur les Maladies Observées à l'Hôpital de la Basse Terre pendant l'Année 1843; Ann. Marit., 1844, ii. 735.

many instances in which epidemic fever has broken out, and raged with great violence, at a period when no rain had fallen for several months; nay, in some stations a dry, in others a wet season, is looked on as the most unhealthy—an anomaly not likely to occur if excess of moisture was uniformly an essential cause of insalubrity.”¹

Madeira, the Canary Islands, not far from the coast of Africa, the islands of St. Antonio and St. Nicholas, are healthy, though humid; while Fernando Po, Princes, and St. Thomas Islands, not far from these, are, like Senegal, unhealthy. Barbadoes, St. Christopher, and Bermuda, though at no great distance from fever countries, are healthy; and, while St. Lucia is unhealthy to a degree, Pigeon Island, which is not less humid than the opposite coast, is free from febrile diseases. Dr. Rollo calls attention to the circumstance, that some of the troops that landed in 1778 at St. Lucia, and were encamped at the Vigie, were there exposed to fatigue, *constant rain*, and changes from heat to cold, and yet they were not sickly. They were thence removed to the windward of the Carenage, where, to the former causes, were added marshy exhalations. They then became subject to fevers, from which they were once more freed by resuming their former position.²

In speaking of Fort Augusta and Port Royal, Jamaica, Dr. Hunter remarks that simple moisture is harmless, “at least as far as relates to the production of fevers, of which the two last-mentioned places may be given as examples, for they are nearly surrounded with water on all sides. It is true,” he adds, “the air is perfectly clear, yet it must be loaded with moisture in consequence of the great heat of the sun acting upon the water. But the vapour arising from water is harmless, even when rendered more an object of our senses, by being condensed into fogs and clouds. The parish of St. Thomas in the Vale, is every night covered with a thick fog, owing to the rivers which pass through it, sending forth vapours, which, in day time, are perfectly transparent; but towards evening, by the cool air coming from the neighbouring mountains, they are condensed and remain visible till next day’s sun disperse them, without, however, being at all unwholesome.”³

It should be remembered, also, as is well remarked in a work of great authority, that excess of moisture is not confined to the West Indies, but is a general characteristic of all tropical regions; and were it, even when combined with heat, so productive of fevers in the western hemisphere, the same effect might be expected to ensue from it in the east; whereas, on the contrary, the Malabar coast, which is deluged by rain for six months in the year, is generally one of the most healthy quarters in the Madras presidency. The author also calls attention to the fact that “there are numerous instances of two adjacent islands, or even of two contiguous sections in the same island, being subject in an equal degree to the operation of these agencies (heat and moisture), and yet, while the one has been desolated by the ravages of fever, the other has been enjoying a degree of salubrity equal to

¹ Page 101.

² Diseases of the Army at St. Lucia, p. 67.

³ Observations on the Diseases of the Army in Jamaica, pp. 13, 14.

that of Great Britain.”¹ Furthermore, it is remarked that if the mortality from febrile diseases depended materially on the influence of moisture, we might expect it to attain its maximum in those stations where the fall of rain was the greatest; whereas, the average mortality of the troops in Jamaica is at least double that which prevails among those in British Guiana, though the quantity of rain which falls in that island is little more than half as great.²

If humidity, combined with heat, were the primary cause of the yellow fever in tropical and other climates, the disease ought to be found to prevail more extensively and severely in seasons characterized by an excessive degree of it, or at the wettest portion of a wet season, and to be absent in periods differently circumstanced. But such is not the case everywhere and at all times; for seasons present themselves when the fever rages with much less violence, or indeed scarcely shows itself, if at all, though the atmospheric humidity or the quantity of rain which falls is very great. Speaking of St. Domingo, where he practised many years, Desportes says: “I have observed that after prolonged and abundant rains, tertian, double tertian, and continued (or rather contravening) fevers were the most common diseases.” “I have likewise noticed that, under these circumstances, contagious diseases (the fever of Siam) were much less common, and that the reverse occurred when the rain fell in small quantity and was preceded and followed by great dryness. Now, the summer of this year (1737) having been very rainy, these diseases were less common and contagious than in the preceding years, 1735 and 1736” (i. 86, 87). The season of 1745 was remarkably wet, but nevertheless free from the yellow fever (p. 145). Seasons in which rain prevails are found more healthy than when dry weather has continued for any length of time in St. Domingo.³ A writer of our own times, who has noticed the fever at Dominica, remarks that varieties of seasons do not appear to have much influence in producing the disease; for it will break out and extend its ravages when perhaps there is no unusual change in the weather.⁴ During the season of 1841, when the fever raged in that island, the rain was not as heavy as in ordinary seasons.⁵ Dr. Macarthur, of Barbadoes, quoted by Dr. J. H. Dickson,⁶ remarked that in years when the rain was abundant in June, July, and August, the fever did not appear until September, October, and November; but when June, July, and August were comparatively dry, it commenced at an earlier period, and was generally violent when great heat followed partial rain. “It has been asserted that, in Barbadoes, the weather which is most favourable to the crops (when a large proportion of rain falls) is also most favourable to health; a proposition, it may be, commonly true, and yet not without exceptions.”⁷ Hillary says: “I have always observed that the symptoms of this, as well as most other fevers, are generally more acute, and the fever usually higher, in a very hot season, especially if it was preceded by warm, *moist* weather.” In the

¹ Report of Sickness, &c., pp. 101, 102.

² Ibid., p. 101.

³ H. McLean, p. 25.

⁴ Imray, Edinb. Journ., liii. 93.

⁵ Ib., lxiv. 331.

⁶ On the Causes of Yellow Fever, Edinb. Journ., xiii. 47.

⁷ Davy, Notes on Blair, p. 52.

United States, the disease spares, usually, our cities during very rainy and damp seasons.

The fever made its appearance in Charleston in 1728. Hewatt informs us, in his *History of South Carolina*, that the summer was uncommonly hot and dry; the beasts suffered from the want of water, and the fever, raging with violence, swept off a number of whites and some blacks. "The calamity was so general, that few could grant assistance to their distressed neighbours" (i. 317). In the same city, the atmosphere was more humid before 1792 than after. In this last-mentioned year, a new era of the yellow fever commenced, when the moisture of the atmosphere, which had been very great, gave way to an opposite condition—the air being "not only much less damp in the number of degrees, but seldom damp, and generally dry." The fever appeared in 1794, and in several years successively, generally making its first attack within a fortnight after heavy rains, if succeeded by hot weather and light west and southwest winds, not sufficient to refresh the atmosphere by dispersing the exhalations, and not even carrying off the perspiration. These occurrences were so frequently repeated, that, when a condition of the atmosphere of that kind prevailed, the appearance of the fever was predicted, and always followed. Heavy rains, if followed by fresh, cooling winds from the east—which pass directly over the sea—were not succeeded by fever.¹ In 1796, 1799, 1801, and 1817, fever broke out at Charleston (but only there) during the continued fall of heavy rains in warm, sultry weather; but why?—Cellars were filled, and in many instances remained so; water was left to stagnate, in contact with rotten wood and other substances in a state of decomposition.² If the season of 1817 was hot and moist, that of 1824 was hot and dry, and yet fever prevailed in both.³ The month of June, 1839, during which year the fever prevailed extensively in the same city, was uncommonly hot, and the whole of the summer was remarkable for the great drought as well as high temperature. All the southern country experienced the same weather, and the principal cities south of Charleston were invaded with yellow fever.⁴ Of the years 1845 to 1852, inclusive, the last and 1849 were the only ones during which the yellow fever prevailed at Charleston.

In 1852, the quantity of rain which fell during July amounted to 6.95 inches; August furnished 4.21, and September, 12.27; making a total of 23.43. In the year 1849, July gave 6.35; August, 5.16; and September, 6.27; total, 17.78. But while the quantity in the first of these two sickly seasons exceeded considerably several of the healthy years of the series, it was only 0.68 greater than the quantity in 1847, when no fever prevailed; while the amount that year (22.75) and in 1845 (19.71) exceeded that in 1849 by several inches. So far as Charleston is concerned, therefore, though, as

¹ Johnson, *Some Account of the Origin and Prevention of Yellow Fever*, Charleston Journ., iv. 154-6.

² *Ib.*, Charleston Journ., iv. 162, 163.

³ Simon, *Observations on the Yellow Fever, as it occurred in Charleston (S. C.)*, South Carolina Med. Journ., i. 5.

⁴ *Ib.*, Report to the Board of Health, p. 10.

remarked by Dr. Hume, from whom these facts are derived, the yellow fever has never appeared in a non-pluvial season, showing that some degree of humidity is required, yet, from the facts mentioned, and many others that could be gathered from Dr. Chalmers's account of the climate of South Carolina, pluvial seasons are not necessarily accompanied by fever. In 1752, 27.45 inches fell during July, August, and September; and yet, notwithstanding, the season was healthy.¹

Dr. Hulse informs us that the fever does not usually prevail at Pensacola during the wet season. In 1841, the months of August and September were excessively dry.² In 1819, at Natchez, the rains were more copious during the summer than those of the preceding nine years, except 1812. They amounted to 23.43 inches, or 10.26 inches more than the mean of the preceding nine years; they came within 1.56 inch of the year 1812, and rose above 1813, 3.04 inches; finally, they exceeded the rains of 1817, 11.11 inches—that is, were nearly double. But, on the other hand, it will be found that in the summer of 1813 nearly as much fell, that in 1812 there was a greater fall, and yet that in neither year was there any fever. “Still further, the fever prevailed in 1817, when the quantity of rain was little more than half that of 1819.”³

We have seen that in the ten years, 1840–1849 inclusive, the average annual quantity of rain at New Orleans amounted to 69.632. Now, during the year 1847, the quantity that fell amounted to 100, and thereby would seem to lend support to the opinion that an excess of humidity is a main cause in the production of the disease. But, on the other hand, we find that in 1848, when the fever prevailed to a moderate extent, the quantity far exceeded that of the preceding year—by 37 inches; and in 1846, when comparatively few cases occurred, the amount was 98.474 inches. Let it be observed, besides, that in several years, when there was no yellow fever at all, the quantity did not differ materially from what it was at other times when the disease assumed the character of a wide-spreading epidemic. Compare, for example, 1840 and 1845, when there was no fever, with 1841, when it spread extensively:—

	1840.	1841.	1845.
Spring	11.874	12.887	11.642
Summer	15.205	8.787	9.208
Autumn	11.543	8.592	15.175
Winter	8.429	18.976	10.797
	<hr/> 47.051	<hr/> 49.202	<hr/> 46.822

The excess in 1841, being in the winter months, would have but little influence in the production of the fever. In the summer and autumn, when a different result might be presumed to obtain, the quantity was larger during the non-sickly years. In 1853, when the epidemic was most extensive, the quantity was 62.620—spring, 14.660; summer, 7.465, autumn, 23.748; and

¹ Hume, *On the Causes of Yellow Fever*, Charleston Journ., viii. 64.

² Maryland Med. and Surg. Journ., Jan. 1841, p. 392.

³ Drake, ii. 264.

winter, 16.767—which was much less than during the mild epidemic year of 1849, when there fell 78.283, and still more so than during the moderate epidemic year of 1846, when it exceeded 98 inches.

Dr. Nott, of Mobile, remarks that the origin of yellow fever, at different epochs, is entirely independent of appreciable meteorological changes; though, after it has once *started*, it progresses more rapidly in dry than in wet weather. “No one can pretend to predict the occurrence of yellow fever the day before the first case appears. In Mobile, we are often taken by surprise.”¹ Mobile, in 1819, suffered much from yellow fever. Prior to the 28th of July, there had been a severe drought. From that day to the 10th of September, rain fell incessantly, and often descended in torrents. The whole of the low ground around the town was covered. On the evening of the 10th it cleared off; wind northwest, with hot sun, and so continued for sixty-six days, to November 16. During the whole of this long period, there was scarcely rain sufficient to lay the dust in the streets. During the dry weather of July, a number of violent cases of bilious fever occurred among persons unaccustomed to the climate, and some of a more questionable character. Cases of yellow fever continued to show themselves in August, but did not fairly become epidemic until after the atmospheric change of the 10th of September, when it made its appearance in different parts of the town, in all its fearful, deadly garb.²

At New York, about the middle of May, 1795, the weather became very warm and the sky serene; the heat increased, and a clear sky continued, with very little variation, until the 15th of July, when it became sultry and changeable, varying, by short intervals, from rain to sunshine. It continued so, with little variation, though constantly very warm, for about two months. In 1798, the warm weather commenced about the middle of April, and continued to increase, with some intervals of rain, until about the 10th of August—nearly four months. During that time, the sun heated the earth to so high a degree, that it made even the nights almost intolerable. The sky then became cloudy, and seemed much perturbed; the elements appeared wild, much thunder ensued, and afterwards a wild and dreadful shower of rain; the weather became variable; the earth was often shaded with clouds, and again sprinkled with rain, and the pestilence spread throughout the city.³ In both these years, the disease made its appearance about the close of July; and though there is little doubt that the atmosphere became damp, in 1795, after the 15th of that month, and in 1798, after the 10th of August, the cause of the pestilence must have been formed during the long dry spell which preceded.

The heat in New London, in the summer of 1798, exceeded, both in intensity and duration, what had been known within the memory of the oldest

¹ N. O. Journ., iv. 571.

² Report of the Committee appointed to investigate the Causes, &c., of the extraordinary Sickness and Mortality in the Town of Mobile, in Letters, &c., on Fever of Baltimore, pp. 202-4.

³ A Physical Inquiry into the Origin and Causes of the Pestilential Fevers (New York, 1798), pp. 22-25.

inhabitants, and was attended with an unusually dry atmosphere—no thunder showers, &c.—for five or six weeks consecutively, with the exception of a few nights.¹

“On the Spanish main, the coast all along is very low and swampy, with a high range of mountains in the interior. The seasons consist of two, the dry and the rainy. During the latter, which generally lasts from December till June, heavy showers of rain fall, which continue, generally speaking, for an hour or two. The sun shines out with tremendous power, the heat is excessive, and, acting on the half-inundated soil, a bluish haze arises off the ground, carrying to the door of every inhabitant the seeds of the terrible malady. The earth is no sooner dry, than down comes the rain again in torrents, followed by the same deleterious atmosphere as before. Such is the state of things which generally lasts for six months in the year. This is considered *the unhealthy season par excellence*. However, the most unhealthy part is the termination of the wet and the commencement of the dry season.”²

In 1821, the city of New Orleans remained almost entirely free from the fever, though it rained almost constantly during the whole of the summer and autumn.³ In 1846, the season in the same city was a very wet one, the rains continuing until the 20th of September; but, though the fever prevailed, it did not assume the character of a general epidemic.⁴ The average quantity of rain at Gibraltar is $25\frac{1}{2}$ inches; in 1796, the quantity that fell amounted to $73\frac{1}{2}$ inches, and yet the season was healthy.⁵ It may be remarked that if humidity is the principal or remote cause, it ought to be so everywhere. The Levanter wind at Cadiz, under the prevalence of which the disease originates there, is dry and hot, passing, as it does, over a large tract of Africa and of the land of Spain. At Gibraltar, it is warm, but moist. At Malaga, it is rather refreshing. It is moister, but, passing over a large extent of sea, is cooled. The terral, or land wind, is very hot and oppressive during summer.⁶ To this let me add that, at Vera Cruz, where the disease commences with the rainy season, it prevails more extensively at the beginning of that period, when of necessity the earth is less drenched with water and the atmosphere less humid, and also at the close of it, when, through the process of evaporation and other means, the quantity has lessened, than during the middle and wettest period;⁷ which could not be the case if the humidity were the primary cause. It will be found, also, that in Jamaica, and, indeed, in other tropical regions, the fever prevails more severely and extensively at the decline of the wet season.⁸

It may be here mentioned, that whatever be the true effects of dews and fogs—upon which a great deal has been said—we cannot look upon

¹ Channing, New York Medical Repository, ii. 403.

² Observations on Remittent (so called) and Yellow Fever of the West Indies. By David Lake Finlay, p. 9. Dublin, 1853.

³ Thomas, p. 63.

⁴ Ib., p. 18; Fenner, N. O. Journ., iii. 446.

⁵ Tullock's Reports, &c., p. 4; Fellowes, p. 466.

⁶ Fellowes, p. 15, note; Bally, p. 363. ⁷ Humboldt, p. 765.

⁸ Pinkard, ii. 486; Henderson, p. 8; Report on the Fev. of Cayenne in 1850, i. 157–8.

them as the efficient or necessary agent in the production of the yellow fever. If they were so of themselves, and without the aid of concurrent and more active causes, it would matter not whence they come. The effect would be the same whether they arose in elevated and mountainous regions or in valleys and plains, or in city localities. Yellow fever might be expected to occur in all foggy and moist countries, as also in every place where the dews are heavy, provided the temperature were high. On the other hand, localities where fogs and dews are not observed would not be the seat of the disease. Now, we know that this is not the case. These atmospheric conditions are not the attributes of yellow fever localities only. Dew, for example, will be abundant wherever vegetation is rich, and may, like fogs, be seen on hill-tops, where yellow fever, or other forms of kindred febrile complaints, have never been heard of, as on low ground, and seaport and other towns, where the causes of those diseases are copiously evolved. This is no fancy on my part; for, if the reader inquires into the matter, he will easily find numerous localities which, like those referred to by Dr. Hunter, are subject to fogs and heavy dews, associated with considerable heat, and yet notwithstanding are, and have always been, exempt from the disease, while in numerous instances the fever has broken out under meteorological conditions which forbid the possibility of its being referred to such agencies. The banks of some streams in this country, and not a few at a short distance from our city, though the seat of heavy fogs and dews, are seldom visited by autumnal fevers—not as frequently so, certainly, as other kindred localities differently circumstanced; and it frequently happens that the usual abodes of yellow fever are covered with fogs and heavy dews, in hot weather too, during seasons of unparalleled health, and in *other* seasons are visited by the disease, although the atmosphere is clear, and the dews are of trifling amount. In reference to ordinary autumnal fevers, I need only mention the upper part of the Wissahickon, and of Chester Creek, where, notwithstanding the fogs and dew observed in certain seasons, these fevers are not known. The city of Charleston furnishes us with another interesting illustration of the want of connection between the degree of evaporation and condensation and the production of malarial fevers. During July, August, and September, of the fever year of 1849, the evaporation was much higher than it was in the non-malarial years of a series extending from 1845 to 1852 inclusive, amounting to 1,485. But in 1852, which was highly malarial, the evaporation did not exceed 1,454, while in 1850 it was 1,418; a difference too trifling to justify us in investing it with importance. As to the degree of condensation, it amounted, in 1849, to 173; contrasting with what took place during the two non-malarial years of 1850 and 1851, when it was 111 and 81. On the other hand, the sickly year of 1852 gives us an amount of 83; only two degrees higher than 1850, and 28 lower than 1851.¹

While the fever thus spreads, generally or occasionally, in certain localities during wet weather, it will be found frequently to prevail during opposite conditions of the atmosphere. Rufz says that there was greater dryness in

¹ Hume, Charleston Journal, viii. 67.

Martinique in 1838 than in ordinary seasons when the fever does not prevail (pp. 29, 30). We have found that the fever that decimated the companions of Capt. Beaver at Bulam, in 1792, occurred in the wet season. "Yet when the fever which was so named from that island appeared in various parts of the West Indies, it was not during the wet season, but, in many places, after long-continued *dry*, windless weather."¹ The same observation has been made at other times and other places.² Bally remarks, that there is little danger from yellow fever for strangers during the rainy season—called *hivernage*—from November to April, the disease appearing in a dryer portion of the year (p. 304). In speaking of the Mole (St. Domingo), Dr. R. Jackson, while regarding the wet as the sickly season, says: "Very little rain falls at the Mole in ordinary seasons, no natural swamp is near it, and little more than the very site of the town is level ground. The character of the endemic, however, is more varied than in most parts of the island." "Intermitting or remitting fevers prevail in the autumnal months and beginning of winter, continued fevers, sometimes with strong, sometimes with obscure marks of vascular excitement, are frequent, especially during dry and hot weather, and among subjects newly arrived from Europe."³ In another work, after remarking that the diseases which occur during the wet season, though numerous, are rarely violent, concentrated, or malignant, the same distinguished writer says that in dry weather, and on a dry, rocky, barren soil, the form under which the endemic appears is usually continued. If the dry weather be of long continuance, the character which it assumes is frequently malignant and fatal, even among such as have been long "resident in the country, and are considered to be assimilated to the climate; among European strangers the concentrated form, which commonly goes by the name of yellow fever, is *then* epidemic, and often fatal as a plague."⁴

The true yellow fever of New Orleans, and Natchez,⁵ has appeared during the prevalence of dry weather, and when the disease could not be ascribed to the agency of atmospheric or terrestrial humidity. It may be remarked, that fevers of kindred, though not identical character, arise under the influence of a similar agency. Dr. McWilliams informs us, in reference to the fever of the river Niger, that the dry period immediately after the rain is the most deleterious.⁶ Dr. Pritchett, in his account of the same fever, states that old residents fare worse during the prevalence of the rains; but that individuals recently arrived from Europe are principally affected in the dry, hot season, without suffering much from disease during the wet season. "The native Africans themselves fall sick in the rainy season, like the old resident Europeans, and, in the dry season, feel themselves healthy. Accordingly, while the whites were nearly all affected with fever at the confluence of the Tchadda

¹ Edinb. J., lxiii. 448; Chisholm, i. 145.

² Hillary, pp. 146-7; Imray, liii. 93.

³ Outline of History and Cure of Fever, p. 92.

⁴ A Sketch of the History and Cure of Febrile Diseases, i. 13.

⁵ Barton, p. 13; Rept. on Fever of 1839, Rev. Méd., p. 322; Merrill, ix. 233, 240.

⁶ Med. Hist. of the Expedition to the Niger, p. 184.

and Niger, the natives were perfectly well. But previously, when the vessel was wooding and watering during the rainy season (July, 1841), at Grand Bassa and Cape Palmas, many of the Africans were affected, whilst the Europeans, generally, enjoyed good health." Dr. Pritchett further remarks, as an interesting fact, that when the rainy season has been imperfectly formed—when there has been an unusual continuance of drought, instead of reiterated deluges of rain, fever has been noted as extremely prevalent and dreadfully destructive.¹ We find, also, that the fever of other parts of Africa,² and of Sicily,³ as also that of the Ionian Islands,⁴ prevails during the continuance of dry and hot weather.

Instances are not wanting, also, to show that the progress of the disease has been checked or mitigated by rain. In the language of Dr. Rush, "it is destroyed, like its fraternal diseases, the common bilious and intermittent fevers, by means of *long-continued* and heavy rains."⁵ Such was the case in the famous epidemic of Grenada in 1793, when the disease ceased about the middle of September. The rain during that month and the following, to December, fell in great abundance, while the heat was greater than it had been before or was subsequently (average of September, 86; October, 85; November, 83; December, 84).⁶ Furlong states that the fever he describes was checked by rain; and we have other authorities for the same fact.⁷ Even in Martinique, a heavy fall of rain, which, of course, must add to the existing humidity, mitigates the prevalence of the fever. Gillespie, after speaking of the ravages occasioned by the epidemic during June and July, 1796, says: "The rain which fell in the second week of August occasioned a diminution of the number of persons attacked by ardent fever, as well as of the malignancy of the disease" (p. 137). The rains, the action of which does not appear to be the same in all countries, were, there cannot be a possibility of doubt, the cause of the cessation of the yellow fever of Cayenne in 1850. So say the authors of the Report on that epidemic, who add: "It is a fact well established, and to which we again recur purposely, that rain has always preceded the few moments of repose allowed us by the epidemic, the cessation of which was preceded and followed by heavy falls of water."⁸

It will be found, also, that in some places subject to the two seasons—the dry and the rainy—the latter is the more healthy; thus reversing what we have seen to occur often, if not generally, in tropical regions. Such is the case in Cayenne,⁹ at Surinam,¹⁰ Berbice, Demerara, and the Dutch and French colonies on the coast of Guiana generally, as well as in the adjoining settlements

¹ Some Account of the African Remittent Fever, pp. 108, 144.

² See Edinb. J., lxi. 132; Rept. on Sickness of British Army, p. 26.

³ Boyle, Edinb. J., viii. 178.

⁴ Report of Sickness, p. 64.

⁵ Yellow Fever not Contagious, iv. 154.

⁶ Chisholm, i. 294.

⁷ Johnson (R.), xxv. 290; Caillot, p. 121; Valentin, p. 89; Lempriere, i. 26; Rochoux, p. 11; Arnold, p. 31; Desportes, i. 52, 80, 87; Ferguson, Chapman's Journ., vii. 8, 9, and Med.-Chir. Trans., viii. 180-1; Johnson, Fever of Mariegalante, p. 362; Hector McLean, xxv. 137.

⁸ Fever of Cayenne in 1850, p. 158.

⁹ Dazille, p. 10.

¹⁰ Firmin, Maladies de Surinam, pp. 3, 18; Chisholm, ii. 196; H. McLean, p. 25.

on the continent.¹ At Cadiz, during the memorable epidemic of 1800, it was found that heavy rains were beneficial; but that when the rain fell in small quantity, and the heat remained unchanged, the disease extended, and became more violent.² On the eastern coast of Africa, the dry is the sickly and the wet the healthy season.³ So also at Delagoa Bay and the island of St. Mary or Nossi Ibrahim, and sometimes, though not always, at Quitimane and Sofala;⁴ and it has similarly been found, in the West Indies, that while in some stations a wet season is looked on as the most unhealthy, in others the reverse is found to be the case.⁵ The same results are obtained in Algeria, where febrile diseases increase and are especially malignant at the period of disappearance of vegetation, when plants have been dried up by the effect of the powerful sun of July, August, and September.⁶

If humidity, when combined with heat, were the primary cause of the yellow fever, it would be difficult to account for the fact that, in most countries subject to the disease, seasons occur in which it does not prevail, though such seasons are not marked by a diminution in the ordinary degree of moisture and rain; and that during sickly seasons the degree of either is not increased. In tropical regions, where the humidity and rain varies but little each year, there are intervals of exemption, of greater or less extent, which cannot be accounted for on the principle in question. In Martinique, the disease did not prevail from 1826 to 1838. In Dominica, it had not prevailed for seventeen years when it broke out in 1838. The years 1839 and 1840 were healthy, though as humid as the preceding; and in 1841 it again appeared, at a time when the humidity and heat were not greater than during the healthy periods.⁷ Lining mentions a fact perfectly in point, and says that the disease appeared in the autumns of 1732, 1739, 1745, and 1748, though none of these years (excepting 1739, whose summer and autumn were remarkably rainy) were either warmer or more rainy (and some of them were less so) than the summers and autumns were in several other years in which the fever did not prevail.⁸ It is also remarked that at Barcelona, in 1821, the humidity was not greater than usual.⁹

On the coasts of the West Indies and of Africa, as well, indeed, as in this country and Europe, vessels remain healthy so long as they keep at a distance from land.¹⁰ But woe to them if, during the sickly season, they approach the

¹ Bancroft, p. 200.

² Berthe, p. 156.

³ Voyage of Discovery by Capt. T. Boteler, i. 137, 155-6.

⁴ Loc. cit., p. 356.

⁵ Report, p. 101.

⁶ Pallas, Réfl. sur l'Intermittence, p. 109.

⁷ Imray, lxi. 331-2.

⁸ Edinb. Essays and Obs., Phys. and Lit., ii. 407.

⁹ Pariset, p. 480.

¹⁰ Lind on Hot Climates, pp. 138, 178-9; Ib. on Seamen, p. 78; Trotter, i. 456; Rouppe, p. 65—English translation, p. 69; Rush, iii. 83; Bancroft, pp. 125, 171; Ib., Sequel, p. 166; Clark on Long Voyages, i. 124; Moseley, p. 57; H. McLean, p. 26; Ferguson's Recollections, p. 151; Gillespie, p. 20; Fontana, p. 12; Bally, p. 455; Pringle, pp. 57, 98; Chervin's Letter to Dr. Monfalcon, p. 12; Burnett, pp. 264-274; Caldwell's Prize Diss., p. 139; Ib., Essay on Mal., in Boston Journ., p. 510; Williams on Morbid Poisons, ii. 446; Smith, Edinburgh Journ., xxxv. 49; Amiel, Edinb. Journ., xxxv. 264; Johnson, Charleston Journ., iv. 160; Med. Obs. and Inq., iv. 156; Blane, pp. 204, 252; Datrouleau, Et. sur les Mal. Maritimes, Gaz. Méd. de Paris, 1850, p. 792.

shore or enter the river streams. The moment they do that they become liable to the disease. Lind, in his essay on the means of preserving the health of seamen, states that when Commodore Long's squadron, in the months of July and August, 1744, lay off the mouth of the Tiber, it was observed that one or two of the ships which lay nearest the shore began to be affected by the pernicious vapour from the land; whilst some others, lying further out at sea, at but a very small distance from the former, had not a man sick at the same time (p. 67). While in the autumn of 1852 many British steamers and vessels of war had the yellow fever for going into port at St. Thomas, "another of her majesty's ships, the *Devastation*, was at St. Thomas, but did not come into the harbour, keeping, however, only about a mile off the town, and remained intact."¹ "I have known," Sir G. Blane remarks, "a hundred yards in a road make a difference in the health of a ship at anchor, by her being under the lee of marshes in one situation and not in the other."²

It is stated by masters of ships that, during the prevalence of the late yellow fever epidemic in Brazil, though they came direct from Europe, and held communication with no vessel of any kind on their passage, the disease made its appearance on board their ships as soon as they approached the coast and came within the influence of the land breeze. Dr. Pennell, who, among others, makes this statement, remarks that most of the cases were slight, but some were attended by black vomit, and proved fatal after their arrival in port.³ Dr. Gavin states that when the yellow fever broke out in Georgetown, Demerara, at the end of 1851, some seamen arriving from Europe were attacked with the disease on nearing the coast and getting into the muddy water, some days before their arrival in harbour.⁴

Lind states, in his well-known work on hot climates, that "many persons escaped the yellow fever which prevailed in Pensacola in 1765, by retiring to the ships which lay in the harbour" (p. 179). In another place the author remarks: "When the violent and fatal sickness raged at Cadiz in 1764, it did not extend its influence to any ship which lay at a distance from the city." His majesty's ship the *Tweed*, which was then at anchor in Cadiz Bay, like others, escaped. All the sick that were sent on board recovered, no bad symptoms appearing in their fever, "while a disease similar to the black vomit and the yellow fever, and equally mortal, depopulated that large city" (*Ib.*, p. 178).

Dr. Rush says of the epidemic of 1793: "I heard of some seafaring people who lived on board their vessels, who escaped the disease."⁵ Dr. Caldwell remarks that "marsh malaria cannot reach the crew of a ship lying at anchor but a cable's length from the shore, where it is generated;" and adds: "Similar facts may be collected from the history of yellow fever in our own country. During the prevalence of that disease in Philadelphia, many individuals, and

¹ Wiblen and Harvey on Yellow Fever, *Lancet*, April, 1853, Am. ed., p. 322.

² *Diseases of Seamen*, p. 228.

³ A Short Report upon Yellow Fever, as it appeared in Brazil during the Summer of 1849-1850, p. 8.

⁴ Second Report on Quarantine (London, 1852), pp. 14, 15.

⁵ *Op. cit.*

several whole families, are known to have retreated to vessels lying not more than from two hundred to two hundred and fifty yards from the wharves, and to have remained healthy. In New York and Baltimore like instances have occurred."¹

Who is unacquainted with the fact, noticed in diverse latitudes, that while, in vessels at a short distance from infected localities, those who remain on board are exempted from fever, the boat crews, who, from the nature of their duties, are obliged to explore the river banks, and those who land on business or for recreation, are, especially if they sleep on shore, sooner or later attacked?²

On the other hand, vessels that are infected in such localities lose the fever (if they themselves do not contain sources of infection) by shifting their position, and anchoring at a distance—sometimes at a very short one—from the shore; or by going to sea, and thereby placing themselves beyond the influence of the land air. The morbid agency (in Georgetown, Demerara) seemed to move in shifting swarms or vortices, hovering over a vessel here and there. “Thus, in the beginning of 1839, the *Thomas King* lay in the division A, between Kingston and Wighart’s Stellings. In one week she lost four hands. She unmoored, and took her station *outside*, or to the leeward of the *Louisa Baillie*; the mortality ceased, and the health of the crew became re-established. The *Louisa Baillie*, that had been right abreast, and sheltered by the *Thomas King* before the unmooring, and had no death, although several cases of fever, which readily yielded to treatment, became soon very sickly; she lost four men, after which she shifted her moorings, and the mortality then ceased in her also.³ These effects have been observed on frequent occasions in the West Indies, and on the coast of Africa.⁴

I am informed by Mr. Martin, surgeon of the *Cananeuch*, a Guinea trader (Dr. Lind states) that when he was in Gambia River, in company with four other ships, the men in one of those ships were daily taken ill of fevers and fluxes, and several of them delirious; while all the English in the other ships and in the factories were in perfect health; but upon removing that ship about half a league from her first anchorage, which was too near some swamps, her men became as healthy as those in the other ships.⁵ Sir G. Blane remarks: “When ships watered at Rockfort (Jamaica), they found

¹ Prize Dissertation, Boston Journ., iii. 150.

² Badenock, *Med. Obs. and Inq.*, iv. 157–8; Trotter, *Med. Naut.*, ii. 86; Bally, *Typhus d’Am.*, p. 455; Clark on Long Voy., i. 38–40; Boyle, p. 75; Rouppe, pp. 65, 75; Gillespie, p. 20; J. Hunter, p. 17; Valentin, p. 77; Caillot, p. 200; Bancroft, p. 172; *Ib.*, Sequel, p. 166; J. Wilson, p. 66; Fontana, p. 12; Blane on Seamen, pp. 92, 392; Lind, pp. 106, 108, 134, 162, 195; Lind on Seamen, pp. 73, 77–78; J. Johnson, pp. 63–65, 127, 134; J. Wilson, *Stat. of Brit. W. I. Squadron*, p. 85; Bryson, p. 151, &c.; Burnett, pp. 188–225, 268; Smith, *Edinburgh Journ.*, xxxv. 13, 47, 50; Allon, *Edinburgh Monthly Journ.*, Aug. 1847; Bryson, *Stat. Rep. on the Health of the (Brit.) Navy*, pp. 215, 220, 230.

³ Blair, p. 36.

⁴ Lind, p. 200; *Ib.*, on Seamen, p. 85; Ferguson’s *Recol.*, p. 143; Trotter, i. 358; Hunter, p. 16; Kéraudren, p. 18; H. McLean, p. 26; Rufz, *Report*, by Chervin, p. 60.

⁵ Lind on Hot Climates, p. 180.

that if they anchored close to the shore, so as to smell the land air, the health of the men was affected; but upon removing two cables' length no inconvenience was perceived."¹

These facts cannot be accounted for on the supposition of the fever being due only to excess of humidity, for it results from the observations collected by Kaemtz,² and other meteorologists, that at sea, in all latitudes, the air is always in a state of aqueous saturation. The sea climate may, indeed, be regarded as the type of a humid climate. On shore, even in a damp atmosphere, a period arrives when the air, being heated, ceases to be in a state of saturation, not only because its capacity for aqueous vapour becoming excessive, evaporation is no longer sufficient to furnish materials to insure that saturation, but because the ascending aerial current carries the humidity into the upper strata of the atmosphere. At sea nothing of the kind takes place. The air is as fully saturated during the heat of the day as during the coolest portions of it, and as a consequence a precipitation of vapour takes place whenever the temperature lowers, either at the setting of the sun or on the occurrence of a refreshing wind. From this it follows, that more than any other the sea climate is subject to those nightly and evening condensations, and those accidental precipitations upon which so much stress has been laid. Add to this, that the humidity penetrates to every corner of the vessel, and that in the hold, between decks—and, indeed, everywhere—those on board are exposed to a larger sum of moisture than they would probably experience on land. If then the yellow fever were due to the hygrometrical condition of the atmosphere alluded to, the disease would be more likely to break out at sea than elsewhere.³ It would not spare those who remain on board, and attack those who go ashore, and the surest way to arrest the progress of the fever in a sickly ship would not be to send it to sea.

CHAPTER VIII.

CIRCUMFUSA—HUMIDITY, CONTINUED.

AFTER what has been stated in the preceding chapter as regards the prevalence of yellow fever in the dry seasons of some places, and during dry weather of other localities where it sometimes or generally appears during the rainy season, we shall find little difficulty in understanding that the disease assumes often its worst character at periods of great dryness. Gilbert, in his medical history of the army of St. Domingo, while attributing much of the effect to a hot damp atmosphere on the unacclimatized, remarks: "It

¹ Diseases of Seamen, p. 178.

² Cours Complet de Météorologie, Tr. par Martins, pp. 91-2.

³ Jacquot, Ann. d'Hygiène, July, 1854, p. 41.

is thus that a particular cause has imparted greater intensity to the disease of St. Domingo, and rendered it epidemic, inasmuch as it attacked a large number of the natives. It is the extraordinary dry temperature which has prevailed during the last six months in all parts of the known world." "In the year VI. (1798)," he adds, "under the action of a like temperature, the English occupied Mole St. Nicholas, and the vicinity. Their number amounted to 25,000, and, in the course of the summer, they lost seven-eighths of them by the yellow fever" (pp. 4, 69, 70). Pouppé Desportes, who practised during fourteen years in the city of the Cape, and to whose high authority M. Gilbert appeals (pp. 65-6), testifies to the fact that the disease was always the more violent in proportion to the dryness of the season.¹ Such was the case in 1733, 1734, 1739, 1740, 1741, and 1743; whereas, in 1735, 1736, 1737, 1738, and 1742, when the weather was of a different kind, the fever only appeared in a sporadic form (p. 191). Dr. Ferguson, basing his conclusions on facts of the kind, and on the result of his personal observations, is of opinion, that one only condition is indispensable to the production of the yellow fever, and other kindred diseases—paucity of water where it previously and recently existed, and a surface capable of absorption, provided that paucity be short of actual dryness. To this, he says, there is no exception in climates of high temperature, and he argues from this that we may infer, that the cause of the disease is produced in a highly advanced stage of the drying process—an effect incompatible with great atmospheric humidity.² In another and more recent publication, Dr. Ferguson says: "Drought under an equatorial temperature, and in the favouring localities, would seem to be the *sine qua non* of the appearance of the yellow fever in Europe. In the terrible epidemic of Cadiz, in the year 1800, no rain had fallen there for seventy days (vide *Annual Register*), and vegetable putrefaction had become just as impossible as the putrefaction of an Egyptian mummy, or the dried fish of Holland. When the same drought pervaded Gibraltar and Barcelona, there needed not the arrival of smugglers to bring the pestilence."³

Dr. Ferguson's observations in the West Indies led him to the same conclusions. "It might there be seen, that the same deep, marshy country which the rains made perfectly healthy, as if by deluging a dry well, was speedily converted, under the drying process of a vertical sun, into a hotbed of pestiferous miasmata. Thus, in the island of St. Lucia, the most unwholesome town of Castries, at the bottom of the Carenage, which is altogether enbosomed in a deep mangrove fern, became perfectly healthy under the periodic rains; while the garrison on the hill of Morne Fortuné, immediately above it, within half cannon-shot, began to be affected with remittent fever. The two localities within this short distance evidently changed places in respect to health. The top and shoulders of the hill had been cleared of wood, and during a continuance of dry weather, the garrison had no source

¹ *Maladies de St. Domingue*, i. 86-7, 110, 116, 121, 123, 129.

² *Med.-Chir. Tr.*, viii. 130; *Philad. Med. and Phys. J.*, vii. 8, 9, 16.

³ *Recollections*, p. 159.

of disease within itself; but this was amply, though but temporarily supplied, as soon as the rains had saturated the soil on which it stood. Thus, an uncommonly rainy season at Barbadoes seldom failed, in that perfectly dry and well-cleared country, to induce for a time general sickness; while at Trinidad—which is almost all swampy, and the centre of the island may be called a sea of swamp, where it always rains at least nine months of the year—if it only rained eight, or if at any time there was a cessation of the preserving rains, the worst kind of remittent fevers were sure to make their appearance. General dryness of soil, however, is far from being the ordinary characteristic of our West India colonies. The swamp is too often exposed to the continued operation of a tropical sun, and its approach to dryness is the harbinger of disease and death to the inhabitants of its vicinity. On the whole, it may truly be said, that although excessive rains will evidently cause the acknowledged wholesome and unwholesome soils to change places for a time, in respect to health, a year of stunted vegetation, through dry seasons, and uncommon drought, is infallibly a year of pestilence to the greater part of the West India Colonies.¹ In all cases, however, previous saturation of the soil is necessary to insure the effect, and fever ceases when the exsiccation is thorough and complete.

On all these points, other facts, numerous and authentic, may be collected in other writers on the fevers of Europe, of the West Indies, of Mexico, South America, Africa, and this country.² Indeed, we can scarcely open a book, large or small, which treats of the subject *in extenso* or incidentally, without finding therein a repetition of the same story—absence of fever during excess of terrestrial humidity; occurrence of the disease under the influence of the drying process; on the other hand, absence of fever when the exsiccation of the soil is complete, and penetrates to a considerable depth, and every particle of moisture has been destroyed; and afterward reappearance of disease during the wetting period.

This connection of dryness with unhealthiness had not escaped the notice

¹ Marsh Poison; see Notes and Recol. of a Professional Life, p. 191.

² Rochoux, pp. 11, 42; Berthe, pp. 51, 156; Pariset, p. 177; Fellowes, pp. 13, 32, 35; Report on Fever of Gibraltar, p. 4; Caillot, p. 121; Furlong, Med.-Chir. Rev., xxv. 290; Valentin, pp. 87, 89; Lempriere, i. 26, 31; ii. 47, 48, 49; Desportes, i. 52, 80, 86, 87; Ferguson, Med.-Chir. Tr., viii. 130-1; H. McLean, pp. 25, 72; Gillespie, pp. 20, 137; Johnson, Trop. Cl., p. 362; Dazille, Mal. des Nègres, p. 10; Firmin, Mal. de Surinam, pp. 3, 18; Bancroft, pp. 200, 314; Edinb. J., lxiii. 448; lb., lxix. 132; Bally, pp. 304, 363; Hillary, pp. 146-7; Jackson, Outlines, p. 92; Tullock's Rep., p. 64; Gilbert, pp. 4, 69, 70; Rufz, pp. 10, 29; Catel, p. 9; Beguerie, p. 10; J. Clark, p. 75; Towne, p. 8; Leblond, p. 229; Imray, Ed. J., liii. 93; lb., lxiv. 331; Pinkard, ii. 486; Henderson, p. 8; Arnold, pp. 31, 148, 174; Chisholm, i. 145, 294; ii. 196; Humboldt, p. 765; Boteler, Voyage of Discovery, i. 137, 155, 156, 356; Brit. and For. Med.-Chir. Rev., i. 382; Simon's Rept. to Bd. of H. of Charleston, pp. 6, 10, 18; Watts, Med. Reg. of N. Y., p. 278; Barton, p. 13; Revue Méd. (1840), p. 322; Merrill, Med. and Phys. J., ix. 233, 240; lb., N. O. J., viii. 7; lb., N. A. J., ii. 218; Rush, iv. 154; Chabert, p. 20; Lining, ii. 407; Townsend, p. 263; Barton's J., ii. 22; Drake, i. 717; Gros, p. 5; Johnson, Charleston J., iv. 155; Hulse, Maryland J., Jan. 1841, p. 392; Thomas, pp. 18, 63, 213; Craigie, Pract. of Med., i. 82; Goupilleau, Bulletin de l'Acad., i. 456; iii. 306.

of Lord Bacon, who remarks: "The general opinion is, that years hot and moist are most pestilent, upon the superficial ground that heat and moisture cause putrefaction. In England, it is found not true; for many times there have been good plagues in dry seasons. Whereas the cause may be, for that drought, in the bodies of islanders habituated to moist airs, doth exasperate the humours, and maketh them more apt to putrefy or inflame; besides, it tainteth the waters commonly, and maketh them less wholesome. And again, in Barbary, the plagues break up in the summer when the weather is hot and dry."¹

These remarks hold good, not only in reference to the plague, but to other fevers of kindred origin, as observed in this country, various parts of Europe, in Senegal, India, Ceylon, Batavia, Algiers, the coast of Africa, &c.

We have here, therefore, the reverse of what was said above respecting the injurious effects of the *wetting* process; for while the latter, by its operations on localities more or less dry and parched, gives rise to fever, the same disease is often an attendant on, and a result of, the *drying* process—not making its appearance until the surface, after having been covered or saturated with water, is becoming, through the evaporating agency of heat, to a considerable extent desiccated; excess of dryness and excess of moisture being alike inimical to the production of the febrile cause. In neither of these two processes can we admit, therefore, a monopoly in regard to the power of giving rise to that dire effect; and the very fact of two influences of such antagonistic characters exercising that power, under opposite local circumstances, must naturally lead to the inference that the result in question is obtained, not in virtue of any morbid influence dependent on a wetting or a drying operation, abstractly considered, but on the change they each occasion in the soil of the locality thereby rendered sickly, or in the organic materials placed on its surface—drying them in the one case when too wet, and moistening them in the other when too dry. If we admit this, we must admit also that as the effect is obtained in none but localities of the peculiar kind already mentioned, and as neither the wetting nor the drying process exercises any morbid influence of the kind alluded to, under different conditions of soil or locality, that effect, when produced, must be due, not to a little more or a little less dryness, either of which, as we have seen, may exist in salubrious situations, but to the extrication from that soil, or the materials scattered over it, of some peculiar agent which operates as a poison on individuals exposed to its influence.

But let this be as it may for the present, with the above facts before us, we can better appreciate the pertinency of Major Tullock's remarks, already referred to, that "in some stations a dry, in others a wet season is looked on as the most unhealthy." Long before Major Tullock's days, Dazille had recorded the same observation in a passage strangely mistranslated by Dr. Rush, and handed down, in all its imperfections, by several successive writers. "It is during dry weather that diseases prevail at Cayenne; on the

¹ Bacon, Nat. Hist. Cent., p. 4; Exper., p. 383.

contrary, it is during the rainy season that they spread in St. Domingo. The reason of this difference is that at Cayenne, during the period of rains, the marshes contain a sufficient quantity of water to be preserved from corruption, and to be renewed gradually by the flow and ebb of the sea. When once the rainy season is over, the waters become stagnant and in a state of corruption, and occasion by their putrefaction that of a large quantity of insects and animalcules, the effluvia from which are exhaled in the atmosphere, thence pass, through means of respiration, into the lungs, and carry into the humours the germ of the diseases which afflict the inhabitants of the vicinity.”¹

That a high dew point has a tendency to produce injurious effects on the system, that it is often found to exist in unhealthy localities or during pestilential times, and that it must assist somewhat in the development of autumnal fevers, and, among these, of yellow fever, are facts which no one will question. But that it plays the all-important part in the causation of those diseases attributed to it by Mr. Hopkins and some others, is far from being demonstrated. Were the belief correct, we should expect to find that a high dew point—reaching to at least sixty degrees—is always attended with the occurrence of autumnal fever in one or other of its various forms; that countries or seasons in which the dew point is high are necessarily insalubrious; that the prevalence of fever is proportioned to the elevation of the dew point; and that, on the contrary, localities in which the degree is low are as necessarily healthy.

¹ Dazille, *Maladies des Nègres*, p. 10. The mention of this distinguished physician calls to mind an incorrect reference to his views by some of our writers. Dr. Rush, in his account of the epidemic of 1793, says: “Dr. Dazille, in his treatise upon the diseases of the negroes in the West Indies, informs us that the *rainy* season is the most healthy at Cayenne, owing to the neighbouring morasses being deeply overflowed; whereas, at St. Domingo, a *dry* season is most productive of diseases, owing to its favouring those degrees of moisture which produce morbid exhalations” (iii. 108). The sentence thus worded has been quoted, *verbatim et literatim*, by several writers—by Dr. Chapman (*Philad. Med. and Phys. J.*, viii. 367), and Dr. Smith, in his excellent work on Epidemics, p. 70.

But the reader will easily perceive that in this there is contradiction, for if the views of Dazille are rightly represented by Dr. R., we find no difference between the case of Cayenne and of St. Domingo, while the *whereas* would lead us to infer that the results were not the same in the two places. If, at Cayenne, the rainy is the healthy season, the dry season must be the sickly; and if, at St. Domingo, a dry season is unhealthy, a rainy season must be the reverse. Where is the difference between a rainy season being the most healthy in one place, and the least unhealthy in another? This reminds us of the proposition of the *cute* boy to his playfellow on tossing a penny—“head, I win; tail, you lose.” On turning, however, to Dazille, it will be found that the contradiction or uncertainty is not to be laid to his account, for he says, in very plain language: “C’est pendant la sécheresse que les maladies règnent à Cayenne, c’est au contraire dans le temps des pluies qu’elles exercent leurs ravages à St. Domingue” (p. 10). Here it will be seen that while, according to Dr. R.’s translation, the rainy season is represented as the most healthy in the former place, it is the *wet*, and not the *dry* season which, we are told, is the most unhealthy at St. Domingo. We might regard the error as resulting from a mere typographical blunder, but Dr. R., after putting down the word *dry*, instead of *wet*, goes on to inform us how the dryness produces the baneful effects—an explanation nowhere given by Dazille himself. Be this as it may, it furnishes an example of erroneous quotation, and especially of the danger of quoting at second hand.

Now, let us inquire how far facts will bear us out on these points, so far as regards the disease more particularly under consideration. We have seen that the latter has not unfrequently broken out and spread extensively in places where but little visible humidity existed; where the soil was arid, dry, and cracked, and where everything upon it was parched in consequence of long continued and severe drought; where the absence of rain was not compensated by fogs and dews, and where, as a natural result, the dew point was found, or might be inferred, not to have reached very high. We have seen, on the other hand, that the same or other places have remained exempt from the disease under hygrometrical conditions of an opposite character, combined with a degree of heat well calculated to promote an excess of atmospheric moisture, visible and invisible; and when, from these circumstances, the dew point, if not positively ascertained to have been high, may, with perfect propriety, be supposed, in the absence of direct observation, to have risen considerably. It may be remarked, also, that in places subject to occasional visitations of autumnal fever of various grades of intensity, sickly seasons are not necessarily characterized by a higher dew point than the corresponding periods of other years in which the disease does not make its appearance.

In Demerara, the height of the dew point, in ordinary seasons, differs but slightly from that to which it reached during the prevalence of the fever of 1837-1845. In 1843, the mean degree was 73.8, varying from 72 (February) to 74.9 (July, August, and November). In 1844, the average height was 75.1, with a variation of from 73.3 (December) to 76.0 (May). In 1845, eight months give us a mean of 74.6—from 73.0 (January) to 76.0 (April); while in 1846, the year after the cessation of the disease, the mean dew point was 74.4, with a variation of from 70.8 (February) to 76.2 (May and June).¹

The following table of the dew points during the months of June, July, August, September, October, and November, from 1845 to 1854, inclusive, at Charleston, S. C., will show to those who are aware that the yellow fever prevailed there during only three of those years (1849, 1852, and 1854), that the difference in regard to the saturation of the atmosphere during sickly and healthful seasons is but trifling, and cannot alone account for the production or absence of the disease; and, indeed, that the results obtained are very different from those that might have been expected did a high dew point and fever stand in the relation of cause and effect.²

			June.	July.	Aug.	Sept.	Oct.	Nov.
1845	.	.	69.46	72.77	71.96	66.00	54.61	41.00
1846	.	.	70.83	72.82	75.64	70.66	59.22	50.46
1847	.	.	71.70	73.39	74.23	68.20	58.67	53.53
1848	.	.	70.26	73.00	72.09	66.26	58.74	44.80
1849	.	.	73.46	70.77	72.71	66.00	60.93	53.13
1850	.	.	67.86	75.03	74.93	69.70	55.00	50.86
1851	.	.	70.73	74.38	74.67	65.13	57.41	48.23
1852	.	.	66.63	73.16	70.70	67.66	61.38	49.50
1853	.	.	70.43	75.64	74.45	70.70	55.80	52.76
1854	.	.	72.50	76.70	77.29	73.83	59.48	46.90

¹ Blair, *op. cit.*, p. 120.

² Hume, *Charleston Journal*, v. 10; viii. 67.

Thus the average dew point in June and October of the sickly years of 1849 and 1854 was higher than in the corresponding months of any other year of the series; but when we examine the results obtained in the other months, matters are found to take a different turn; for while in July, 1849, the average was 70.77, in 1852, 73.20, and in 1854, 76.70, we have a higher point than in the former in each of the other years of the series, and a higher one than in 1852 in the same month of 1847, 1850, and 1851. The average in August, 1846, 1847, 1850, and 1851, was higher than in 1849, and higher than in 1852 in every year of the series. September, in 1846, 1847, 1848, and 1850, gave a higher point than in 1849, while the amount in 1852 was exceeded by that in 1846, 1847, and 1850.

In New Orleans, where the average annual dew point, calculated on a basis of eight years, was found to be 63.56 (observed at 12 o'clock)—June being 73.95; July, 75.42; August, 75.59; September, 73.63; October, 62.73; November, 54.27; the year 1849, during which both the yellow fever and cholera prevailed extensively, presents an average, for the twelve months, of 63.71—June, 74.43; July, 75.53; August, 76.75; September, 73.78; October, 61.16; November, 57.71.¹

In 1850, when the fever was only, as it were, sporadic, the average observed at 12 M. was 64.46; June, 72.94; July, 76.80; August, 76.95; September, 74.05; October, 62.95; November, 53.69. In 1853, in the summer and autumn of which the fever prevailed to an unprecedented extent, the average dew point amounted to 62.122—June being 73.20 (range 80.9–66.3); July, 72.13 (80.9–66.5); August, 78.08 (79.4–66.2); September, 70.93 (78.3–50.3); October, 59.31 (74.5–31.9); November, 59.46 (60.5–36.1).

Dr. E. H. Barton, of New Orleans, informs us, as the result of an examination of fifteen epidemics, that the average dew point at the commencement of the sickness was 95.82; the average at the maximum of the epidemic was 74.34, and that at the declination 62.12. The difference between the period of the commencement and that of declination was 13.70, and we have the remarkable fact that a diminution of the dew point brought the epidemic to its maximum of intensity, while a further diminution brought it to its declination. Dr. B. remarks, in reference to this matter, that, during the last nine epidemics, it was found that, although the results stated were but an average of the whole, the extremes or variations from it, in any year, were very small; in other words, the minimum dew point at which the epidemic passed off, and which was required to destroy that character, was noted at 58.26, and the maximum, in any year, under which it ceased its ravages, was 66.64, being a difference of a fraction over 8° only.²

The above statements show that the mean dew point of the summer in Charleston and New Orleans is several degrees above that of the autumnal season, when fevers are more apt to be prevalent. In the latter city, the ex-

¹ Southern Med. Reports, i. 100; ii. 148.

² Introd. to Rep. on Fever of 1853, p. 13.

cess amounted to near nine degrees. In Charleston, the difference between the mean dew point of the three summer months of the seven years specified, and that of the three following months, reached above fourteen degrees. Dr. Gardner, in his essay on the dew point, states the excess, in the United States generally, to be upwards of fifteen degrees. It is evident, also, from facts mentioned already, and from many others that might, if necessary, be added, that in all parts of this and other countries—in localities frequently or occasionally visited by yellow or other forms of malarial fever—a dew point of sixty or more degrees is often experienced months together without ill health, although the temperature be such as to promote the production of disease. On the other hand, as Dr. Gardner properly remarks, the fens of Lincolnshire, Walcheren, the marshes of Holland, are pestiferous with a dew point of less than fifty degrees.¹

These circumstances, viewed in connection with the fact so often referred to, that the yellow fever occurs in ships and circumscribed localities, while other ships and localities in the immediate vicinity, and placed under like meteorological influences, remain exempt; that the alleged effects are not found to be produced by a saturated atmosphere in the higher latitudes; that seamen at sea are exposed with perfect impunity to an atmosphere saturated with moisture, and are attacked only when they land, indicate plainly the impropriety of attaching too much weight to the hypothesis in question, and must debar us from the possibility of connecting, as efficient cause and effect, a high dew point and malarial fevers of various grades, and especially of the disease more specially under consideration. The impropriety of the belief is further proved by circumstances which Dr. Gardner appears to have established, *i. e.* that the detention of the insensible vapour from the lungs or the skin, by a high point, is much less considerable than it is represented by those who have urged the theory; that the dew point in insalubrious places is often much lower than is thought, and that a high point does not arrest the separation of carbonic acid and the other constituents of sweat. Let us examine the effects produced as we may, and concede to a high dew point what extent of agency soever we may think to be warranted by the nature of the facts observed, as well as by the well-known influence of atmospheric moisture, we arrive always at the conclusion that something more is necessary to enable us to account for the development of the yellow fever; that, by itself, a high dew point will not occasion the disease.

The foregoing facts afford, I think, a convincing proof, that the yellow fever is produced under opposite hygrometrical conditions of atmosphere; and that those who ascribe its origin and prevalence to excess of atmospheric and terrestrial humidity in all places, and under all circumstances, err as greatly as those who refer it exclusively to a dry or drougthy state of the air. Nevertheless, while the fever breaks out and spreads in some localities and seasons during the prevalence of rain and of atmospheric humidity, we may infer from what has been stated, that it does so often under the influence of a

¹ American Journal, N. S., ii. 107.

different state of weather. The experience of this city would appear to favour this view of the subject. It will be found that the disease usually breaks out in the latter part of summer—that it prevails until arrested by the accession of frost—and that during the greater part of this period the weather is ordinarily dry. The summer of 1699 was, as we have seen, one of the hottest, or, of course, one of the driest experienced—men died at harvest, and all business in the city was suspended. In 1762, it prevailed after a very hot and dry summer.

On the heat, dryness, and drought, of the summer and autumn of 1793, I have already dwelt, on the authority of Dr. Rush. There was no rain, of any amount, from the 25th of August to the 15th of October, and such was the dryness, that the crops were injured and the springs failed. “The weather,” says Carey (p. 70), “during the whole of the months of August and September, and most part of October, was generally remarkably dry and sultry. Rain appeared as if entirely at an end.”¹ Dr. Rush, in his description of the fever of 1794, tells us that, from the influence of occasional showers of rain in the months of September and October, the disease was frequently checked, so as to disappear altogether for two or three days in his circle of practice. It was observed, that while showers of rain lessened, moist or damp weather, without rain, increased it (iii. 201). In 1797, the months of June and July were dry with little rain falling. In the early part of August—2d to 9th—the weather was wet; but, after that day, it became once more dry.² In the year following, 1798, when the disease made such disastrous progress, the latter part of May, the whole of June, part of July, as well as August and September, were characterized by extreme dryness, in consequence of which whole fields were burnt up by the sun and the crops of hay seriously injured.³ The season in 1799, on the contrary, was marked by a greater degree of humidity than the preceding sickly periods; though not more so than in other years, unvisited by the fever. June was mostly dry; no rain falling from the 12th to the 26th. In July, there was some falling weather (eight days), and several of the ordinary thunderstorms of the season, accompanied with heavy showers. The weather in August was very variable. A considerable quantity of rain fell at different times in the course of the month;⁴ while, in September, rain fell at twelve different times—sometimes during the whole day and heavily (p. 30). Yet, as Dr. Rush states, the weather was dry in these last two months. There was no second crop of grass; the gardens yielded a scanty supply of vegetables, and of an inferior size and quality (iv. 56). In 1801, the disease broke out about the middle of September after a drought of some duration.⁵ Prior to this the weather, during the three summer months, had been pleasant with a sufficient fall of rain, and the disease was mild and manageable.

The season of 1802 was of a rather different character. In the month of

¹ Devèze, p. 20; Currie, *Fever of 1793*.

² Currie, on *Bilious Fever*, p. 214.

³ Rush, iv. 39, 40; Condie and Folwell, p. 13.

⁴ Currie, p. 29.

⁵ Caldwell, 1805, p. 36.

June the weather was cool, rainy, and hot in succession. During the first half of July, rain fell in abundance. June of 1803 was attended with but little rain. In July, the cool refreshing northerly breezes of May and June were exchanged either for oppressive calms, or for the humid and sultry winds of the south. In August, similar weather prevailed; but a greater quantity of rain fell during this than during the preceding month.

In 1805, the summer commenced in June, and set in with great severity. The heat was unusually intense from thence to the end of August. This heat was accompanied by a severe drought, which commenced on the 28th of June and continued without any intermission, except a very few sprinklings of rain that barely moistened the surface of the earth, till the close of August. During this period, not only the rains failed, but even the dews ceased to descend; the earth became parched, and the water in the Schuylkill was lower than it had been for years before.

The month of May, 1820, was wet. The number of rainy days amounted to 18; the rain gauge marking 5.04 inches. June was hot and dry—the rain being only 1.20 inch. July was hotter, but humid, with 4.92 inches of rain. August and September were warm and dry—the dryness amounting almost to a drought—the quantity of rain being in the former 1.98, and in the second 1.56 inch.

In 1853, the quantity of rain which fell in May, June, July, August, and September, amounted to 22 inches, being only five inches more than the year before, when there was no yellow fever, and two inches more than in 1854, when the number of cases was very limited. Five inches, more or less, of rain in the long space of five months, can scarcely be supposed capable of acting as the efficient cause of an epidemic, which commenced early in July, and ran on till October; inasmuch as the quantity of rain which had fallen for four months prior to the time of appearance of the disease was larger in 1850, 1852, and 1854 than in 1853; being in March, April, May, and June, 1852, 28.257; in 1854, 20.560, and in the yellow fever year of 1853, only 15.587. Add to this, that, in many non-sickly years, the quantity far exceeded that of sickly years. In 1841, 1842, 1843, and 1850, it amounted, during the months of May, June, July, August, and September, to 23.70, 25.98, 23.07, and 30.582 inches.

The dew point in Philadelphia in 1844, '45, '46, '47, and '48, varied—

In June, from 37 (1847) to 58 (1845); in July, from 62 (1844 and 1845) to 67.5 (1848); in August, from 61 (1844) to 65 (1846), and in September, from 49.13 (1848) to 59 (1846). In the afore mentioned years, the city remained entirely free from yellow fever, and when the results are compared with those obtained during the same months in sickly seasons, the difference, if any exists, is found to be very trifling. The fever, as is known, prevailed somewhat extensively in 1853; appeared, though only sporadically, in 1854, and was not seen at all in 1852.

The dew point, taken at 2 P. M., was, according to the careful observations of Professor Kirkpatrick, of the Philadelphia High School:—

	1852.	1853.	1854.
May	48.5	49.3	49.1
June	50.9	54.2	54.8
July	55.9	57.2	59.2
August	55.3	59.3	56.7
September	47.5	54.2	56.5

From this it would appear that, during the sickly season of 1853, the dew point was higher in each of the afore-stated months than in 1852; but it was lower in June, July, and September than in the same months of 1854, when there were but a few cases—nearly equal in May and only higher in August. It was lower in May, 1853, than in May, 1844, '46, '48; lower in June of the former year than in 1844, '45, '46, '47; lower in July and August than in the corresponding months of all those years, and lower in September, 1853, than in September, 1844, 1845, 1846, and 1847.

The fever of this city is thus shown to be connected, in most instances, at least, with a deficiency of atmospheric and terrestrial humidity—though, in all instances of its occurrence, it will be found that prior to the accession of dry weather, the earth had been more or less saturated by the fall of rain. But, whatever may be the degree of such a saturation, it soon subsides under the desiccating agency of a heat truly tropical; and not a long time elapses before the humidity resulting from the evaporation of the soil, is so limited as to preclude the possibility of referring the origin of the fever that supervenes to its sole influence. Doubtless there may, and often does exist much atmospheric humidity during the prolonged absence of rain, and the deficiency of the latter is no proof of the absence of the former. It need scarcely be remarked that the coexistence of a clear atmosphere with a high dew point, so far from being impossible, is often seen. In such cases, the air is saturated with vapour and no condensation takes place, provided the temperature of the air and the dew point be the same. In tropical regions, and some portions of Europe and this country, this condition of atmosphere is often found to exist; and even in this city it is far from uncommon, resulting from evaporation, and the influence of damp winds and dews. Nevertheless, this humidity, striking as it may be, must necessarily be less abundant than during the prevalence of rain associated with a high thermometrical range; and if it were the primary cause of the yellow fever in countries where it exists generally, or during a wet season everywhere, the effect ought to be much more marked and frequent in rainy weather than in seasons characterized by a prolonged drought, when the soil is dried to a great depth, when everything on the surface is parched, and when the small streams have disappeared, and the large ones are greatly reduced in size—and when the dryness which thereby ensues is not compensated by an increased amount of dews. And yet we find that the reverse is often the case; while, on the other hand, seasons of great humidity present themselves both here and elsewhere, when, though the heat is, at the same time, of the degree of intensity usually associated with the production and prevalence of the fever, the disease does not appear. Add to this, that the same morbid influence of the atmosphere—humidity, whether exhibited by rain, fogs, mucky weather, or simply a high dew point—

to which yellow fever is by some ascribed, exercises its baneful effects in the production of other diseases only remotely, if at all, connected with that fever, and of which the latter does not constitute an aggravated form—is it reasonable to suppose that the same cause can produce such diversified effects?

We conclude, from what precedes, that humidity, even when combined with high temperature, cannot be viewed as the efficient cause of yellow fever. But, though such is the case—though, when the disease occurs, it must be ascribed to other agencies—there can be no reason to doubt that humidity—both that of the atmosphere, in whatever shape it may exist, and that of the soil—exercises an influence in its production. That influence, as Humboldt (p. 785), has remarked, may be twofold; direct, by predisposing those submitted to it, to the action of the true efficient cause; and indirect, by promoting the formation of the latter.¹ Without prejudging in this place the question as to the agency of infectious miasms in the production of the yellow fever, I may remark that if the causation of this disease, like that of many other fevers, is truly ascribable to such miasms, humidity and rain must be viewed as instrumental in eliciting the evil; for there are few facts more satisfactorily settled than that a certain degree of dampness of the soil is necessary—and that atmospheric humidity is generally favourable to the evolution of the poison. Without it the decomposition of such matters as give rise to the latter cannot be effected—everything is dried and parched.

We have seen that in situations where, and in seasons when, no such humidity exists, and the soil is perfectly dry, the fever is not developed, and that its course is arrested, when, by a prolonged and severe drought, aided by high solar heat, the soil becomes parched up, and every trace of humidity disappears; and it is probable that the effect in this case, as well as in other fevers, is due to the total deficiency of terrestrial humidity preventing the evolution of the miasm. As Dr. Drake remarks: “A hot climate, with a high dew point, is eminently fitted to exert an influence in the production of the gaseous products of organic decomposition; and may, in that way, prove an indirect cause of fevers. In such a climate, everything, both animal and vegetable, which is dead, putrefies rapidly; while in colder or drier climates, from lack of heat or of moisture, such decomposition goes on much more slowly.”²

While the necessity of a certain amount of humidity is thus rendered probable, and its effects are explained, we can easily understand on the same principles why, under opposite circumstances, the accession of rain will, when the drought has been great, prove instrumental in occasioning the disease—furnishing, as it does, aliment to the decomposing process, or, as Pallas thinks, eliciting electricity; while, on the contrary, an excess of it will, by deluging, or covering over all sources of such exhalations, prevent the development or arrest the course of the disease.

¹ Desportes, i. 17; Dazille, p. 11; Savarésy, p. 237; Pugnet, pp. 332, 342; Waring, p. 21; Maher, p. 831; Drake, i. 610; Bertullus, p. 18.

² Principal Diseases of the Interior Valley of N. A., i. 610.

To Mr. Blodget, to whose report on the condition of the summer of 1853, reference has already been made, we are indebted for the following statements relative to the hygrometrical state of the atmosphere.

The heats of June, in the summer of 1853, were remarkably dry. The fraction of saturation was at a mean of about 50 in the northeast, and but 40 to 45 in the interior; and in Texas, during the hot days, from the 14th to the 17th, though much higher at Pensacola and the extreme south, where the heat was not so great from the 20th to the 23d; the rate was about the same in the districts of excessive heat. On the 29th and 30th, the percentage was but 35 to 40 in the narrow district through Tennessee, Kentucky, and Virginia, which marked 100 as the maximum of temperature.

The first two days of July were a continuation of the condition of the last of June. The remainder of the month was not unusual in its hygrometric character generally, though at New Orleans the evidences of high saturation are given in the profuse and constant rains of the middle of the day, preceded by a hot and oppressive morning. The great heat of August was most remarkable in its hygrometric condition, also, and universally attended with a high fraction of saturation; at Washington, it was 50 to 60; and at New York, where the sickness and mortality, whatever they may have been, did not arise from malarial fevers, and where no yellow fever occurred, the fraction of saturation was near 70 per cent. at 2 P. M., and almost at saturation morning and evening.

Rains in August were excessive.

The temperature of evaporation at New York in August was from 80 to 84, being higher than the maximum temperature of evaporation at New Orleans at any time in 1852, by two degrees; at the latter place, it reached 82 but once in that year; with the exception of New Orleans and New York, at this limited period, the heats of the summer, though extreme, have been attended with a low humidity.

In June, the amount of rain was much less than usual generally; in July, it was particularly large at Philadelphia, and southward to Florida, where it was 11.5 inches. In Alabama, and at New Orleans, the amount was nearly as great, and in Iowa and Wisconsin, it was again large—from 6 to 8 inches. In some places, there were severe droughts, as in Eastern Ohio, Western Pennsylvania, and New York. In August, the rains were excessive from the lower part of New Hampshire to Northern New Jersey; at Bloomfield, New Jersey, and New York, the amount which fell was 12 inches. From Baltimore to Savannah, also, the amount was large, being from 5 to 6.5 inches, and about $1\frac{1}{2}$ inch more than usual, the amount being about 3.5 inches. The last days of July, and the first days of August, gave an excessive precipitation in Eastern Pennsylvania and New York, New Jersey, etc. These flooding rains, which gave in some instances 4 to 8 inches in depth of water in a single storm of a few hours, were attended by very warm weather, and immediately preceded the heats of the 12th to the 14th of August. These were also followed by profuse rains, and the whole period, from the 25th of July to the

15th of August, seemed a substitution of a tropical climate for the usually elastic one in the space of country referred to.¹

Now, it must be remarked that the hygrometrical conditions here referred to, extended over a large surface of country, and that while some localities suffered severely from malarial fevers, of various grades and types, others, similarly circumstanced in point of humidity, evaporation, and rain, escaped unscathed. To this agent, therefore, we cannot look for the efficient cause of the disease, even when it is combined with high atmospheric heat.

In regard to its agency as a predisposing cause, it would be useless to enlarge in this place. The effects of humidity on the system are so well known that we might admit the reality of that agency, even did not experience furnish facts in support of it. Very properly, indeed, has it been remarked, that the dew point exercises as great an influence on the living being, as temperature itself; and we can well understand the propriety of a statement long since made, that the *vis medicatrix* is less efficacious in the West Indies and other damp regions (*Fontana*, p. 9). Excessive humidity, especially when combined with heat, diminishes the tone and power of the tissues, penetrates them, and diminishes the vital cohesion of the molecules. Nay, it is not certain but that this influence is even greater than this. A high state of the dew point—implying as it does the existence of a considerable moisture—besides the effects mentioned, interrupts, as Dr. Lee suggests, the healthy functions of the skin and lungs—the insensible transpiration from which it diminishes or checks—thereby putting a stop or impediment to the cooling process resulting from the evaporation. It diminishes the functions of exhalation and absorption everywhere, hindering a natural escape from the blood of those azotized materials which contribute by their presence to render the system liable to zymotic diseases, or maintaining them in a condition favourable to the morbid change. It impairs also the power of the muscular system, and produces atony of the small vessels, and exercises, besides, an effect on the decarbonization of the blood. It carries off the vitreous or positive electricity which acts so important a part as a vital stimulus, deranges the energy of the organs generally, and causes languid circulation, laborious respiration, a sense of oppression, an increase of mucous secretions, bluntness of sensibility, and obtuseness in the organs of sense.

From these there result a feeling of languor and listlessness, an indisposition to mental and corporeal exertion, a state of depression and debility which predispose to the influence of cold and to the production of inflammation. This morbid state, though it never can, so long as the humidity giving rise to it is not aided by a peculiar and more active morbid element, constitute the yellow fever, may well be admitted to place the system in a condition suited to receive a morbid impression from that element, and may hence be classed among the more active predisposing causes of the disease.

After all that has been said on the subject, it is scarcely necessary to remark, that the cause in question is not the only one to which predisposing effects

¹ New York Journal of Medicine, November, 1853.

can be ascribed. Active as it may be, others are found calculated to produce a deep impression on the system. It may, indeed, be dispensed with, since instances frequently present themselves when the fever rages with even great severity during a condition of atmosphere the very reverse of moist. Nevertheless, I repeat, when the latter condition, connected with other causes of insalubrity, exists in any country, or at any season, the fever may be expected to prevail more certainly and extensively than it would otherwise do. We discover in this fact one of the causes of the habitual prevalence of the disease in tropical climates and in portions of our southern States, where the humidity is great, and the heat, in consequence, oppressive. In the middle States, and the South of Europe, where the humidity of the atmosphere, for the causes assigned, is much less considerable, and exists in excess only at particular seasons, and where the heat is less frequently and continuously oppressive than in tropical regions, the fever is of rarer occurrence, and requires for its production and epidemic diffusion a combination of circumstances seldom encountered together—as well, perhaps, as a primary cause of greater intensity than that which usually occasions the disease in climates where the predisposing influence in question paves the way to its spread.

Heat and Humidity combined.—From the foregoing facts and considerations, we may conclude that neither heat nor moisture, when acting separately, can be productive of yellow and kindred fevers. Equally objectionable is the belief that the disease arises from the combined influence of those two agencies, either unassisted by another cause of a more efficient kind and peculiar character, or with the aid of some agent calculated only to render the system more prone to the impress of the other. Dr. Barton says: “Let it be distinctly understood, that fevers do not prevail in proportion to the height of the dew point or amount of moisture alone, but that they do not prevail without a high dew point—that is, that a large amount of moisture, with a high degree of heat, is essential to the evolution or development of the high grades of fever.” “We have never had an epidemic yellow fever in this country without this combination.” “That there is a dew point peculiar to each of the high classes of fever (in their aggravated or epidemic grade) is doubtless true from what we know of the temperatures essential to their existence, and how greatly they are all injured by humidity. The dew point of yellow fever is from 70° to 80° —it rarely exists long where it is under 60° ” (pp. 292, 299). With this high dew point there must be a high range of atmospheric heat. When these are combined, and associated with any condition of soil calculated to deteriorate health, yellow fever will be produced—the combination in question being the main link in the chain of causation—while a declension of the temperature to less than 70° , and of the dew point to near 60° , puts a speedy stop to the epidemic existence of the disease (xiv.). In most of the instances mentioned in the preceding pages, humidity, when it existed, was associated with atmospheric heat, and yet the combination failed to prove injurious. Dr. Drake remarks, in corroboration, so far as autumnal fevers are concerned, that they seldom appear on board of vessels which cruise in the Gulf of Mexico, although the air, at the tempera-

ture of 80° , is nearly saturated with vapour; that the inhabitants of Key West, who breathe a similar atmosphere, are much less afflicted with the fever than those on the Peninsula of Florida, several degrees farther north; that the pine woods around the Gulf of Mexico, at the distance of only two or three miles from the estuaries of the rivers, are places of retreat from fever, although there is a sea and land breeze which tends to equalize the humid atmosphere; that the inhabitants of the Balize suffer less from the fever than those along the rivers of the interior of Louisiana, two or three degrees farther north, notwithstanding they are immersed in an atmosphere of great heat and vapour. He states also that, at our different salt works, the operatives spend their lives in a hot atmosphere, saturated with vapour, and yet, on the whole, are more exempt from fever than the surrounding population; and, lastly, that in some of our manufacturing establishments, the in-door artisans and operatives labour in a heated atmosphere supersaturated with vapour, but remain free from autumnal fever. To this it may be added, that, in the plains of Meta, situate on the east side of the Cordilleras, fever does not prevail extensively, while in the valley of the Magnalena, on the west of those mountains, it is of almost constant occurrence; yet the state of the barometer, as well as the hygrometrical variations, are the same in both places.¹

Occurrences of a similar character are noticed in reference to the yellow fever. Vessels in the Gulf, though immersed in the hot humidity above described, remain perfectly exempt from the disease. If the latter breaks out in them the cause is to be sought in some peculiar condition of their hull or cargo totally distinct from hygrometrical and thermometrical states of the atmosphere, or to their having approached too near an infected shore. In the West Indies, and on the African coast, where the atmosphere is usually—at those periods of the year particularly when the yellow fever prevails—at a high degree of humidity, if not at the point of saturation, and where the heat is considerable the yellow fever does not always or annually prevail; and there are spots where the combination generally exists, and which, nevertheless, are always free from the disease.

These various facts, showing the innocuousness of the combination in question, lead to the inference that, when the prevalence of the fever happens to be associated with the existence of heat and moisture combined, the efficient cause of the disease is not to be sought in the latter, inasmuch, particularly, as we find that the disease often breaks out, and spreads widely and fatally, though the atmospheric conditions in question are not carried to the degree mentioned. Humidity, therefore, if instrumental in producing the fever—and no one will deny the fact—must, as already stated, act in a secondary capacity; and, instead of being regarded as the efficient cause, not only when alone, but when combined with heat, must be viewed as merely aiding in the development of that cause, and in predisposing the system to its morbid effects.

So far as the views of Dr. Barton—to which attention has been called—are concerned, I have only to state that they appear to be based on data

¹ Boussingault, *An. de Chimie*, lvii. 153-4.

which may accord with facts observed in New Orleans, but not with those that may be collected elsewhere. From them, therefore, no law, in the strict acceptation of the term, can be established applicable to the yellow fever of all places and all climes. That a high temperature, and a certain degree of humidity, are required to produce, or rather contribute to the production of, the yellow fever, every one will concede; but that the effect will result every time the combination occurs, we cannot admit for reasons already stated. It will not necessarily occur even when the combination is associated with a variety of conditions of locality calculated to deteriorate health—the filth of our streets, courts, alleys, common marsh miasmata, &c. The dew point and temperature of Demerara are much the same in yellow fever and non-yellow fever seasons. The dew point in all the months of the year, and especially in those in which the fever appears, is above the degree Dr. Barton regards as necessary for the generation of the efficient cause, varying from 70.8 to 76.2° . The mean temperature of the year varies from 79 to 81° —presenting a difference in the several months of from 77.4 to 82° . At the same time, no one will contest the existence there of ordinary causes of insalubrity all the year round and every year, and yet, notwithstanding, the yellow fever has there appeared only at long intervals. The same remarks are applicable to St. Vincent's. There the hygrometer gives a usual annual average of about 69 ; varying from 67.14 to 70.25 . The average of the heat is about 83° ; varying from 82 to $85\frac{1}{2}^{\circ}$. There also the terrene causes, admitted by Dr. Barton, exist as they do in every other West India Island; but yellow fever is not an annual visitant of the place.

Nor is this all. Whatever may be the case in New Orleans, facts may be adduced to show that the yellow fever may break out and prevail with a dew point much below 70 or 80° , and that when once established it will often continue extensively and in an epidemic form, though the point it had before reached has fallen below 60° , and the thermometer is also reduced to less than 70° . The records of 1853, in this city, compared with those of several non-sickly years, will establish much of what is here stated, besides showing that the yellow fever broke out in July, and continued to prevail till the accession of cold weather, though the dew point did not reach, at any period of that year, an average of 60° . Again, the fever of 1793 continued, as we have seen, to prevail epidemically long after the temperature had descended below 70° , causing the death, in September, of more than one thousand four hundred individuals out of a population of less than forty thousand, though the thermometer barely averaged that degree—and of nearly two thousand in October, when the mean temperature of the month was about 60° .

Vicissitudes of Temperature will not account for the Occurrence of Yellow Fever.—Neither can we admit the propriety of referring the efficient cause of yellow and kindred fevers to the difference of temperature between day and night, or to mere atmospheric vicissitudes—the succession of cool or cold nights to hot days; nor to the sudden exposure of the body, at any period of the twenty-four hours, to a low degree of temperature after it has been placed for a greater or less extent of time under the influence of a high degree. The

agency of such vicissitudes was noticed as early as the days of Hippocrates. Hoffman sets them down as the general remote cause of epidemic fevers, and after him several writers, among whom Broussais and Dr. Dundas figure conspicuously, have laid much stress on their efficiency, and displayed considerable ingenuity in endeavouring to explain the manner in which the morbid effects are brought about. Dr. Metcalf, now of this city, in his remarkable work on caloric, ascribes fever to a reduction of temperature, and considers all the varieties of fever as modifications of the same disease, arising from different degrees in the intensity of the causes producing them; the extent to which the vital properties of the blood become impaired, determining either a quartan, intermittent, or the more deadly forms of typhus, yellow fever, &c. The doctrine has been upheld by other writers, as regards, particularly, the fever of New Orleans, and it would not be difficult to point out some of our own physicians who are disposed to look no further for the cause of the epidemics of Philadelphia than to the contrast between the heat of day and the cold of night, or to sudden changes that may occur at any period of the twenty-four hours. But, after carefully examining what the advocates of the opinion have adduced in its support, it appears to me that the theory fails. Were atmospheric vicissitudes the efficient agent in the production of yellow and other forms of malarial fever, we might expect to find these diseases prevailing principally in seasons in which the number of dew or cold nights following on hot days is greatest. We should be justified, also, in expecting yellow fever to occur not once or occasionally, but frequently, if not universally, more particularly in latitudes and places where such nychthemeral oscillations are most commonly observed, and to spare those where they are not met with. We should expect to find it whenever the supposed cause manifests itself; or, rather, it ought to be found that every time the fever prevails, sporadically or epidemically, the difference of temperature between day and night is greater than in healthy seasons. We should, besides, expect to find it appearing, not sporadically only, but in an epidemic form, as well in clean, well-paved, and well-aired cities, where atmospherical vicissitudes are as apt to be felt as elsewhere, as on the borders of rivers, on wharves, amid vessels, and in filthy lanes, alleys, streets, &c.; and whenever a man whose body has been overheated is suddenly exposed to a cold atmosphere, or plunges into a cold bath, he ought to be regarded as no less liable to suffer from a yellow fever, of the most legitimate kind, than from a pleurisy, a catarrh, or any other kindred disease. Lastly, we might expect the disease to spare individuals who shield themselves against such vicissitudes or contrasts of temperature.

But, so far from this being the case, experience shows that the fever occurs and prevails extensively in situations where, and at periods when, such vicissitudes are not felt at all, or are so too inconsiderable an extent to be productive of the baneful effects ascribed to them; and, on the other hand, that the disease is either seldom seen or completely unknown in localities where, or seasons when, sudden changes of temperature, or the contrast between night and day, are as common and noted as, if not more than, in places and at times remarkable for insalubrity. Surely,

a morbid agent which, if it really exercises any influence in the production of the fever in question, does so only in localities of a special kind, where, let it be remembered, the fever often appears, and even abounds, without its aid; an agent whose sphere of operation is circumscribed within the narrowest limits—a few hundred yards—attacking only those who venture within the small infected area, and sparing those who keep aloof; which affects only one house, or one side of a street; exercises great ravages on board a ship, and leaves others, close by, untouched; an agent which habitually fails to produce the same fever in localities of a different kind; which produces the effect ascribed to it only in a certain season of the year, however manifestly it may show itself at other periods; and whose known ordinary products, everywhere and at all times, are diseases very different in every respect from the one in question: such an agent, I say, cannot lay a just claim to be held in the light of the efficient or necessary cause of the latter.

The conclusion will appear the more natural when we bear in mind that the same agent exercises the very same influence in regard to every other disease of a zymotic character; that the fever in question, as well as many others, manifests itself occasionally days, and even weeks, after exposure to the place where the epidemic cause is known to prevail, and breaks out, after such exposure, in places where nothing of the sort exists; or where, if it had any influence in bringing on sickness, it ought to occasion a disease common to the place, and not one existing in the locality which the individual had left. Vicissitudes, if really the efficient cause of yellow fever, appear to be whimsical in their operations. The meteorological tables published in the account of the voyage of Dumont d'Urville to the South Pole and Oceanica, show conclusively that the minimum degrees of nychthemeral oscillations occur in hot latitudes, the difference between the maxima and minima amounting only to a very few degrees. In temperate and cold climates, these oscillations are much more marked; and yet the yellow fever is a disease of hot climates. There it occurs frequently—in some parts almost annually; while in temperate climates, where the vicissitudes in question are constant, the fever only occasionally, and in many places never, shows itself. In hot climates themselves, places subject to considerable oscillations are free from the disease, while others, where the changes are unimportant, are not unfrequently visited by it. Humboldt and Bonpland inform us that, in the healthy city of Quito, the oscillation amounts to seven degrees (Cent.), while in Cumana, which is low, unhealthy, and subject to fever, the difference of temperature between day and night does not exceed three or four degrees. At Caraccas, where true yellow fever has seldom, if ever, prevailed, the temperature is continually changing; so much so, indeed, that the inhabitants complain of passing through several seasons in one day.¹ At Martinique, where the yellow fever is of frequent occurrence, the oscillations are very trifling. See how these oscillations comport themselves in Charleston. The autumnal seasons of 1849 and 1852 were decidedly malarial. During the months of July, August, and September of

¹ Voyage aux Régions Equinoxiales du Nouveau Continent, iv. 185, 8vo. ed.

the first, the number of nights of dew, with high temperature during the day, amounted to fifty-one, and far exceeded that of 1845-'8 and 1851, and by eight that of 1850, in neither of which years did the yellow fever prevail. So far, therefore, there seems to be some connection, as cause and effect, between cold nights with hot days and fever. But when we come to inquire into the results obtained in 1852, which was a decidedly malarial year, as evidenced by the occurrence of both the yellow fever and dengue, we find that the number of dew nights amounted only to thirty-nine, or four less than in 1850, when there were no malarial developments, and the range of the temperature was greater. For this fact we are indebted to Dr. Hume,¹ already referred to. The range in 1849, during July, August, and September, amounted to 1,524 degrees (the minimum or night temperature being 63.92, and the maximum or day temperature 79.16), and exceeded by far that of 1845, 1846, 1847, 1848, 1850, and 1851. But the range in 1852 was only 1,284 (minimum 66.25, maximum 78.91), being only 26 degrees higher than the range of 1851, and less by .82 than that of 1850, when the minimum was 68.00, and the maximum 81.58. In a word, the resemblance of 1850 and 1851 with 1852 is too striking in this and other respects to make us attach much importance to this point.²

It may not be improper to remark that persons who take the fever are seldom sensible of having felt the effects of atmospheric vicissitudes, and often have been placed, purposely or accidentally, beyond the possibility of experiencing these—supposing them to occur; and that the cold incident to such vicissitudes impresses organs different from those concerned in the development of yellow or malarial fevers. Producing its first impress on the skin, it affects most frequently those parts which act as supplements to that eliminating tissue—the lungs and the urinary passages. Other parts, of course—even the abdominal viscera—are sometimes implicated; but the symptoms which then occur bear no resemblance to those of yellow fever, unless other causes have co-operated. That the cold felt under the circumstances is instrumental in exciting or multiplying attacks of that fever, is no doubt true; but it does in regard to the disease what it affects in regard to many other complaints, and it would be wrong, from its agency in those cases, to argue that it is the efficient cause of the disease, since alone it cannot give rise to anything of the kind. More rational is it to view it as having simply aroused from its slumbers a morbid influence floating in and impressing the system in a slow and invisible manner, and requiring, for its outward manifestation, the disturbing influence of some other agency. The same result attends the action of atmospheric vicissitudes in regard to diseases of a specific nature, and which no one would have the hardihood to ascribe solely to such a cause. What the latter does as respects malarial fevers, it does, for example, in regard to mercurial ptyalism in persons whose systems have been impregnated with mercury; but in whom the affection of the mouth has not yet been developed. In such instances, exposure to atmospheric vicissitudes—to cold, however applied, espe-

¹ Charleston Journal, viii. 67.

² Hume, *op. cit.*, p. 58.

cially when the system has been previously heated—will hasten and increase the elimination of the poison, and salivation will be established. The same remarks apply equally well to hydrophobia, which has not unfrequently been developed through the agency of the cause in question. Nothing is more dangerous than the action of atmospheric vicissitudes, or cold, on individuals bitten by the lanceolated trigonoccephalus of the Antilles. The same injurious effect is said to be experienced by individuals who have been exposed to the action of the mancinella and the *Rhus toxicodendron*.¹

In all these instances of change from heat to cold, the latter has acted simply as an exciting cause, and, as it acts much in the same way, must be ranked with several other agencies, intemperance in eating or drinking, venereal excesses, exciting and depressing passions of the mind, &c., which no one in his senses can feel disposed to regard as capable of producing yellow or malarial fevers any more than of producing ptyalism or hydrophobia.

Dr. E. H. Barton is impressed with the opinion that solar radiation, derived from the difference between the temperature of the sun and shade, as a source of disease, has not attracted sufficient attention, though no observant practical man, who has passed through many epidemic yellow fever seasons, can have failed to notice the peculiar weather that usually exists during the *clear* days of those seasons. "In fact," he remarks, "old, experienced men, out of the profession, have been in the habit of denominating it '*yellow fever weather*,' without analyzing the conditions which constitute it. It is characterized by being very hot in the sun, and cool in the shade at the same time; on one side of the street a broiling temperature, and on the other so cool as to urge to buttoning up the coat. The uncomfortable alternation of chilliness and heat is productive not only of uncomfortable feelings, but, when exaggerated, passes into disease—constitutes the first stage of yellow fever." The difference of temperature between sun and shade is at these times very great, and essentially constitutes, with other circumstances, a sickly season. Dr. Barton's attention has been called to it for many years; but he has not remarked it to influence materially other diseases beyond the class of fevers, except *coup de soleil*, of which, doubtless, it is the principal cause. He adds that it may be, so far as concerns the yellow fever, only the exciting cause, developing dormant disease, from the predisposition being already present.² Further and more extended observation will be required to decide whether the radiation is similar in other places visited by the yellow fever, and whether the peculiarity in question is an essential attendant on all epidemic seasons. But, whether it be so or not, from all that precedes, and from the fact that this difference influences not the yellow fever only, but the whole class of pyrexia, and is the principal cause of *coup de soleil*, it is impossible to admit the propriety of regarding it as doing more than exciting into action a disease produced by some other cause.

¹ Boudin, Géog. Méd., p. 67.

² Sanitary Condition of New Orleans in 1853, Rep., p. 300.

CHAPTER IX.

CIRCUMFUSA, CONTINUED.—WINDS.

MUCH as has been written on the subject of the agency exercised by particular winds in the production of yellow fever, I am not certain that any conclusion has been reached of a definite or satisfactory character, and applicable equally well to the various regions visited by the disease. For while, by some, the effect has been ascribed to some special current proceeding from a given point of the compass, by others, other currents are pointed out as the source of the evil; and yet, in several instances, the winds thus referred to are of opposite character—as well in regard to the qualities of the air they waft, and the effects they thereby produce on the system, as to the localities over which they pass.

Aerial currents, as productive and disseminative agents of febrile complaints, produce their effects in several ways, both direct and indirect. They act on the system, 1. Through the medium of the degree of temperature to which they give rise, or which attend during their prevalence, overpowering us by heat when they blow from the south, or chilling us by cold when they proceed from a different quarter. 2. Through means of the hygrometrical character of the air attendant upon them, or which they tend to produce or promote. 3. Through that of the electrical condition of the atmosphere resulting from the qualities of the latter, or connected with it in some way. 4. Through the influence of noxious and malarious poisons they are instrumental in diffusing, and to the evolvment of which they contribute by virtue of some of the various atmospheric conditions which, as just stated, are in some degree under their control.

From this extended survey of the agency of winds in the matter in question, we may infer, that the agency in question is exercised directly on individuals exposed to it, by placing these in a condition calculated to render them more susceptible to the impression of a remote or efficient cause; or by exciting in those so exposed the development of the disease to which they were already predisposed. We may also infer that the agency in question is exercised indirectly by promoting the elaboration of that cause, or disseminating it when already formed from the place where it is evolved to distant parts.

Applying these remarks to the influence which winds may exercise, so far as regards the causation and extension of the yellow fever, it will be easy to perceive that the effect produced by any given one, whether direct or otherwise, will not be similar in all places and in all corresponding seasons, but must vary according to the natural qualities of the localities and surface over which it passes, or, in some cases, to the accidental and momentary con-

ditions of those localities, as well as to the relative position existing between the sources of the effluvia it carries along with it and the masses of individuals placed within reach of its influence. The same wind which in one place is characterized by dryness of atmosphere, in another is attended with humidity. In one locality it accompanies a low temperature, in another it is attended with more or less heat. In some places the air it propels is the vehicle of effluvia inimical to health, in others it is the promoter of atmospheric purity. Hence, in some places, the same wind is the harbinger of pestilence, while in others it proves perfectly harmless, or even beneficial. Furthermore, it not unfrequently happens, that in the same locality the same wind which in one season had blown prior to or during the continuance of the pestilence, and appeared to contribute largely to its development and diffusion, in other seasons proves singularly innocuous, from the absence, evidently, of other agencies, the concurrence of which is requisite for the elaboration of the true remote cause.

But however this may be, it is a fact which the experience of all places cannot fail to confirm, that the elaboration of the agent giving rise to the yellow fever, or the state of predisposition to its impression which may exist among individuals moving within the sphere of an infected locality, has very generally, if not invariably, been connected in some way, in temperate regions particularly, with the prevalence, during a greater or shorter space of time, of southerly currents. Nor could this be otherwise; for this fever requires for its development the long continuance of a high range of thermometrical heat, which could seldom be obtained during the prevalence of opposite currents. Under the influence of these winds, whether from due south, or from the neighbouring points, S. E., E. S. E., or S. W., accompanied as they are, in yellow fever climates and seasons—and, indeed, in many if not most malarious regions—with heat, dryness, or humidity, as also with infectious exhalations, the fever originates and spreads to a greater or less extent; while from a change of wind from these to different points of the compass, modifications of a beneficial kind result. The malignancy of the disease becomes less intense—the mortality to which it gives rise lessens—the number of cases diminishes—and, in many instances, its epidemic progress is arrested for a time, or completely.

In the West Indies, the heat of the atmosphere is modified by the sea breeze, which blows with nearly uniform force from one direction during nine months of the year. “It is termed the trade-winds, and generally comes from the east and its collateral points, except from August to December, when it veers around, and blows slightly from the south and west, with frequent calms at intervals.” “A land-breeze, in all the large and mountainous islands, blows with almost equal regularity at night.”¹ In the small islands, and those in which no mountains exist, either the land breeze does not blow, or does so slightly. The sea breeze generally sets in between 10 and 11 A. M., and blows with increasing force till 3 P. M., and dies away about sunset. Shortly after, the land breeze commences, and blows until sunrise.

¹ Tullock's Rep., p. 4.

These easterly winds—the pure east and the northeast especially, which are the most predominant in those islands—originate at some hundred leagues from the coast of Africa, and pass over the Atlantic at the rate of eight leagues an hour (*Bally*, p. 361). Refreshing the body, exhilarating the spirits, and enabling the inhabitants to endure the intense heat of the day without considerable inconvenience, they are the usual attendants of the healthy season; and, so long as they blow with proper force and regularity, the yellow fever seldom, if ever, shows itself to an alarming extent. Very different, however, appear to be the effects of other winds in regard to the development of the disease. In tropical climates, where the most frequent variations are from E. S. E. to E. N. E., and where the wind never remains long at N. E. or S. E., and rarely blows from due north or due south, the prevailing current is always attended with considerable heat and moisture; and with few exceptions, when the wind blows from the west longer than usual, and when the heat is nevertheless very great, as happened at Martinique in 1839, the prevalence of fever is very usually associated with southeast or easterly currents. The remarks of Lempriere on the subject, though having special reference to Jamaica, will apply to all the islands. “The sea breeze, when it blows in its regular track, refreshes the body, exhilarates the spirits, and enables the inhabitants to bear the most intense heat without any considerable inconvenience. But when it blows from the southward, the atmosphere is more frequently loaded with moist vapours, in the form of low clouds; the air is highly oppressive, the spirits are depressed, and such a temporary relaxation, both mental and corporeal, is often induced, as to render many people, otherwise active, nearly unfit to go through even the common duties of their employment. It is this kind of weather which prevails previous to the setting in of the seasons or heavy rains, and, by its relaxing tendency, no doubt subjects the body to that fatal form of disease which occurs after that period.”¹

That the influence of these winds—of the S. W. particularly—has been doubted by respectable authorities, I know full well. In speaking of the epidemic of Martinique in 1838, Dr. Rufz remarks in reference to the influence of the wind, that it blew almost constantly from south to north, passing by west, where it sometimes remained stationary during several days (p. 29). But further on, as regards the fever of 1839–40, he says that the disease having, on that occasion, prevailed all the year round, and the winds varying according to each season, it is not probable that this circumstance (the prevalence of southwest winds) could have had, on the production of the disease, the degree of importance which was at first attributed to it.² If, however, we turn to other writers, we shall find that the innocuousness of these and other winds, even on the very theatre of Dr. Rufz’s observations, may reasonably be doubted. In reference to the land breeze in St. Domingo, Dr. Bally (p. 361) remarks, that it is a source of evil either in consequence of the suffocating heat it occasions, or because it passes over an infectious locality. To its injurious effects upon the system he attributes the dreadful mortality which

¹ Diseases of the Army in Jamaica, i. 17.

² Rept. by Chervin, p. 54.

occurred among the troops during the fatal expedition under General Le Clerk.

To S., S. E. and S. W. winds, an injurious influence is almost universally ascribed. Though less frequently prevalent than those from the East and N. E., they sometimes blow, for the space of twelve or fifteen days consecutively, or even longer, and whenever this is the case, the pernicious action they exercise never fails to exhibit itself. On this subject, we may safely appeal to the testimony of the best writers on the climate and diseases of the West Indies, who have dwelt on the injurious effects of those currents, or noted the connection of their prevalence with epidemic manifestations of the disease.¹ Dr. Catel, who describes the epidemic observed at Martinique by Dr. Rufz (1838), states that, during twelve months of the time the disease prevailed (from 1st October, 1838, to last of September, 1839), the wind blew 203 days from a southerly direction, S. and S. W. (p. 20). These, and the west winds, which are light, hot, and damp, and pass over the Gulf of Mexico, Dr. Catel feels disposed to regard as the cause of the various epidemics of that island. "Everywhere within the tropics," he remarks, "they aggravate the disease. It would be difficult to say why those winds are so unfavourable to health; but it matters not, since their effects are known" (pp. 7, 8).

In his report on a later epidemic, 1843, Dr. Catel states, that in the course of the third quarter of the year, south winds prevailed during some portions of fifty-four days, and he has no doubt that the occurrences of that sickly season point out satisfactorily the influence of those winds on general health, and particularly on the production of the yellow fever. In July, the wind blew from the south six times, and only one death from acute disease occurred among the troops. In August, the wind blew from the southward eighteen times, and the fever appeared sporadically. But in September, the latter assumed the epidemic character and prevailed extensively under the influence of southerly winds, which blew thirty days. "In the presence of these facts," M. Catel says, "it is impossible to doubt the influence of these winds on the production of the yellow fever; and we may say, with full confidence, that this disease is but an effect of these deleterious winds."²

Another of the writers, to whom I have referred below, and whose ample experience and great accuracy of observation cannot be doubted, Dr. Lefort, remarks on the subject: "The development of the yellow fever in the West Indies, in a great number of men at the same time, in different parts, at a distance from each other, on a level with the sea or slightly above, on board vessels

¹ Desportes, i. 19; Lempriere, i. 17; Bally, p. 361; Gilbert, p. 12; Humboldt, p. 765; Rochoux, p. 113; Lefort, *Du Quinquina et de la Saignée*, &c., p. 66; *Ib.*, *Mém. sur la non Cont. de la F. J.*, p. 9; Leblond, p. 81; Lind on Seamen, p. 67; Dariste, p. 33; Arnold, p. 26; Savarésy, pp. 189, 242-3; Moreau de St. Méry, i. 716; Chisholm, i. 88; Chervin's Report on Rufz's Mem., p. 45; Catel, p. 20; Olivet, p. 6; Vincent, p. 7; Maher, p. 836; Moreau de Jonnes, *Climat. des Antilles*, p. 63; Brette, *Rapt. Méd. sur l'Epid. de Fièvre Jaune qui a régné aux Saintes*, &c., en 1838; Achard and Regnier, *Rapt. du Conseil de Santé de la Martinique*, &c.

² *Annales Marit.*, 1844, iv. 22, 221-2.

in port or at sea, coincides so exactly with the increase of heat and humidity, and with the prevalence of the south winds, that it is impossible not to recognize in these meteorological conditions the true cause of the epidemics of yellow fever."

He further remarks, that during the general epidemic of 1825, the wind was more usually at S. S. E. and S. S. W., and that the heat was more intense than in ordinary seasons.¹ In Martinique, from the opening of 1823 to 1825, the wind was mostly from the east and E. N. E., and never blew, for any consecutive time, more than twenty-four hours from the south. During no part of this long period did the yellow fever prevail. But, in May, 1825, the wind changed to S. S. W., accompanied with storms, rain, and intense heat. Then southerly currents, and a temperature of 24° to 27° (R.), and even greater, prevailed with slight modification at St. Pierre till the month of December, when the wind passed to east and N. E., attended with a reduction of several degrees in the temperature. During this long period, the yellow fever prevailed without intermission. It ceased the next year, 1826. Much the same result obtained at a later period. From 1828 to 1838, the fever did not show itself. During that period of exemption, the southerly winds did not prevail. In the latter year, they again began to blow, and with them the fever made its appearance (*Rufz*, pp. 44, 45). Governed by the above facts, Dr. Lefort is disposed to regard south winds, combined with heat and humidity, as the cause of the yellow fever.² In another publication he remarks: "The effect here is intimately and necessarily connected with its cause. The production and extension of yellow fever under the prolonged influence of the south winds, is a fact observed by every one; so inevitable, indeed, that it can be predicted without fear of ever being mistaken. This action of the south winds is felt here by everything that breathes. They produce undefinable effects on our senses. We feel them in bed, or sitting at the desk; they enervate, cause oppression, and depress the spirits. To say in what these atmospherical changes, the effects of which are so much to be dreaded, consist, and to seek to determine their specific nature, is doubtless a task beyond the faculty of man." "It is a fact, I cannot say it too often, that yellow fever is never developed in the same manner as other epidemics, but by and under the influence of the south winds. When this general cause fails, the yellow fever only exists in certain circumscribed places, or only a few sporadic cases appear here and there, as occurred in 1819 and 1820. There were only, in all, forty-two men attacked by the yellow fever in the Hospital of Fort Royal, in 1820, notwithstanding the garrison, including soldiers, seamen, and military workmen, was very large; but in that year the wind did not blow from the south, or did so only occasionally, and never more than twenty-four hours at a time. It may, moreover, be remarked that this injurious influence of the south wind, or of winds from neighbouring points of the compass, is observed under the same parallels of latitude, or in corresponding parts of our hemisphere."

¹ De la Saignée et du Quinquina, op. cit., p. 589.

² Mém. sur la Non-Con. de la F. J., p. 9.

In Vera Cruz, the brisons, or S. E. winds, blow with tolerable regularity from March to September. The fever appears soon after they commence; but assumes a truly epidemic character in September and October, after they have already prevailed some time,¹ and we find in Dutch Guiana that, during the rainy or unhealthy season, the wind, at night, blows from S. E., passing over the immense marshes and forests which cover the soil and render the air excessively damp, cool, and poisonous. But to the injurious effects of the south wind, and beneficial effects of east winds in the West Indies, there seem to be some exceptions. In the city of the Havana, as we learn from Dr. Osgood (p. 29), when the wind blows from the east or the west along the sea-coast, the yellow fever prevails more generally among strangers, both in the harbour and on shore, and is more genuine or unmixed in its character than at other times, when the direction of the wind is from the open sea on the north, or from the land on the south.

In temperate regions, the concurrence of south winds ought, it seems, to be even more requisite than in the West Indies; for the elevated temperature required for the elaboration of the yellow fever cause, may and does exist in the latter, as is seen during some sickly seasons, without the aid of such currents; whereas, in the former, the heat seldom, if ever, attains the degree alluded to, or remains at it long, except under the influence of the south wind, and its collaterals. Indeed, it is doubtful whether in our latitudes fevers of a malignant character have often been found to prevail under a much different condition of aerial currents. It was under the influence of the warm winds that the fever described by Hippocrates, and which approximated in some respects to our yellow fever, appeared. Their agency at Leghorn is recorded by Palloni (p. 23).

In Norfolk, in 1795, and in New Orleans, in 1839, the south wind accompanied the disease, and in proportion to its continued prevalence, the latter became aggravated, and extended its sphere of action. In Baltimore, 1819, S. E. and S. W. winds prevailed frequently during June, July, and August. In Norfolk, in 1821, S. W. winds prevailed from February to July, inclusive. In Boston, in 1798, the air blew mostly, during June, July, August, and September, from S., S. E., S. S. E., S. W. In New York, in 1795, the direction was, for the most part, from S. E., though at times from N. E., and in the year 1822, the wind, during half or two-thirds of the time, from May to September, inclusive, blew from S. S. E. and S. W. At Natchez, in 1823, light breezes generally prevailed, during the heat of the day, from S. W. Nearly similar was the case at Marseilles, in 1811, and at the time of the dreadful epidemic of Barcelona, in 1821, the prevalent winds were, as usual, in June, July, and August, from S. S. W. or S. E. Dr. Rochoux states that at the commencement of the disease, the wind blew in a S. W. direction during six weeks; and, at a later period, south and west currents became predominant.²

¹ Humboldt, p. 765.

² Caldwell on Malaria, p. 135; *Ib.*, *Med. Repos.*, vii. 144, 145; Barnwell, p. 366; S. Jackson, p. 40; Waring, p. 23; *New Orleans Rept. for 1839*, p. 156; *La Revue Méd. for 1840*, p. 322; Rand, *Med. Repos.*, ii. 466; Valentin, pp. 85, 86, 89; *Letters, &c.*, rela-

In the city of Philadelphia, the wind, in 1793, blew mostly at first from the southward. In 1820, the direction was almost constantly from the south, and the same remark is applicable to the sickly season of 1853.

But, while such appears to be the connection between the prevalence of southerly winds and that of yellow fever in some parts of temperate regions, it will be found that in other places, and at other times, the disease breaks out and prevails under the influence of different currents. Chabert states that, at New Orleans, the yellow fever is not produced during the prevalence of S. W. winds, as is the case in some other countries, but springs up during north winds, assigning, in explanation of the difference, that the latter pass over a malarious locality; whereas, the south winds come from the sea and are cooled while ascending the river (p. 26). The same statement is made in a report to the Medical Society of that city on the epidemic of 1819.¹

Dr. H. S. Hawkins, U. S. A., in a report dated August 6, 1839, says: "The S. W. and S. E. winds prevail during the five months, from April to August, and N. E. in September. It is to be remarked, that E., N. E., and S. E. winds come from the Gulf of Mexico, over an immense tract of low swamps, and that the prevalence of north and east winds in July, August, and September, is always attended with the epidemic yellow fever. In fact, these three months are the only ones that can be considered as proper seasons of disease, that is, the cause of epidemic yellow fever is produced during these months. Its ravages may and do extend into October, but when there has been no epidemic during August and September, strangers are not as liable to disease in October. It has also been remarked, that during an epidemic, for example, in September, if the wind prevails steadily for a few days from the S. W. or W., the disease seems to be checked, fewer new cases occur, and those who are sick recover more readily. If, after this state of things, the wind shifts around again to the N. E. the disease resumes its virulence, cases occur more frequently, and those who are convalescent are suddenly thrown back, and frequently succumb."²

Dr. Gros, of New Orleans (p. 5) states, in reference to the epidemic of 1817, that, until the end of June, the wind blew from the east and south. It then turned to the north, and continued in that direction during July, August, and September, attended with considerable heat and moisture. We learn, however, from Dr. Barton (p. 9), that, during the early summer of the epidemic year of 1833, easterly winds blew almost all the time, "with their remarkable influence, as every one observed, in lowering the healthy tone of the body, probably from depriving it of its electricity, blasting with its sirocco breath the vitality of the system." In 1820, the prevailing winds

tive to the Fev. of Baltimore in 1819, p. 95; Archer, Recorder, v. 61; Bailey, Fev. of New York in 1795, p. 55; Townsend, &c., 1822, pp. 263, 268; Merrill, Phil. Journ., ix. 233; Barton, Fev. of New Orleans, in 1833, p. 9; Robert, Guide Sanitaire, &c., pp. 110, 111, 113; Pariset, Fev. of Barcelona, in 1821, pp. 7, 176; Rochoux, p. 113.

¹ Chabert, viii. 9, Report by Dr. Trabuc, p. 50.

² Statistical Report of Sickness, &c., in U. S. A., p. 268.

were from the south, but about the period of the disease they changed to the north and northwest; and such variations were immediately followed by an increase of the malady.¹ In 1853, the wind blew as follows:—²

	S. Days.	N. Days.	E. Days.	W. Days.
June	11	9½
July	11	9½
August	2	7¾	9	5
September	3	4¼	12	...
October	16	9¾	...
November	12	12½	...

In reference to this epidemic, Dr. Barton remarks: "The winds have been nearly one-third more easterly than during the last five years, especially during the epidemic; more northerly—not half the usual southerly winds; about one-third more of westerly winds."³ Dr. Barton states that at the commencement of the disease, basing his calculations on the results obtained during fifteen epidemics, the prevailing winds are from the southeast and east. At the maximum they blow from east and northeast, and at the declination they blow from the northeast, north, and east.⁴

In Savannah, according to Dr. Waring, east and northeast winds are unwholesome. In all fatal seasons these winds have predominated. They pass twelve miles along the river, and absorb all the moisture and all the poisonous gases connected with it.⁵

In Mobile, the south and southwest winds are usually most prevalent in summer. They are the most healthy. Sometimes, however, they will be from the north or northeast for several days in succession, a change said to be always attended with unfavourable effects on the body. In 1819, the weather cleared off, after a long spell of rain, on the evening of the 10th of September, wind northwest, with hot sun, and so continued for sixty-six days, to November 16—the wind varying from northwest to north and northeast.⁶

"Yellow fever," says Dr. Bone, "prevails on Brimstone Hill, St. Kitts, when the strong north winds that have swept foul ground on Mount Misere impinge upon the persons in the ill-constructed barracks and out buildings on that hill. And in Tobago, Dominica, Grenada, St. Vincent's, and in all the hilly uncleared islands of the West Indies, strong north and east winds and rain, impinging upon the troops and their families in ill-constructed barracks, are causes of the disease."⁷ "In St. Domingo," says Desportes, "the acclimated inhabitants regard the north wind as unwholesome, while the south wind is very pernicious to new comers. This is particularly noted in the plain of the Cape, because the northeast and north-northeast winds, before reaching it, pass over a large surface of marshy localities, from the effect of

¹ Report to the Med. Phys. Soc. of New Orleans, p. 5.

² Fenner, Trans. Med. Assoc., vii. 427–8.

³ Report of Sanit. Commiss., p. 242.

⁴ Introd. to ditto, p. 13.

⁵ Report, p. 23.

⁶ Report on the fever of that year, in Letters, &c., on the Fever of Baltimore, p. 203.

⁷ Essay, p. 15.

the exhalations of which the natives, who are proof against seasoning fevers, are not exempt."¹

In the fevers of Cadiz, Carthagená, Seville, and Gibraltar, the east wind, as we have seen, has invariably been found to play a most important part.² In speaking of the Levanter, or east by south (southward of east), Sir James Fellowes says: "The best practitioners have observed that, while it continues, all the complaints produced by a morbid state of the bile very frequently occur, especially cholera morbus, bilious diarrhœa, &c.; and that in general it so affects the human body, and occasions such debility, uneasiness, and apathy, as to unfit men for their ordinary occupations" (p. 14). "The whole circulating system is influenced by it in a very extraordinary manner: the fibres become irritated, the quality of the bile itself is altered, and the most pacific, quiet temper is rendered irritable, so that quarrels, wounds, and assassinations are said to be never more frequent than during the prevalence of a Levanter, which, in the province of Andalusia, is called the 'wind of discord'" (p. 13). "It has been shown," Sir James continues, "from experience, that the Levant wind has always had a considerable influence over the Andalusian fever. The disorder has usually made its appearance in Spain after the prevalence of easterly winds. This fact, observes Gonzales, although not sufficiently conclusive to enable us to account for the production of the disease, is, however, strong enough to induce us to consider it as one of the most exciting causes" (p. 15).

In Sicily, too, where the fever is of a character allied to that of the yellow fever, east and west winds, for the most part, prevail from the commencement of June to September, and consequently before the appearance and during the continuance of the disease;³ and we know that in Philadelphia, however it may be in some sickly seasons, it was found that, during the months of July, August, and September, of the years 1793 and 1797, the wind blew less frequently from the south than from other quarters.⁴

Pariset compares the Levanter at Cadiz to the khamsin of the Arabs, the sirocco of Naples, and the catia of Caraccas; it parches the ground and dries up vegetables, gives a remarkable expansion to the blood, irritates the nervous system, and blows on Cadiz both diseases and crimes. It uniformly exasperates the yellow fever, and if it does so, it can surely tend to produce it. The truth is, that, during the great epidemics of 1800 and 1819, it blew constantly for a period of three months. Ameller says: "Sucedio por el espacio di qua-

¹ *Maladies de St. Domingue*, i. 21.

² Tullock's Report, p. 4; *London Quarterly Review*, lxvi. 135-6; Vance's Rep. in Pym, p. 60; Fellowes on Pestilential Fever, pp. 13, 15, 469; Berthe, *Fièvre Jaune*, p. 51; Pariset, *Fièvre Jaune de Cadiz*, p. 96; Gillkrest, *Cycl. of Pract. Med.*, ii. 279; O'Halloran on *Yel. Fev. of Spain*, p. 14; P. Wilson, *Fev. of Gibraltar*, Chervin's translation, p. 9; Rochoux, p. 113; Gilpin, *Med.-Chir. Trans.*, v. 339; Bally, p. 363; Amiel, in Johnson on *Tropical Climates*, p. 260; Townsend's *Spain*, p. 340; Maclean on *Epidemics*, ii. 485; Burnett, *Dis. of Mediterranean*, p. 340.

³ Boyle, *Edinb. Journ.*, viii. 178.

⁴ Rush, iii. 120; *Ib.*, iv. 5; Currie, *Fever of 1793*, pp. 15, 16.

renta dias un viento leste constante y recio, que siendo sumamente caliente en está pays, en ardecio a sus habitantes, que sudando copio samente non tenian mas consuelo que en el tiempo que estaban en el baño."¹

In 1828, at Gibraltar, contrary to what appears to have been the case in 1804, there was no unusual prevalence of easterly winds, either preceding or during the progress of the epidemic. It appears that, in the three months ending August, 1828, there were only thirty-nine days of easterly wind, and only nineteen in the months of July and August; whilst in the corresponding three months of 1827 (a healthy year) there were fifty-nine days of easterly wind, twenty-nine being during July and August.² In the non-epidemic year of 1811, the east wind was more prevalent during September, October, and from the 1st to the 13th of November, than during 1813; for while in the latter it blew forty times from that quarter, it did so fifty-three times in the former.³

The details into which I have entered will show, beyond the necessity of further demonstration, that the yellow fever occurs, in different countries, under the influence of different winds. In the greater portion of the West Indies, it would seem to be brought on through the agency of, or to be attended with, the prevalence of south winds; while in the Havana this wind is comparatively innocuous, and the east and west winds exercise injurious effects. In some parts of this country, and in this city particularly, it has appeared after and during the prevalence of south winds, sometimes during the occurrence of west winds. In other localities it has required an east or a north wind. Nor is the same difference less strikingly noticed elsewhere. In Leghorn, it occurred under the influence of south winds; in Barcelona, of southeast and southwest winds; while in Andalusia and Gibraltar it has been almost invariably in some way connected with the prevalence of the east or Levanter wind, and was never produced by or associated with a south wind.

From the diversity of result arising from the same wind, and the sameness of effect resulting from currents of different character, we derive the proof that no particular wind can be said, with any show of reason, to constitute, by itself, the necessary and efficient cause of the disease; and that whenever any of them exercises an agency as regards the origin or diffusion of the fever, it derives that power not from the fact of its coming from any particular quarter, but from the temperature and hygrometrical conditions of the moving column of air, and more especially, perhaps, from the injurious effluvia it raises from the localities and surfaces over which it passes, and which are carried along with it. How far the degree of heat thus created, or the hygrometrical condition of the air thus promoted, are instrumental in aiding in the development of the disease, has already been examined. It was then shown that excessive and long-continued heat, as well as dryness and humidity, should be held only in the light of predisposing or exciting causes, and probably as essential agents

¹ Descripcion de la enfermedad epidemica que tuvo principio en la ciudad de Cadiz, por Don Carlos Franco Ameller.

² Gillkrest, in Quarantine Report of General Board of Health, p. 178.

³ Gilpin, Med.-Chir. Trans., v. 339.

in the evolvment of another and more efficient cause. The Levanter, as we have seen, before reaching Cadiz and other parts of the adjacent territory, sweeps over a large tract of the opposite shore of Africa, and a considerable portion of the heated fields of Andalusia and Grenada, and thereby becomes parchingly dry and hot.¹ On the contrary, at Gibraltar, it has quite an opposite character, being surcharged with moisture during its transit across the Mediterranean. It is consequently always damp, raw, and unpleasant, and when from the southward of east is generally accompanied with thick fogs, which envelop the rock.² From the division of the kingdom of Grenada into two parts, by high mountains, there results that Malaga and its environs feel but little the effects of north winds. While south winds are there very hot, the east wind, or Levanter, is cool, because it passes over the Mediterranean. San Lucar and Xeres, are protected from the south winds by the Sierra de Xeres; Grenada is shielded from the southeast by the Sierra Morena; Murcia, where the fever prevailed, is situated in the centre of a dry and hilly country, over which winds exercise but little influence, as the currents are impeded by the inequalities of the soil. There the climate is delightful.³ This difference in the hygrometrical condition of the atmosphere during the prevalence of winds attendant on the development of the yellow fever, points to the fact that such winds, as well as south winds in the West Indies, &c., must be viewed as capable of acting (so far as they are instrumental in creating these conditions) as well as heat, as exciting or predisposing causes. If they exercise a different and more direct agency in the production of the disease, it is by aiding, through the peculiar conditions of atmosphere they occasion, in the development of effluvia injurious to health, and contributing to their dissemination to more or less distant parts. Scarcely a place visited by the disease can be pointed out in which conditions of locality favourable to the evolution of the yellow fever cause may not be discovered; and wherever such is the case, and the degrees of heat and humidity required for the production of this cause exist, it will be wafted by the wind which may chance to blow over the infected district before reaching such localities as are exposed to its action. The north, southwest, and west winds, in New Orleans, pass over an unhealthy district. The latter two blow over an extensive region of swamps, and the bed and banks of the river for upwards of eighty miles. The easterly winds, in Savannah, pass twelve miles along the river, absorbing all the moisture, and all the poisonous gases connected with it.⁴

In Boston, in 1798, the south winds wafted the exhalations from the wharves, stores, and docks at the foot of the hill upon the inhabitants that resided on those sides of it.⁵ The same winds, S. E. and S., produce the same effects in New York.⁶ The winds during the summer are generally from the sea, and arrive at the lower part of the town uncontaminated by passing over unwholesome grounds. Yet, such is the fact, that the poisonous

¹ Fellowes, p. 33; Bally, p. 363, &c.

² Report on Sickness, &c., in the Mediterranean, p. 4.

³ Bally, p. 363.

⁴ Waring, p. 23.

⁵ Rand, Med. Rep., ii. 466, 468.

⁶ Osgood, p. 29.

exhalations which abounded in that quarter during the warm weather of 1795, so changed the air that the inhabitants on the south side of Pearl Street, between Old Slip and Whitehall, almost all concurred in their testimony that the disagreeable effluvia frequently compelled them to close their windows, especially in the evening on the south side of their houses.¹

The fever of Gallipolis, in 1796, to which I shall have occasion to advert in a subsequent chapter, was due to an efficient cause transmitted by the wind. The inhabitants of the village, principally French, and very poor, as well as filthy in their mode of living, began to suffer first, and continued to do so, so long as the wind blew towards their habitation, while the garrison, at some little distance, but to leeward of the source of infection, continued healthy. But, on the wind shifting to the reverse point, the soldiers began to sicken.²

A similar influence of the wind was noticed at Sierra Leone in 1823, as inferred from a review of the cases of fever which occurred in the shipping in the harbour and from the situation of the houses in Freetown in which the disease prevailed. "The ships suffering," says M. Boyle, "were invariably exposed to the N. E. winds; and in the town, the fever limited its course, and in the most striking manner, to those houses between which and the N. E. winds, passing over the Bullom country, there was no obstruction" (p. 268).

In the city of the Havana, the sickly winds, east and west, blow along the coast and drive the exhalations into the harbour. The same thing takes place at Cape Haytien (St. Domingo), from the land breeze which passes over a sickly soil before reaching the city.³ In Vera Cruz, the S. E. wind blows directly over the swampy district around the port.⁴ In 1838, the epidemic of Martinique broke out in localities most exposed to the action of southerly winds. The south winds in that island, as we learn from Dr. Lefort,⁵ blow the effluvia from the port, and the parts of the coast where sources of infection abound, over the town of St. Pierre. In Barcelona, during the epidemic of 1821, it was found that while the wind blew from the S. W., which was the case in the early part of the season, the disease extended from the port to the village of Barcelonetta; but that when, later in the season, the course was generally from the south, and especially from the east, the fever soon spread to every part of the city which lay in that direction.⁶

Ships approaching to, or at anchor near, the shore, sometimes communicate the disease to individuals in the vicinity, if the wind passes over them before reaching the latter. In 1848, the yellow fever prevailed on board of several infected vessels at the New York quarantine ground. Dr. Smith, of Staten Island, informs us that the disease soon broke out on shore. "Most of the cases occurring from its commencement on the 19th of August, up to the middle of September, were probably communicated through the medium of

¹ Bayley, Letters from the Board of Health submitted to the Common Council of the city of New York, p. 8.

² Potter on Contagion, p. 15.

³ Bally, p. 361; Gilbert, p. 12.

⁴ Humboldt, p. 765.

⁵ De la Saignée, &c., p. 66.

⁶ Rochoux, p. 113.

the air. Easterly winds had prevailed for many days together, varying from N. E. to S. E., and blowing constantly from the ships to the shore; and only those living immediately on the shore suffered, and where a building of any kind intervened, so as to break the horizontal current of wind, those so protected escaped. Moreover, from the point where the vessels were anchored, taking the range of the prevailing winds, N. E. and S. E., it would just include the infected district.”¹

On the other hand, vessels take the disease by approaching near an infected port or place, and remaining within range of the wind blowing off the latter. The occurrence at Sierra Leone has already been referred to. In other parts of the African coast, on that of the West Indies, as well, indeed, as in this country and Europe, vessels remain healthy, so long as they keep at a distance from land. But woe to them, if during a sickly season, they approach the latter; for the moment they do so, and place themselves under the influence of the land air, they become exposed to the infection.

Dr. Cummins refers to an instance “where the men were not allowed to hold communication with shore in an infected island, lest they should visit the low, unhealthy houses where the disease might be localized, while, at the same time, the officers could go and come at pleasure; still the disease appeared on board the ship, and not amongst the officers, but amongst the crew.”²

At Georgetown (Demerara), in 1848, vessels lying alongside of the wharf were safer than those far off. In 12 vessels there were 28 cases (8 gravior cases), with a rate of mortality of 11.11.

	Vessels.	Cases.	Gravior cases.	Rate of mortality.
75 fathoms	16	79	37	16.66
100 “	14	82	37	10.14
125 “	3	29	15	60.00

The safest was 100 fathoms.³

On the other hand, vessels that are infected in such localities lose the fever (if they themselves do not contain sources of infection) by shifting their position, and anchoring at a distance—sometimes at a very short one—from the shore; or by going to sea, and thereby placing themselves beyond the influence of the land air. The morbid agency in Georgetown (Demerara), seemed to move in shifting swarms or vortices, hovering over a vessel here and there. Dr. Blair, from whom we derive the information, states, that in the beginning of 1839, the *Thomas King* lay in the division A between Kingston and Wighart’s Stellings. In one week, she lost four hands. She unmoored, and took her station *outside*, or to the leeward of the *Louisa Baillie*; the mortality ceased, and the health of the crew became re-established. The *Louisa Baillie*, that had been right abreast and sheltered by the *Thomas King* before the unmooring, and had no death, although several cases of fever, which readily yielded to treatment, became soon very sickly; she lost four men,

¹ New York State Documents, Jan. 30, 1849, p. 44.

² *Lancet*, July, 1853, Amer. ed., p. 50.

³ Blair, p. 35.

after which she shifted her moorings, and the mortality then ceased in her also (p. 36).¹

But, in all these cases, the winds themselves are not the efficient cause. By themselves they can do nothing. When unconnected with other and more necessary agencies, or under circumstances of temperature and hygrometrical conditions of air necessary for the evolution of such agencies, the fever does not prevail, while the latter, in the same localities may take place during the predominance of other winds, when these circumstances combine. In Barcelona, the same winds blew in 1820 and 1823 as in 1821, and yet the city remained free from the disease in the former two years.² In Gibraltar, in 1828, no unusual prevalence of east wind was noticed.³ In Sierra Leone, the wind blew in the same direction and presented the same character during healthy as during sickly seasons.⁴

It may be remarked, in addition to what precedes, that while the yellow fever does not always manifest itself during the prevalence of winds under the influence of which it seems at times to arise, and that in other instances it appears during the predominance of different and even opposite currents; experience shows, that free ventilation and strong unimpeded currents of wind, are inimical to the generation of the fever cause; that where the wind blows freely and strongly, or finds no obstacles from surrounding objects, and where, in consequence, the air is quickly renewed, localities which otherwise might be expected to be fruitful sources of fever may be visited or inhabited with impunity, and that places of the kind very soon become insalubrious, if the air is stagnant. "Calms," says Dr. Drake, "permit the exhalations from foul localities to accumulate in the atmosphere which rests over them; but all winds operate to disperse and dilute them with purer air; in doing which they may promote the salubrity of one spot and diminish that of another" (*Op. cit.*, i. 587). This injurious effect of a stagnant state of the atmosphere was noticed as early as the time of Hippocrates, who alludes to it in the third book of his epidemics, and has been dwelt upon, on just grounds, by many writers on the fevers of the West Indies, of South America, this country, and Europe.⁵

Many of the bays and inlets in the West Indies, while they derive their security, also derive their unwholesome air through the agency of the hills

¹ See, on this subject, Lind on Hot Climates, p. 200; *Ib.* on Seamen, p. 85; Ferguson's *Recol.*, p. 143; Trotter, *Med. Naut.*, i. 358; Hunter, p. 16; Kéraudren, p. 18; H. McLean, p. 26; Rufz, *Rept.* by Chervin, p. 60; Blane on Seamen, p. 178; Osgood, *Cautions to Seamen*, &c., p. 7; Burnett, p. 313.

² Pariset, vii. p. 176.

³ Gillkrest, ii. 279.

⁴ Report on Sickness, &c., in Africa, p. 26.

⁵ Le Blond, pp. 7, 16, 19, 106; Clark on Long Voyages, i. 5; Imray, *Edinb. Journ.*, liii. 92; Lempriere, i. 17; Rufz, pp. 31, 32; *Rep. on Sickness, &c.*, of Br. A., p. 102; Merrill, *Med. and Phys. Journ.*, ix. 233; Celle, *Hyg. des Pays Chauds*, p. 20; New York *Med. Repos.*, ii. 403; Vincent, *Fièvre Jaune*, p. 7; Ralph, *Edinb. Med.-Chir. Trans.*, ii. 55, 60; Copland, *End. Influences*, *Dict.*, i. 759, Am. ed.; J. Clark on Yellow Fever, pp. 49, 56, 57; Deveze, p. 134; Caillot, p. 108; Caldwell, *Med. Repos.*, vii. 144, 145; McCabe, *Edinb. Journ.*, xv. 33; Channing, *Med. Repos.*, ii. 403.

surrounding them, which gives them an atmosphere little agitated by winds, and in which, consequently, the morbid exhalations from sources of infection situate along their banks, may well be supposed to continue suspended, accumulate, are concentrated by heat, and become infinitely more pernicious. Dr. Chisholm, among others, while calling attention to this fact, and to the correctness of the explanation, adduces several cases in illustration. The French have, therefore, not inappropriately denominated these situations “*lieux étouffés*.”¹ After mentioning that the state of the atmosphere during September, and the first two weeks of October, favoured the accumulation of the miasmata in this city during the memorable epidemic of 1793, Dr. Rush remarks, that the register of the weather shows how little the air was agitated by winds during the above time. “In vain,” he says, “were the changes in the moon expected to alter the state of the air. The light of the morning mocked the hopes that were raised by a cloudy sky in the evening.” He very truly observes that, “however inoffensive uniform heat, when agitated by gentle breezes may be, there is no record of a dry, warm, and stagnating air having existed for a length of time without producing disease.”² What most distinguished the month of July, 1853, at New Orleans, as Dr. Barton remarks, was the unusual number of calms—amounting to twenty-six during the month—“showing, nearly one-fourth of the month, the atmosphere to be in a stagnant condition, hot, saturated, filthy.” In August, the number of calm days was without a parallel in that city, amounting to seventeen. Sixty-eight observations gave evidence of a close, suffocating, inelastic atmosphere. In a word, “what has eminently distinguished the season has been the unusual occurrence of *calms*, or stagnant state of the atmosphere for the whole year; it has been about four times as many as usual, and for August more than eight times as many calms as the average of the five years.”³

Indeed, all regular and fresh currents of air have the effect of sustaining the healthiness of localities, which, in their absence, would be more or less the seat of the fever. In this country, cases in point might be gathered without the least difficulty as regards common periodic fever; and every physician among us knows full well—for the fact is proverbial—that the yellow fever usually breaks out and is most rife in places noted for want of due ventilation, while it requires but a slight acquaintance with medical literature to know, that the same observation has been made in respect to this disease everywhere. A similar remark is applicable to the oriental plague. In all places, they prevail and are most rife and malignant in narrow, close streets and alleys, and the poisons to which they are due, if formed in open and well-aired situations, are soon dissipated and rendered inert.

Under those circumstances, whether in hot climates or in our latitudes, during the tropical heats of summer, the air becomes close, oppressive, and hot to a degree beyond what might be expected from the height of the mer-

¹ Manual of the Climate, &c., of Tropical Countries, &c., p. 20.

² Works, iii. 86, 87.

³ Barton, in Rept. of Sanit. Com., pp. 236, 242.

cury in the thermometer. This condition of atmosphere occasions in many a wakefulness, and in others, of particular constitutions, a variety of disagreeable nervous sensations; and from this it may be readily conceived to render the system impressionable to the action of the efficient cause, while at the same time it tends greatly to promote the evolution of the latter, and concentrate and aggravate its effects.

Sensible of the injurious effects of the stagnant state of the air referred to, many have attributed the unhealthy character of the West Indies during the sickly season—from July to October—to a want of free ventilation arising from the total cessation, or the great irregularity, of the trade-winds which take place at this period of the year. In opposition to this hypothesis, it has been remarked that, though these two events, the failure of the trade-winds and the increase of sickness and mortality, take place at corresponding periods, the latter can never be regarded as a necessary consequence of the former, when we find that in other quarters of the globe, beyond the range of the trade-winds—that is, countries north of the 30th, and 32d degrees of north latitude, and in which ventilation is quite as perfect at that period as at any other—the unhealthy nature of these months is marked as strongly as in the West Indies.¹

So far as the yellow fever of temperate regions, and of this country and city in particular, is concerned, the above argument—which has reference to febrile complaints generally—will apply; for, however frequent may be the stagnation of air in question, and however injurious its effects, it cannot be held as essential to the production of the disease, seeing that the latter often breaks out and prevails extensively under circumstances of an opposite character, and where the ventilation is no less free and perfect than in healthy seasons, while at other times this stagnation, attended with its usual effects on the system, presents itself frequently during seasons noted for their salubrious character, and unmarked by a single case of the fever.

CHAPTER X.

EFFICIENT AND IMMEDIATE CAUSE.

FROM what precedes—from the facts and arguments adduced—it will be perceived that neither of the causes enumerated—and the true agency of which I have endeavoured to point out—can be considered as capable, by itself, of giving rise to sporadic cases, and much less to an epidemic manifestation of that form of disease which constitutes the subject of our inquiries. They are to be viewed, for the most part, in the light of predisposing and exciting causes, and though some of them, as heat and humidity—and, per-

¹ Report of Sickness, &c., of the British Army, p. 102.

haps, to a certain extent, electricity, light, and certain winds—may very deservedly be regarded as contributing, under particular circumstances, to the development and diffusion of the efficient cause, we are justified in the conclusion that, whether singly or combined, they cannot be admitted to possess the power in question. Such being the case, such the restricted sphere of agency of those morbid influences, we must look to some other more efficient cause to account for the development and propagation of a disease which, though somewhat modified as to character and virulence through the influence of external contingencies or individual peculiarities, is in fact always fundamentally the same in kind, while differing in many particulars from the common fevers of the season, and from diseases ordinarily or occasionally ascribed to the action of one or more of the causes we have enumerated.

In this city, as elsewhere, much diversity of opinion has existed, and, to a certain extent, continues to exist, relative to the nature of the efficient cause of the yellow fever—to the sources whence that cause is derived—as well as to the mode of diffusion of the disease after it has once made its appearance. By one set of physicians, backed by the larger portion of the public, the disease is regarded as the offspring of a morbid poison formed and elaborated in a diseased body, and is invariably referred to importation from abroad, more especially from the West Indies, or some other parts of tropical regions. By another class of physicians, as well as by many unconnected with the profession, the fever has been as invariably referred to the evolution, under particular circumstances, both terrestrial and meteorological, of a febrile poison originating in molecular changes inherent to the spot where the disease appears, or in materials accumulated on or near it, and independent of, and having very limited or no connection with the arrival of vessels from sickly ports, whether of foreign countries or American States. By another set, again, the fever, though viewed as of domestic origin, is not regarded as the offspring of a morbid poison, but as the result of some of the agencies to which I have already called attention.

The doctrine of importation carries along with it, in the minds of most medical inquirers, the idea of the existence in the disease of a power of transmissibility from one individual to another through means of a principle of contagion—a specific virus—emanating from the bodies of the sick, and communicated either by contact, or through the medium of the surrounding air, or by means of objects impregnated with it. Hence, by the advocates of this doctrine, the yellow fever is supposed to possess the power in question either inherently—both in the place where it originates, and to which it is carried—or to acquire it by some inexplicable process after it is formed, during the progress of its transportation, or at some other time, while its diffusion, whether on a small or a large scale, is ascribed solely to the exposure of the well to the effluvia arising from the sick.

By those, on the contrary, who believe in the domestic origin of the disease, the above-mentioned power of its transmission is for the most part denied, and its diffusion ascribed to the continued operation of the cause to which its original development is due. To this it must be added, however, that by some writers it is maintained, that though the fever arises from the operation of

domestic causes, yet it manifests contagious properties either in every case and under all circumstances, or only occasionally and under the operation of peculiar contingencies. And as a counterpart to this, some—though few in number—while disbelieving the doctrine of contagion, adhering to that of domestic origin, and discarding the idea of the importation of the fever by means of individuals labouring under or convalescing from it, are more or less decided in the opinion that it may be, and has been introduced from abroad through the medium of the vitiated atmosphere of a ship's hold, or of the seeds of the disease in some more tangible form, and has spread epidemically in consequence of these meeting with an appropriate condition in the localities and atmosphere of the place where they are introduced, and multiplying themselves on the principle of a ferment. In the following pages I shall examine the grounds upon which these various views are founded, and endeavour to arrive at a correct decision on the question before us.

Deferring to a future chapter what may be said relative to the theory of the transmissibility of the disease without the aid of a contagious poison, it may be remarked, that from what precedes we may infer that the question as to the origin and propagation of the yellow fever here and elsewhere, resolves itself, in great measure, into that of the contagious or non-contagious nature of the fever; for, if the latter is imported, *per se*, through the instrumentality of individuals labouring under or convalescing from it, it must be endowed with contagious properties. If it be so endowed, it is not likely to arise from domestic causes, diseases of this kind being seldom if ever contagious, and must therefore be ascribed to importation from places where it more usually prevails. In that case, all epidemic scourges, and even sporadic cases, may naturally be referred to a foreign source; and we can find no difficulty in ascribing the diffusion of the fever, when so introduced, to the communication of the well with the sick, or with substances impregnated with the poison. If, on the other hand, the fever is not endowed with the power of transmissibility alluded to, it can scarcely be presumed to have been imported in the way contended for by the contagionists. It must hence be referred to the operation of domestic causes, unless it can be shown that this, or any other fever, can either lose or acquire that power by a change of locality and the agency of external circumstances, and its propagation must be admitted to take place independently of any influence on the part of the sick.

The point at issue being thus narrowed to the question of the contagious or non-contagious character of the fever, we may proceed to the examination of that question with a greater chance of avoiding repetitions and confusion, and without the necessity of examining separately the statements afloat respecting the introduction of the disease from abroad—those statements finding an appropriate place among the facts by which the doctrine of contagion may be supported, while their confutation—if confuted they can be—will be used as evidence of the fallaciousness of that doctrine. After this portion of my task is accomplished, I shall pass to the examination of the domestic causes to which the disease may be, and has been ascribed—of the arguments

and facts adduced in favour of the doctrine of local origin—of the sources whence those causes originate—of their nature—of the circumstances which favour their development and diffusion, and modify their energy, as well as of the laws by which they are regulated.

It may be well, at the threshold of our present inquiries, to say a few words respecting the terms I have adopted to denote the two opposite explanations which the origin and propagation of the fever have received, agreeable to one of which the disease is ascribed to the deleterious agency exercised by a person already affected on individuals in health; and, according to the other, to the operation of morbid agencies of a local or domestic character, and independent of any poison exhaling from the sick. From an early period, diseases of the first class have been denominated contagious, and the power through the instrumentality of which the communication is effected has been called contagion. In our present investigation, there can be no reason for a resort to other terms. Even were the latter in any way objectionable, the substitution of others would be still more so, from the risk of the confusion that might thereby result. But as regards the denomination applicable to the second source to which the disease has been ascribed, the selection of an appropriate term is not devoid of difficulty. In accordance with several writers, I have resorted to the word infection for that purpose, and restricted it exclusively to the sense in question. Agreeably to this arrangement, therefore, I shall apply the word contagion to signify a poison, effluvium, or emanation, generated by morbid secretion in the course of a distemper, and possessing the power of inducing a like morbid action in healthy bodies, whereby it is reproduced and indefinitely multiplied, whether by contact, near approach, or the medium of external bodies impregnated with it.¹ All diseases that are so communicated are contagious. By infection, on the other hand, the reader will understand that power or poison which results from the decomposition of dead animal and vegetable substances, or other putrescent materials, if such exist, and through means of which a morbid state is induced in the system of individuals exposed to its action. In conformity with this restriction in the signification of the word infection, individuals labouring under the effects of exposure to the morbid cause in question are said to be *infected*; while the diseases resulting from that cause are denominated diseases by infection, or infectious. The agent of infection usually exists in the state of gas or miasm, and, as such, occurs in filthy localities—houses, ships, jails, hospitals and cities—as well as in marshes and fenny and low districts of country.²

The distinction here established between diseases by *contagion* and those by *infection*, and the opposite significations attached to those terms, are not offered as devoid of all objections, and as fully justified by the real etymology of the words; but as the words, when used in the above sense, appear to convey more precise views of the character or origin of the disease to which

¹ Smith on Epidemics, p. 39; Hancock on Pestilence, pp. 23, 24.

² Smith, p. 43.

they are applied than when employed in a less restricted sense—as by so using them we avoid the necessity of repetition, explanation, and periphrasis, and, as a natural consequence, much ambiguity—I have not hesitated to follow that course. The distinction, though not of so recent a date as some writers—and even the author of a creditable work on epidemics—seem to imagine, is nevertheless of comparatively modern date. “During the period that fevers were generally believed to be personally communicable,” Dr. Smith remarks, “the terms contagion and infection were considered as synonymous, but were so ambiguously employed that, in referring to the older works, it is frequently difficult to determine whether they were intended to designate specific poisons generated by morbid secretory action, human effluvia, or the exhalations from putrefying substances.”¹ Nor is it alone in works of older times that the want of precision as to the signification of the terms in question, and their synonymic employment, may be discovered. There is scarcely a writer of our own day, particularly on the other side of the Atlantic, who does not use them in this loose and unsatisfactory manner. Let the reader turn to the writings of those who have attempted to define the terms, or who have used them without making such attempts—let him open the works of Charles Maclean, Granville, John McLeod, D. Gladstone, Faulkner, Quiney, Ferguson, Fellowes, and Hancock, and he will have the proof of the justness of the remark.

The one, for example, tells us that infection includes contagion, and differs from it only in being more comprehensive. Another says: “Infection is a peculiar state of the atmosphere, rendered unfit for the healthy exercise of life by the crowding together of a number of persons, ill of the *same fever*, in a given place and during a given time; thus an epidemic may become infectious.” A third remarks that infection is the act of transferring diseases from one body to another, in whatever way it can be done; contagion is *receiving* the disease by touch alone. Another, again, informs us that infection is disease produced by a contagious state of the atmosphere. A fifth says that “contagion expresses only a *mode* of infection;” the former differing from the latter only in being a term of lesser extension. We are again told that the word contagion should express the morbid poison, or the means of transferring a disease, and that infection expresses the operation of the poison, or the act of the communication of the disease; an explanation not different from that of Dr. Hancock, who, after giving a good definition of the word contagion, and dwelling on the etymology of infection, says that the latter must always have reference to the human body, and that while it always implies contagion, the latter does not always imply the former: “for infection may take place some time before contagion is formed; and contagion may be formed, if we give credit to some of our best writers, without any previous infection” (p. 25). A writer of our own country sees nothing particularly mysterious in the meaning of the words contagion and infection. He gives the usual definition of the former, and says that “the word infection

¹ Smith on Epidemics, p. 27.

refers, on the contrary, to morbid exhalations, derived most usually from organic substances in a state of decomposition." Yet he uses infection as synonymous with contagion, and employs the word *infected* to mean "that condition of the atmosphere when it is charged with the matter of contagion."¹

The first, so far as I can ascertain, who endeavoured to put an end to the indiscriminate use of the terms, and to annex to each a more distinct meaning, is an author who occupies a conspicuous place in the annals of French medicine—Quesnay. In an essay inserted in the first volume of the *Memoirs* of the old Academy of Surgery,² he indicates the theories of contagion and infection, as now taught, in a more comprehensive and accurate manner than had been done by any other writer prior to his time. After defining contagion to be a property by which the vicious state of a body can extend itself to other bodies susceptible of the same state, and admitting that the word infection is often used as synonymous with the other, he says: "By infection is understood simply the impurity occasioned in the mass of humours when some deleterious substance, as, for example, an air impregnated with putrid vapours, penetrates into the vessels and mixes and circulates with the humours. The word infection is then used in conformity with its true signification. It is principally with this sort of infection we have to do at present; for the putrid infection may be caused not only by substances that may be furnished by putrid humours of a diseased body, but likewise by substances furnished by the decomposition of a dead body, which dead body may be animal or vegetable. Now, in the latter case, there is no communication of disease from one body to another. This infection is, therefore, very different from that kind of contagion of which we have just spoken" (pp. 30, 31).

By a writer of our own country, the late Dr. Bayley, of New York, the same distinctive view of the terms in question was upheld much in the way that had been done by Quesnay, who, it may have been seen, admitted the application of the word infection to express occasionally the power of transmission of a disease from a person affected to one in health. In his account of the yellow fever which prevailed in New York in 1795, Dr. Bayley, while using the term contagion to denote such diseases as are communicated from one individual to another, under any circumstances of atmosphere, whether pure or impure, limited infection to denote those which are communicated in consequence of an impure or vitiated state of the atmosphere, the impurities being the cause of the disease, and not any specific material derived from a patient, and floating in the air, except such as may be occasioned by the want of cleanliness.

In a word, he limited the term infection to the pestiferous effluvia arising from the excretions of the sick and other species of filth.³ The same views were adopted by Webster,⁴ and other writers. They are no doubt to be considered rather as an approach to the truth than as the whole truth; for if the impurities of the atmosphere surrounding a patient acquire the power of

¹ Townsend, *Fev. of N. York* in 1822, Preface, p. ix.

² *Mém. sur les Vices des Humeurs*, in *Mém. de l'Acad. de Chirurgie*, i. 26, 8vo. edition.

³ *An Account of the Epidemic Fever which prevailed in the City of New York, &c.*, in 1795, p. 38 (1796).

⁴ *A Brief History of Epidemic and Pestilential Diseases, &c.*, ii. 141.

communicating to others a disease precisely like that under which that patient labours, it may be said to derive that power from something peculiar arising from the disease—the latter has then become contagious; and if infection is employed to convey the idea of this power in question, it follows that it is, in great measure, employed synonymously with contagion. The doctrine was hence imperfect, so far as precision is concerned, and, in some respects, less satisfactory than that proposed by Quesnay; for though the latter used infection occasionally in the same sense as contagion, he considers such a use as improper, and adverts not to the morbid effects of human effluvia alone, as is done by Dr. Bayley, but likewise to the infection arising from the decomposition of animal and vegetable substances (p. 32).

But although the views of Dr. Bayley were not as precise and definite as we could have desired, still, he may be said to have led the way more effectually than had been done before, in this country particularly, to the adoption of the proper distinction between diseases by contagion and those by infection; for only three years after the appearance of his clever volume, *i. e.* in 1799, we find the editors of the *New York Medical Repository* advocating it on the same basis as that I have adopted. "In popular use," they say, "no distinction is made between the terms *infection* and *contagion*, nor perhaps would an etymological examination of them justify a distinction. It will be convenient enough for our present purpose, however, to assume a distinction. The reader, therefore, will, for the present, be supposed to understand by the first, *infection*, some noxious gaseous substance, capable of exciting fever, and not emanating in that form and with that power from a diseased body; and by the second, *contagion*, a poisonous material, capable of exciting a peculiar disease, and emanating with that power from a body sick with that peculiar disease. That this is a distinction in fact, none probably will deny."¹

This distinction, approved soon after by respectable writers, has been very generally adopted in this country. Nor have the advantages resulting from it been overlooked by European physicians. Dr. Adams adopted it in his work on morbid poisons; limiting, however, the term *infection* to an atmosphere vitiated by human effluvia.² Dr. Deveze, in the last edition of his treatise on the yellow fever,³ draws the line of demarcation between contagion and infection, and exhibits, with great perspicuity, the characteristics of each, using, however, the latter term to designate only the exhalations from the soil; while Armstrong, Boote (i. 117), Bonneau and Sulpici (pp. 68, 81–85), Nacquart (*Dict. des Sci. Méd.*, art. "Contagion"), Rochoux (p. 50, and *Dict. de Méd.*, xii. 204–5), Dupuytren (*Rep. to the Acad. of Sci. on Costa's Mem.*, pp. 7, 8), Costa (pp. 41, 42), Chervin (*Fev. of Spain*, p. 87), Valentin (p. 153), Palloni (p. 71), Dr. Fermon (p. 11), Lefort (*Sur la Contag.*, p. 23), Dariste (p. 42), Shecut (p. 146), Vaughan (p. 25), Perkins (pp. 63, 64), and Chambolle (*Ann. de la Méd. Phys.*, xiii. 199), have adopted the same views with more or less precision. In conclusion, I remark that, discarding the

¹ *Reposit.*, ii. 84, v. 186.

² *Observations on Morbid Poisons, Chronic and Acute, &c.*, p. 6. Lond. 1807.

³ *Traité de la Fièvre Jaune*, p. 120 (1822).

exclusive meaning of the term infection, applied to it either by Dr. Adams or Dr. Deveze, I shall, in imitation of Dr. Smith (p. 33), combine the views of those writers, and consider infection as embracing all the febrile agents which originate from the decomposition of dead animal and vegetable matters, and endeavour, at the proper time, to ascertain how far the yellow fever can with propriety be ascribed to these morbid sources, singly or combined.

The opinion of the contagious character and exotic origin of the yellow fever was early entertained in this and other countries, and continues to this day to find favour in the minds of professional men, while it may be viewed as the popular creed of the public at large. In the origin of the epidemical visitations of this city, the disease was almost universally ascribed to importation from abroad, and its subsequent propagation in the way mentioned was thought to admit of no doubt. In reference to the epidemic of 1699, James Pemberton says, in a letter to Dr. Wistar, that his father had often told him that the fever was imported in a ship (or other sea vessel) from the island of Barbadoes. On the same authority he states that the fever, being so introduced, spread through the city, attended with great mortality. He states that his father was well capable of making his remarks on occurrences, and afterwards of remembering them, and knew it to be the invariable judgment of the physicians and other citizens that the disease was introduced among them in the manner related above, attributing it to no other cause.¹ Equally convinced were the physicians and public of the foreign origin and contagious nature of the fever of 1740, 1741, 1744, and 1747.² On this subject, Pemberton says: "Having had the opportunity of knowing the state of our city in respect to the prevailing diseases from the year 1740, I am confirmed in my opinion that the malignant and infectious fevers afflicting the inhabitants at various times have been introduced from foreign ports, and that this was the invariable judgment of my fellow-citizens, physicians and others, which I never heard called in question until since the year 1793" (p. 7). Benj. Chew also speaks of the alarm spread among the inhabitants of Philadelphia in 1747, in consequence of "a contagious disease which raged there," and which was universally ascribed to importation from the West Indies. He in like manner perfectly remembered that Drs. Thomas and Phineas Bond, Dr. Graham, and Dr. Cadwalader, with whom he was particularly acquainted, were unanimous as to the exotic origin of the disease. We have already seen that Dr. Bond attributed the disease, in most cases at least, to the same cause, both as regards its introduction and diffusion. As regards the fever of 1762, Dr. Redman and Mr. Willing do not hesitate to ascribe it, as a matter of course, to importation from the West Indies; and while the former states that from the spot where it was introduced it spread rapidly, first affecting the houses nearest the focus of contagion (p. 21), the other affirms that the individuals who brought it died of a contagious fever, and that sixty others soon experienced the same fate in the vicinity of the contaminated spot (p. 10).

¹ Add. Facts by the College of Physicians, p. 5.

² See Letters of Pemberton, Th. Willing, and B. Chew, Add. Facts, pp. 6-11.

In 1793, when, as we have seen, the fever next broke out in an epidemic form, the opinion of the medical profession was very generally inclined to the idea of importation from abroad, and, as regards the contagious character of the disease, the number of dissenting voices was still more restricted. To refer the disease to other than a foreign source—to say that it arose from the operation of domestic causes, was considered as an unpardonable offence, a slander on our city and climate; and those who were bold enough to doubt the contagious character of the fever were looked upon as little better than fools. Every eye was turned towards the West Indies as the place whence all the misfortune had originated; and, from what has been stated in a former chapter, the reader will readily perceive the extent to which the belief in contagion was carried, and the dismay it produced. The College of Physicians, which embraced the larger number of the practitioners of the city, were almost unanimous as regards the first point, and probably quite so in reference to the mode of propagation of the fever. “No instance has ever occurred,” they state to the governor, under date of 26th November, 1793, “of the disease called the yellow fever being generated in this city, or in any other part of the State, so far as we know; but there have been instances of its having been imported, not only in this, but in other parts of North America, and prevailing there for a certain period of time; and from the rise, progress, and nature of the malignant fever which began to prevail here about the beginning of last August, and extended itself gradually over a great part of the city, we are of opinion that this disease was imported into Philadelphia by some of the vessels which arrived in the port after the middle of July. This opinion we are further confirmed in by the various accounts we have received from the best authorities we could procure on the subject.”¹

The College remained steadfast in their belief on the subject throughout many of the subsequent epidemics. In 1798, they were of opinion that to endeavour to prove the contagious nature of this disease would “be equally as useless as to prove the contagion of plague;” and then, as well as at other periods, gave evidence of their profound conviction on the subject, by urging on the legislature, the State executive, and the city and health authorities, the necessity of adopting police and sanitary regulations, most of which could only be calculated to avert, arrest, or moderate the extension of a disease endowed with contagious properties of the most decided character. What these measures were, and what their effects on the public, have already been stated, and need not be reverted to in this place. While such were the views, as regards contagion and importation, expressed by the College—then probably the highest medical tribunal in the land—they were advocated in the most strenuous manner by individual writers, professional or otherwise, who made every effort imaginable to demonstrate the exotic origin of the disease, and to exhibit its contagious character. The writings of Currie, Cathrall, Carey, Barnwell, Nassy, Condie and Folwell, Parish, Samuel P. Griffiths, and Wistar, to say nothing of newspaper articles, on

¹ Currie, p. 83.

those and subsequent epidemics, are lasting monuments of the zeal and activity of that party, and, together with the public actions of the College, constitute no unimportant item in the medical literature of the period on this side of the Atlantic. Nor should it be forgotten that by some of those who disbelieved the importation of the fever from abroad, as Dr. Rush, Dr. Redman, Dr. Foulke, Dr. Hutcheson, Dr. Leib, and I may add, Dr. Caldwell—who, though not having as yet reached the doctorate, took a somewhat prominent part in the discussion—the doctrine of contagion was originally considered as placed beyond the possibility of doubt, and that the writings of the first, whose professional reputation was second to none in the country, tended greatly to foster the adoption of the views in question. It is but justice to our physicians to state, before proceeding further, that though in support of importation and contagion many facts were adduced by them which, to philosophical minds of the present day, will appear as savouring of romance, and cannot bear the test of serious examination, we discover nothing in their writings, animated though they doubtless were by party zeal, and excited by the troubles of the times, of the extravagant and ridiculous kind with which some European and American contagionists of our day may be taxed.

While such was the state of opinion on the subject in this city, a similar view respecting the origin and mode of propagation of the disease was adopted in other parts of the United States and elsewhere. The early epidemics of Charleston were ascribed to importation from West Indian ports, and the spread of the disease was referred to the exposure of the well to the exhalations issuing from the bodies of the sick. Lining, for example, informs us that within twenty-five years of the time at which he wrote (1753), the disease had been four times epidemical in that town—in the autumns of 1732, '39, '45, and '48; that, whenever it appeared, "it was easily traced to some person who had lately arrived from some of the West Indian Islands, where it was epidemical;" and in proof of its contagious character, states that "almost all the nurses caught it, and died of it, and that it soon invaded new comers, those who never had the disease before, and country people when they came to town, while those who remained in the country escaped it" (ii. pp. 407, 408). As we have seen, in one of the opening chapters, the fever which prevailed in Boston in 1692 was ascribed to importation from the West Indies, and regarded as contagious. The same sentiment prevailed in New York in 1702, 1742, and at subsequent periods, 1791, 1795, 1798, &c. The fever of other places in this country, North, South, East, and West, has been ascribed to foreign sources, and maintained to be endowed with a contagious property; while the annals of the epidemics of yellow fever, which have occurred in Europe and in the tropical countries, tell the same tale. But, on this subject it is not necessary to enlarge in this place, as I shall have occasion to recur to it as we proceed.

That the doctrine of importation and contagion, whatever may have been its merits, should have found such support here and elsewhere, cannot be a matter of surprise. In the minds of the public at large, who seldom, if ever,

possess the requisite qualifications, or devote the time necessary for investigations of the sort, it is most natural to attribute wide-spreading diseases to an exotic origin, and their propagation to the effect of exposure, with or without contact, of the well to the sick, or to fomites. Discovering nothing palpable in an infected spot to which they can refer the development of these—observing no difference in external agencies as regards meteorological phenomena, and the condition of localities between healthy and unhealthy seasons—seeing the disease start up almost suddenly and spread rapidly—knowing, at the same time, that it prevails elsewhere, and also that vessels or individuals have arrived from places so visited, they cannot avoid the conclusion that when it shows itself among them, it is necessarily the offspring of importation from contaminated localities. Nor can they perceive how a disease which spreads widely and quickly, which attacks persons who have had communication with the sick, and is, withal, as they think, imported, can do so otherwise than through the influence of personal contagion. Dr. Rush says: “The idea is produced by a single act of the mind; it requires neither comparison nor reasoning to adopt it, and therefore accords with the natural indolence of man. It moreover flatters his avarice and pride by throwing the origin of a mortal disease from his property and country. The principle of thus referring the origin of the evils of life from ourselves to others, is universal. It began in Paradise, and has ever since been an essential feature in the character of our species.”¹ Nor is the doctrine less acceptable to physicians themselves, particularly at the outset of epidemical visitations, when the facts have not been sufficiently sifted, and minds are engrossed with the existing calamity. Some, doubtless, adopt those views after mature examination and careful study; but, in many, if not most instances, they are adhered to because they save a vast deal of trouble, or appear to explain at once and in the simplest manner all the circumstances that occur. To investigate the causes of disease, to collect and compare discordant and often obscure facts, and to draw correct deductions therefrom, and sift the grain from the chaff, is not so agreeable and easy a matter as to be undertaken and to be effected in a satisfactory manner by practitioners of medicine generally, and is beyond the scope of the public at large. Some of the former are too indolent to undertake it; others are ill prepared, by proper mental training, or too much occupied to devote the necessary time to the investigation, and they all, therefore, rest satisfied with, or cannot reach beyond the views entertained by the unprofessional portion of the community. These views become, at last, favourite objects of general belief, and there happens in regard to them, what Locke long ago said of others of a different nature: “Men espouse the well-endowed opinions in fashion, and then seek arguments either to make good their beauty or varnish their deformity.” To this we must attribute the fact, that all wide-spreading maladies—and, indeed, often diseases of more restricted prevalence, have at times been considered as the product of importation, and as being endowed with contagious proper-

¹ Rush, Yellow Fever not contagious, Med. Inquiries, iv. p. 163.

ties. There are not wanting diseases formerly regarded as contagious in reference to which, from the facts that have accumulated, and more extended and correct observation, the opinion of a large portion of the profession, to say nothing of the public at large, has more or less changed. In all ages, men have exhibited a marked reluctance to ascribe such diseases to causes of a local or domestic nature, or to the existence of some irregular or morbid condition of the air; and sooner than look to these, they have in rapid succession sought modes of explanation, which, as science improved, or the fancy changed, have given way to others, the unsatisfactory nature of which was as easily discovered. Such being the case, it would be astonishing, indeed, had matters taken a different course in regard to yellow fever. So far as this disease is concerned, the disposition in question is not peculiar to American physicians, for its history everywhere shows a great tendency in the minds of both the profession and the public, even in places where it has appeared endemically over and over again, to attribute it to importation from other localities.

Each individual will defend his own place of residence; its character and value are involved in the decision, as well as his own sagacity and penetration in having made such a choice.¹ Hence, we find that medical men—and clever ones, too—though having no pecuniary interest in the matter, exert themselves to show that the fever of one spot has originated in another, when by others, on the other hand, its birthplace is, or might be, with equal propriety, referred elsewhere. This fact was, many years ago, noticed by Humboldt: “In all climates,” he says, “men imagine they find some consolation in the idea that a disease reputed pestilential is of foreign origin. As malignant fevers are easily engendered amid a large crew, heaped up (entassés) in filthy vessels, the commencement of an epidemic dates, pretty often, from the arrival of a squadron. Then, instead of attributing the evil to the vitiated air contained in vessels deprived of ventilation, or to the effect of a hot and unhealthy climate on newly arrived sailors, people affirm that it has been imported from a neighbouring port, where the vessels in the convoy have touched, on the passage from Europe to America. It is thus that we often hear it said in Mexico, that the vessel of war, in which such and such a viceroy has arrived into Vera Cruz, has introduced the yellow fever, which had ceased to reign for some years past; it is thus, that during the hot season, the Havana, Vera Cruz, and the ports of the United States, mutually accuse each other of being the source of the disease by which they are visited.”² This disposition has long existed, and continues to exist to the present day. Father Du Tertre, in his *Histoire Naturelle et Morale des Iles Antilles de l’Amérique* (i. 422), says it was introduced into St. Christopher in 1648, by *some vessels*, and at Guadaloupe by a ship from Rochelle (France). The reverend author, as also Father Raymond Breton,³ Mathias du Puis,⁴

¹ Ferguson, Med. Gaz., 1839, p. 367.

² Essai Politique sur le Roy. de la Nouv. Esp., p. 756, 4to.

³ Dictionnaire Caraïbe, v. 276, &c.

⁴ Relation de l’Etablissement de la Guadaloupe, 8vo. (1652).

Maurile de St. Michel,¹ Péléprat,² Rochefort,³ and Ligon,⁴ thought it contagious. When the disease first appeared at Martinique in or about 1688, it was attributed to importation from Brazil, where it was said to be prevailing and where a vessel, the Oriflamme, touched on its return from Siam,⁵ and taking the disease, brought it to the island. It was considered as highly contagious. The same foreign source and contagious character were ascribed to the epidemic which appeared there in 1703.⁶

When it broke out in Barbadoes in 1691, where it prevailed extensively under the name of Kendal fever, it was thought to have been imported from Pernambuco.⁷ Moreau de St. Méry,⁸ in his voluminous description of St.

¹ Voyage des Isles en Amérique, 1652, p. 45.

² Relation des Missiones des P. P. de la Campagnie de Jesus, &c., p. 14, 35.

³ Hist. Nat. and Mor. des Antilles, p. 4.

⁴ The History of Barbadoes, 1657, p. 21.

⁵ Labat, i. 72.

⁶ Feuillée, Journal d'Observations dans la Nouvelle Espagne, &c., p. 187.

⁷ Hughes, Nat. Hist. of Barbadoes, pp. 37, 38.

⁸ Moreau de St. Méry, mentioned in the text, places the introduction of the fever at Martinique to the year 1690 (ii. 701), and says nothing of the Oriflamme having touched at Brazil. Using St. Méry as his only authority, on the subject of that event, Dr. Strobel, of Charleston (p. 13), censures severely Bally for having mentioned it. Had he, however, consulted Labat, who, having arrived at Martinique in 1694, and having published an account of the events of the colony at that time, must be regarded as better authority than Moreau de St. Méry, he would have found the reverend author very explicit on that matter. Speaking of the fever which prevailed at the time of his arrival, he remarks: "This disease was known by the name of *Mal de Siam*, because it had been brought to Martinique by a king's vessel, the Oriflamme, which, on its return from Siam with the fragments of the establishment that had been made at Merguy and Bancok, touched at Brazil, and took that disease, which had been prevailing there severely for the last seven or eight years" (i. 72).

Sir Gilbert Blane, with his usual inaccuracy about dates and facts, says (ii. 127): "The most remarkable instance of this epidemic is that recorded by Père Labat, of its introduction to Martinique in 1686, by a ship called the Oriflamme, from Siam, whence it got the name of *Maladie de Siam*, &c." Again, he says: "With regard to the Portuguese, it is distinctly described by Ferreiya da Rosa, a physician of Olinda, in Brazil,¹ where it prevailed in 1687, immediately after the conquest of Pernambuco, and broke out on the arrival of the ship Oriflamme, already mentioned, on her passage from Siam to Martinique" (ii. 128). Were this allegation, as to the statement of Ferreiya da Rosa, correct, the reader, bearing in mind the remarks of Labat, and which the good Father made on the information he obtained in Martinique, would learn the extraordinary fact, that while, by some, the Oriflamme is said to have derived the fever from Brazil and not from Siam, where the disease did not prevail, others affirm that the same vessel brought it to Brazil from Siam and spread it among the inhabitants. But by those who have recourse to the original authorities, and do not trust to the statements of Sir Gilbert, it will be found that the facts in question exist only in the imagination of the medical baronet, whose references to Labat and Da Rosa are faulty in more respects than one. To say nothing of the circumstance that the Oriflamme is stated to have introduced the fever in one place a year before it is accused of having introduced it in another while on its

¹ Trattado da Constituição Pestilencial da Pernambuco, per Joan Ferreiya da Rosa, em Lisboa, 1694.

Domingo, published in this city in 1797, very gravely informs us that the fever of Siam was first introduced at Port du Paix (a port of that island), from Martinique in 1691. According to him, it was introduced at the Cape, in 1696, from Léogane, and in 1705 from Martinique (i 534-35). At Léogane (ii. 479) and Santa Cruz in 1691, it came, according to the same authority (i. 701), from the latter place. Dr. Trapham, who published, in 1679, a work on the health of Jamaica, states, that about eight years before, 1671, a victorious fleet returning from the signal expedition to Panama, brought with them a high, if not pestilential fever, of which many died throughout the country (p. 81). Dr. Warren, who, in ignorance of the statements of Hughes (p. 37) and Hillary (p. 144), as to the appearance of the fever at Barbadoes in 1691 and 1696, affirmed it to have first broken out in that island in 1721, says it was then brought from Martinique, in the Lynn man-of-war; as it was likewise in 1733. According to this author, to whom we are indebted for our earliest medical account of the yellow fever, the disease is of Asiatic origin, having been brought to Martinique from Marseilles in bales of cotton taken out of a vessel recently arrived from St. Jean d'Acre (pp. 4, 5). The vomito-prieto was said to have been introduced at Guayaquil from Panama in 1740, by the Spanish galleys of the Southern Sea. On this point,

way to the former, I may remark that the first named writer, Labat, is made to refer the fever of Martinique to 1686, whereas he nowhere mentions that date. Nor, indeed, could he have done so without laying himself open to the charge of uttering a falsehood; for the *Oriflamme* having reached that island in December, 1690, and the reverend writer having merely repeated the reports current in the island at the time of his arrival, respecting the agency of that vessel in the origin of the disease, could not have fixed the date of the epidemic four years before. Again: Sir Gilbert Blane, who, it is evident, never read Labat's work, makes no mention of his having stated that the fever was derived from Brazil. Moreover, the fever of Olinda having broken out in 1687, as stated by Da Rosa, could not, admitting its transmissibility by eontagion, have been introduced there by the *Oriflamme*, inasmuch as this vessel did not reach the place until three years after; and, finally, Da Rosa, who evidently entertained the idea that the disease arose from a pestilential constitution of atmosphere, and not from a foreign source, says nothing of the pretended instrumentality of the *Oriflamme* in introducing it there.

I have stated that Sir Gilbert Blane does not appear to have read the work of Labat. The probability is, he derived his knowledge of it from Chisholm. But Chisholm himself was not any better off in that matter than the learned baronet; for, not having the original within his reach, he contented himself with an extract from the *Histoire Générale des Voyages*, lix. 332. This extract, however, though stated by Chisholm to be perfectly correct, and literally taken from the original, is faulty; for, by it, we would be led to infer that, at the time of Labat's arrival, the disease had been already prevailing at Martinique some seven years; whereas, the author merely states that when the *Oriflamme* touched at Brazil, the fever had been committing ravages there during that length of time. The statement about seven or eight years, refers to Brazil, not to Martinique.

Dr. J. Wilson (p. 105, note) is severe on Father Labat for having credited and contributed to spread the report, that the fever of Martinique had been imported there from Siam—the distance of which from the other place is somewhere in the neighbourhood of 10,000 miles, and where there is no proof of the true yellow fever ever having prevailed. From what has been said above, as well as in the text, it will be perceived that the reverend author did not deserve the censure.

we have the authority of Ulloa,¹ who, however, remarks, that in stating that fact he has only followed public opinion founded on the circumstance that, prior to that period, the disease was there unknown. In times nearer our own, the same disposition to deny a domestic origin to the fever has prevailed. Lining thought that the fever of Charleston was always the offspring of importation from the West Indies (p. 370). It is well known that the famous epidemic of Grenada, in 1793, was ascribed to importation from the island of Bulam.² That of Martinique and St. Domingo, the same year, came, as we are gravely told, from Philadelphia.³ Dr. Halliday appears to be convinced that the fever was imported into the Havana from the same place in 1794 (p. 10).

Drs. Domingues and Oyarvide, who wrote in Havana in 1794 and 1801, entertained opinions allied to that of Halliday, affirming that whenever the disease breaks out in that city it is invariably derived from the same "Anglo-American port."⁴ American vessels communicated it, as we are assured by Chisholm (ii. 116), to the army of Sir Charles Gray, the next year after the conquest of Martinique. Lefoulon assures us that the source of the West Indian fever is to be found on our shores; in other words, that the fever of those islands may always be traced to importation from the United States (pp. 33, 278, 360, 369). Such was said to be the case at Guadaloupe in 1793. The same views are advocated in Spain by Dr. Balmis, who is of opinion that the fever of Philadelphia, in 1793, was communicated to almost all the West India Islands, and particularly to Bayaja (St. Domingo), where it caused an immense mortality among the troops.⁵

Dr. Copland, entertaining views not very dissimilar from those suggested before by Audouard,⁶ says, that since his visit to several places in Africa, and knowing the very limited space in which a large number of slaves are often confined, both on shore and in slave vessels, he entertained the idea that this pestilence, or its seminum, or specific infection, had been generated originally by the congregation of negroes in a close atmosphere, or is generated *de novo* by this race when placed in the circumstances now stated; and that, although it affects them in a comparatively slight manner, it is most particularly baneful to the natives of cold countries.⁷

The French medical commissioners sent to Cadiz in 1800 to investigate the causes of the epidemic then desolating that city, as well as Arejula, and a whole host of Spanish physicians and governmental officials, traced the disease, in a way very satisfactorily to themselves, to the city of Charleston. On the same occasion, Seville, Xeres, Malaga, Carthagen, and many other cities

¹ Voyage Historique de l'Amérique Méridionale, i. 140.

² Chisholm, i. 98.

³ Moreau de Jonnes, p. 83.

⁴ Alfonzo de Maria, p. 4; see Med. Report, vi. 52.

⁵ Ensayo Medico-Pratico sobre el tifus icterodes, &c., p. 175.

⁶ Recueil de Mém. sur le Typhus Nautique, p. 12; Ib., Relation de la Fièvre Jaune, qui a régné au Port du Passage, p. 5; Ib., Lettre sur la Traite des noirs considérée comme la Cause de la Fièvre Jaune, An. Maritimes, 1838, i. 289.

⁷ Dict. of Pract. Med., iii. 175.

of Spain, received the disease from Cadiz, or from one another.¹ Subsequent epidemics occurring in Spain were in like manner referred, one and all, to an exotic source. The fever of Barcelona, in 1821, was referred to importation through means of vessels from the Havana. Pariset, and some of his Spanish medical friends, thought it very probable that the fever of Cadiz, in 1819, came from Calcutta, and was the result of the transformation of that disease from the Asiatic cholera.² None of the Gibraltar epidemics have, according to a number of respectable authorities, originated in the place. All—from that of 1804 to that of 1828—have been derived from some foreign port. The same thing may be said of the epidemic of Leghorn in 1804.

I have cited Lining to show that the fever of Charleston was early attributed to an exotic source. The same view has been taken of all the epidemics that have prevailed in this country. Even in our own days we find a like tendency existing in the minds of the public, and not a few members of the profession here and elsewhere to look anywhere but at home for the source of the evil. Dr. Strobel (p. 130), and a few physicians of Charleston, are of opinion that the strangers' fever of that city is generally, if not always, imported from the West Indies. St. Augustine (Florida), Augusta, Savannah, and other southern cities, receive it from distant parts. Even New Orleans is, in the minds of some of her sons, washed from the stain of giving rise to the yellow fever, which is attributed to intercourse with the Havana, or Africa, or Rio Janeiro, or some American port. Dr. Monette will have us know, that when the disease breaks out at Natchez, or any place along the Mississippi, it is derived from New Orleans, or some other infected spot. So also in Mobile. The time was when the yellow fever arose there from the operation of domestic causes. But, as Moliere said of the learned man who described the heart as being on the right side and the liver on the left: "Nous autres grands médecins, nous avons changé tout cela." The fever now comes from elsewhere. The epidemic of Woodville, in 1844, was by Dr. Kirkpatrick traced to Galveston, Texas.

"In the West Indies, many—indeed I may say all colonists, as Dr. Ferguson remarks—will strive to remove the reproach of pestilence from their shores.³ They will greedily imbibe the grossest delusions—swallow, and propagate the idlest tales of importation, and shut their eyes to the most obvious facts of native origin, for as long as they can make the favourite belief subservient to the character and interest of the colony, consequently to their own."

One island receives it from another. St. Vincent's received it, in 1793, from Guadaloupe (*Chisholm*, ii. 145)—some said from the Hankey, one of whose sailors had landed there while the vessel was on her way from Grenada to England. In 1793, St. Christopher received the disease from Grenada (ii. 299), and, in 1796, from some English regiments returning to England from St. Vincent's (ii. 154). The disease was communicated from Grenada to Tobago in 1793—some think from Barbadoes (ii. 213, 214). From the for-

¹ Berthe, p. 53.

² Pariset, *Observ.*, pp. 56-7.

³ In the *Med.-Chir. Rev.*, xxxii. 299.

mer place it was conveyed to Trinidad (ii. 228). The fever of Dominica in the same year, 1793, though affirmed by Dr. James Clark, a competent authority, to have originated there, was supposed by Chisholm to have been imported from some other place—but whence, no one could say. It was brought to Santa Cruz from Guadaloupe (ii. 340–1). The fever of Cayenne, in 1850–1, was at first attributed to a vessel—the Theresa—from Para.¹ The epidemic of Rio Janeiro, in the same year, was attributed to importation at Bahia by vessels from Baltimore, or New Orleans, or Canada, and thence by contagion to the first-mentioned city.² The agency of vessels from Africa was an after-thought.

Within a very few years the inhabitants of Demerara accused the Barbadians of introducing the disease into that colony.³ “What a spectacle,” exclaims Dr. Ferguson, in another publication—“what a treat to the cynic, to behold the circle of West Indian communities all generating yellow fever during certain unhealthy seasons, yet unable or unwilling to perceive that poison springs from beneath their feet, denouncing one another as the importers.”⁴

It is usually admitted that the greater part, if not the whole of the western coast of Africa is prolific of febrile diseases, which at times assumes the character of true yellow fever. But many of the stations along this coast have found defenders. A late writer has discovered that, with the exception of Sierra Leone, none of those stations are chargeable with the offence of giving origin to the fever, and that whenever it has broken out in any place, except the former, its appearance has invariably been preceded, within a very short time, by the arrival of vessels having the disease on board, and by the actual disembarkation of the sick.⁵ While, however, such is the view taken on this subject by respectable authorities, others have not hesitated to advance the opinion that the fever which prevailed in 1829, at Sierra Leone, a place which, as Dr. Johnson says, and as must be inferred if the preceding statement be correct, ought to be the last in the world to accuse others of being the source of contamination, was thought to have been imported from Fernando Po by the ship Eden.⁶ Nay, the same fever was supposed by some to have been imported from the Mediterranean in a vessel named the Caroline (*Gillkrest*). The fever of Ascension, in 1823, is attributed to importation in the ship Bann, and is said to have spread by contagion; and we all have heard of its introduction into Boa Vista in 1845, and of the manner in which it comported itself in that ill-fated island.

¹ Rept. on that Epidemic, Ann. Marit., N. S., viii. 154.

² Anglada, Traité de la Contagion, ii. 50–1.

³ Ferguson, Med. Gaz., Jan. 20, 1838, xxi. 640.

⁴ Bell's Eccl. J. of Med., iii. 13, from United Service Journal.

⁵ Mr. W. Ferguson, Surg. of Roy. African Corps, Med. Gaz., Aug. 1839, p. 838.

⁶ Boyle, Med.-Chir. Rev., xvi. 420; Rickets, Narrative of the Ashantee War, &c.

CHAPTER XI.

FACTS AND ARGUMENTS IN FAVOUR OF THE CONTAGIOUS CHARACTER OF THE
YELLOW FEVER.

THE contagious character of the yellow fever, as it has appeared in this and other cities and towns of this country and elsewhere, and its transmissibility from one place to another, have been, and continue to be, supported on the following grounds: 1. Many physicians in different parts of the world—the West Indies, the coast of Africa, the South of Europe, tropical America, as well as in this country—men of experience, of talent, and of highest professional authority, who have investigated the character, origin, and mode of propagation of the disease, unite in viewing it as endowed with contagious properties of greater or less activity. This opinion may be traced to a remote period in the history of the disease, and continues to this day to be very generally entertained. Thus, to say nothing of Du Tertre, Labat, Trapham, Moreau de St. Méry, Moreau de Jonnes, Rochefort, Péléprat, Mathias du Puis, Ligon, Feuillée, and other historians and travellers, we find Warren, Lind, Chisholm, Blane, Fellowes, Lempriere, Pym, Stevens, Gilpin, W. Wright, Gillespie, Stevens (of Santa Cruz), Dancer, Davidson, Bally, Lefoulon, Caillot, Kéraudren, J. Clark, Oyarirde, Pugnet, Frazer, W. Ferguson, as well as Panting, Lorillard, Negre, Cherot, Scott, Gregg, Stedman, Vicente, Del Valla, Oller, Antigue, Sandoval, McGee, Sir James McGregor, besides a host of others mentioned in Pym, Bancroft, &c., advocating the inherent contagious character of the fever in tropical regions; while, by Arnold, Sir W. Burnett, Gilbert, and McWilliams—to say nothing of many more of greater or less note—it is believed that, though not naturally endowed with such properties, it may acquire them, and has occasionally done so, under the influence of peculiar contingencies.

The opinion thus advocated in reference to the fever of the West Indies and the coast of Africa, has found ample support in the South of Europe, as the reader will discover in the writings of Berthe, Caisergues, Palloni, Dufour, Mantelli, Gianelli, Rochoux, Pariset, Bally, François, Pym, Sir James Fellowes, Copland, Sir David Barry, Faure, Audouard, Robert, Ameller, Arejula, Gonzales, Lafuente, and many more Spanish, French, English, and Italian physicians, most of whom believe in absolute, and some in contingent contagion.

Nor is it less certain, that in this country, from Louisiana to Boston, the disease has, by competent authorities, been viewed as propagated by contagion. In the very first account we have of the yellow fever of this continent, that of Charleston, the author, Dr. Lining, advocates the doctrine in explicit terms; and since his days, we have the testimony on the same side of

Sayre, Currie, Forsyth, Bayley, D. Hosack, Francis, Townsend, John Warren, A. Hosack, Pardon, Bowen, Monson, Barnwell, Monroe, McKnight, Seagrove, Tilton, Girardin, Strobell, Seaman, Carpenter, &c., besides Monnett, Dickson, Nott, Fenner, Frost, Anderson, and others of more modern times.

Some of these writers believe the disease to be always, and under all circumstances, contagious; another set, no less decided in their adhesion to the doctrine of contagion, think it requires for that purpose the co-operation of an impure atmosphere; while a different set, again, though inclining to the belief, express their views with caution, and may, in some measure, be said to be wavering on the border of the line, apparently deterred from the manifestation of an open and decided recognition of the doctrine by the fear of appearing whimsical, or a tender regard for their former creed, but ready to cross the Rubicon, and inscribe their names on the list of pure contagionists on the occurrence of additional facts, or the discovery of a plausible excuse.

Be it as it may with regard to the last of these categories, if men of the stamp of those included in the others, many of whom have had ample opportunities of investigating the subject, and have entered into the examination unbiassed by preconceived views, can arrive at the conclusions adverted to, we may safely form a favourable opinion of the doctrine of contagion as applied to the yellow fever.

2. In support of the contagious character of the yellow fever it may be, and has been adduced, that professional men who at one time advocated the doctrine of non-communication by direct or indirect contact, have subsequently abandoned that doctrine, and openly espoused the opposite one. We are informed by Sir W. Pym, that at the commencement of the epidemic of Gibraltar, in 1804, the medical officers were nearly unanimous with respect to the non-contagiousness of the disease; but that, before long, they became unanimous, with one exception, on the other side of the question.¹ It is also said that the Spanish faculty of Cadiz, in 1800, were all of opinion, at the first appearance of the fever, that it was non-contagious, but that, like the faculty of Gibraltar, they soon changed sides.² Again, we find that, among French physicians who have had occasion to observe the fever in tropical and temperate regions, some may be pointed out who, after advocating the non-contagious character of the disease, became decided supporters of the opposite doctrine. Dr. Guyon, who practised during some years at Martinique, and while there satisfied himself, by clinical observations and numerous experiments, that the fever was devoid of contagious properties,³ subsequently recanted his former views on the subject, and espoused those he had before combated.⁴ Drs.

¹ On Bulam Fever, pp. 22-3.

² Ibid., p. 205; Arejula, pp. 154-5; Bancroft, pp. 450-1.

³ See the certificate given by him to Dr. Chervin, and published by the latter in his essay "De l'origine locale et de la non-contagion de la fièvre jaune qui a régné à Gibraltar en 1828, ou réponse à quelques assertions émises par M. Guyon. Ann. Marit., vol. i. pt. 2. 1832."

⁴ Notice sur Gibraltar, sui vie de quelques réflexions sur l'origine de la maladie de cette ville en 1828, par M. Guyon. Annales Mar., 2 partie, tome i. 1830.

Francis and Bally, who both were attached to the medical staff of the army of St. Domingo under General Leclerc, and witnessed the awful ravages occasioned there by the yellow fever, may be cited to the same effect. The former published, in 1804, an account of that noted epidemic.¹ In this publication, the author warmly espoused the doctrine of non-contagion and non-importation. In a subsequent work, which appeared fourteen years after,² he reiterated the same views, and extended them to the fever of this country, where he sojourned several months. Being sent to Catalonia in 1821, the same physician there became a zealous contagionist, joined Drs. Pariset and Bally in drawing up a report on the epidemic of Barcelona, in which the most ultra views in favour of his new creed are advocated,³ and unhesitatingly taxed, in open academy, the non-contagionists of this country with ignorance and bad faith.⁴ As regards Dr. Bally, who, after the return of Dr. Gilbert to France from St. Domingo, acted in the capacity of chief physician of the above-mentioned army, we have the testimony of Dr. Valentin,⁵ contrary to his own assertion—to the effect that, on his arrival in France, he discarded the idea of contagion. In 1805 he was sent to Spain, and there became a convert to the opposite views, which he expounded in a volume issued in 1814,⁶ and at a late period united with his colleagues, Drs. Pariset and Francis, in the publication of the report above mentioned on the epidemic of Barcelona.

Dr. Girardin resided several years in New Orleans, and was associated with Dr. Gros in drawing up the report published in the name of the Medical Society of that city on the epidemic of 1817.⁷ He was at that time a decided non-contagionist, as is attested by the language of that report (p. 60), and the testimony of his fellow practitioners.⁸ He subsequently became a convert to different views, and propounded them in various publications,⁹ and during the debates which at different times have taken place at the Academy of Medicine. To the same effect may be cited Dr. Rochoux, who, in a highly valuable volume published in 1822, opposed with vigour and ability the contagiousness of the yellow fever which he had observed in the West Indies. But, while continuing to entertain the same views

¹ Dissertation sur la F. J. observée à St. Domingue pendant les années 11 et 12. Soutenue à Ecole de Méd. de Paris le 11 Thermidor an 12 (1804), par André François.

² Gazette de Santé, February 21, 1818.

³ Hist. Méd. de la F. J. observée en Espagne, et particulièrement en Catalogne, dans l'année 1821, par MM. Bally, François, Pariset. Paris, 1823.

⁴ 6 June, 1826. See Chervin, Examen des principes de l'administration en matières sanitaires, p. 100 (note).

⁵ Valentin, Réflexions sur la F. J., Journal Univ., xi. 143; Journal Gén., lxi. 358; Chervin, Examen des principes de l'administration en matières sanitaires, p. 100.

⁶ Du Typhus Américain ou Fièvre Jaune, p. 387.

⁷ Rapport fait à la Société de Méd. de la N. O. sur la F. J., &c., pendant l'été de 1817, par MM. Gros et Girardin.

⁸ Letter of the Med. Soc. of N. O. to the French Consul of that city, 29th Dec. 1829. See Chervin, Pétition à la Chambre des Députés, p. 109.

⁹ Mémoire sur la F. J. considérée dans sa nature et dans ses rapports avec les Gouvernements. Paris, 1820.

in relation to the disease as it shows itself in tropical regions, he became a strong advocate of the contagiousness of the yellow fever of temperate climates, which he, almost alone, views as a disease totally different, in character and phenomena, from the former.

Our own country has furnished us with examples of like conversions from a belief in non-contagion to a belief in contagion. Among these one stands prominent from his acknowledged abilities, his great experience, and the high position he occupies in the professional world—I allude to Professor Dickson, of Charleston. In his account of the epidemic which broke out in Charleston in 1817, that distinguished physician spoke of the fever as being devoid of the property of transmission, and says of Drs. Hosack and Pym: “The ingenuity of the former, and the industry and strength of reasoning of the latter of these writers, have brought forward a great mass of facts and arguments in support of their opinion. Yet I cannot help thinking that, even from their own works, evidence sufficient to prove the contrary may be collected.”¹ In a subsequent publication,² he expresses himself in a way to make us infer that he still, to a certain extent, retained the same views. “If we attribute its spread to contagion, why, at some times, is it transported across the Atlantic, and yet in this, the very land of its nativity, shall refuse to emigrate to our sister Augusta, or our terrified neighbours of Wilmington?” Nevertheless, about the time of this publication, Dr. D., in a letter to Dr. Strobel, under date of Jan. 14, 1848, renounced the doctrine of non-contagion, and espoused the one he had formerly repudiated. It is stated by Dr. Hosack,³ that Dr. Ledyard, when he first entered upon the duties of the Health Office, went to Staten Island with the belief that the yellow fever was not an imported disease, but generated at home; but that he was compelled to change his belief, being satisfied from all his observations, that the disease was exclusively derived from the West Indies.

No one can deny that these changes, and others of the same kind that might easily be pointed out, speak loudly in favour of the contagious character and the transmissibility of the disease; for nothing but facts of the most positive nature could induce men of science and correct judgment, to forego views long cherished, and adopt others of an opposite kind.

3. An argument in favour of the contagious nature and exotic origin of the yellow fever of this city as of many other places where it has at times appeared, is derived from its dissimilarity to the ordinary autumnal diseases of those localities. In a memorial addressed to the legislature of Pennsylvania, under date of 5th December, 1797, by the College of Physicians, of this city, it is said: “The disease in question is essentially different from the fevers that occur in this climate, and which originate from domestic causes. The difference particularly regards the general progress of the symptoms and the mortality, as is evident upon a comparison of its history with that of

¹ Philad. Med. and Phys. J., iii. 273.

² Remarks on the Y. F. in Ch. (S. C.) in the summer of 1838, Eclectic Journal, iv. 111.

³ Letter to Dr. Currie, in Facts by the Philadelphia College of Physicians, p. 32.

the ordinary diseases of this city. A disease which resembles the fever of 1793, and of this year, in many important points, has long been known in the West Indies and those parts of America situated between the tropics, and in seven or eight instances in which a similar disease has occurred in the United States in the course of this century, there is good reason to believe that it was derived from those countries."¹

In another publication, the College say: "Look at the disease itself; does any fever of our country assume such forms, or produce such effects? Do our severest bilious fevers generally terminate fatally in a few days? Do they steal on insensibly, infecting one person after another in a family, and in a neighbourhood? Are they ever equally severe in seasons so opposite as 1797 and 1798? Do not the rise, the symptoms, the progress, and the termination of this fatal disease, demonstrate that it is totally different from anything we have been accustomed to?"² And in proof of this difference, it is remarked that the characteristic symptoms of the disease in question are, generally, a constant fever for about three days, which, in dangerous cases, is followed by a cessation thereof, by a peculiar yellowness of the skin, and in fatal cases by a vomiting of a dark coffee-ground matter.³ These and other phenomena, appertaining to the yellow fever, seldom if ever present themselves, either singly or similarly combined, even in the most malignant form of the remittent fever of this country, as has been fully shown in a preceding chapter; while the disease from which they originate occasions a mortality such as is common to pestilential diseases, and far exceeding that of ordinary seasons.

While some of our physicians are disposed, for the reason assigned, to regard the yellow fever of this city as endowed with properties different from those possessed by ordinary autumnal diseases, and as imported from the West Indies, where a similar complaint often prevails, a reference to the writings of Warren, Chisholm, Pym, and others, will show that kindred facts have there been adduced in favour of the contagious character and foreign origin of that fever, which, as it is alleged, differs essentially from the ordinary febrile complaints of the country.

4. The ordinary autumnal fevers of this and other countries visited by the yellow fever make their appearance, with more or less regularity, and follow a similar course, nearly every year, while the other everywhere shows itself at intervals of greater or less duration, and, after assuming the sway more or less effectually for a short time, and producing a distress and mortality proportioned to the violence of the visitation, but always greater than that occasioned by the other forms of fever, disappears completely, and, in places subject to those forms, gives way to them. During these periods of exemption from the yellow fever, the other febrile complaints, at the accustomed time, show themselves as before, and prevail in their usual way, and to an extent as well as with a character and result differing comparatively little in

¹ See Rush on Fever of 1797, p. 58, for this document.

² Facts, &c., by College of Physicians, pp. 15, 16.

³ Ibid., p. 17.

different years. This irregularity of appearance is noted even in countries where the yellow fever assumes the character of an endemic—in the West Indies, or Vera Cruz, and in some of our southern States, or other similar localities. Warren had practised several years at Barbadoes before he saw a case of the disease in question, which, according to Hughes, did not make its appearance there before 1691. It prevailed in 1713 (*Chisholm*, ii. 117), in 1721 and 1724 (*Warren*, p. 4), leaving the intermediate years free. Since that time, the island has often been exempted from the disease during a succession of seasons. We are informed by Dr. Imray, that when the fever broke out in Dominica in 1838, it had not been known to prevail for seventeen years before. It is affirmed by Moreau de St. Méry,¹ and other writers, that the fever was unknown in the French and other colonies till the year 1688, when it appeared in Martinique, and soon after in other parts of the West Indies.

If we open Desportes's excellent work on the *Diseases of St. Domingo*, we shall find that in the course of the fourteen years he resided at the Cape, the place was free from the fever during five years. From 1783 to 1793, or later, it does not seem to have prevailed in that island. The fever appeared at Cayenne in 1763, '64, and 1802. From that time till 1850 no cases occurred. Fevers of a remittent malarious character alone were very prevalent.² Martinique was free from the disease in an epidemic form, during an interval of ten years, 1827 to 1838. The epidemic of Grenada, described by Chisholm, was the first that had visited that island for thirty-one years. Georgetown (Demerara), suffered severely in 1793; next, in 1803; then in 1819. From that year to 1837 the disease did not show itself in the epidemic, if, indeed, in any form.³ In fact, there is scarcely any locality within the tropics where the fever does not intermit during periods of greater or less extent. From Humboldt (p. 756) we learn that such is the case also on the coast of Mexico and South America. In the city of Vera Cruz, the fever, now so prevalent, did not appear from 1776 to 1794 (p. 756). Ulloa, in his *Voyage to South America* (i. 41, Book I. Chap. V.), informs us that the first appearance of the disease in South America was in 1729, when it destroyed the largest part of the crews of the *Guarda Costas* at Santa Martha. According to the same authority it first appeared at Guayaquil in 1740 (*Ib.*, p. 140). From this period to 1842, it did not show itself.⁴ In Brazil, the disease prevailed in 1687–90. From that date we hear nothing of it in that country until 1849–50, when it broke out and spread extensively in Pernambuco, Bahia, and more particularly in Rio Janeiro.⁵ Peru, in some parts of which periodic autumnal fevers are very common, was visited by yellow fever, for the first time, in 1818. From that year till 1854 there was

¹ Description de St. Domingue, i. 700.

² Extrait du Rapt. Méd. sur l'Epidémie de Fièvre Jaune de la Guyane Française en 1850; Ann. Marit., viii. 1852, i. 153.

³ Frost, Med. Repos., pp. 12, 13; Blair, Some Account of the last Yellow Fever Epidemic of British Guiana, p. 28.

⁴ See Rept. of N. O. Sanitary Commission for 1853, pp. 142, 289.

⁵ Pennell and Lallement, op. cit.

an exemption from the disease. It then prevailed extensively at Lima and Callao.¹

From 1817 to 1822, the city of New Orleans was free from the disease. Since then, that city, as also Mobile, Pensacola, Natchez, and Charleston, have experienced intervals of respite, during which the ordinary fevers of summer and autumn did not cease to prevail with more or less severity. In Charleston particularly, as we learn from Lining, Ramsay, Shecut, and Simons, the intervals have been sometimes very long. The disease did not appear from 1703 to 1728. There was also an intermission from 1739 to 1745. From 1748 to 1792, and from 1807 to 1817 we have no records of the existence of the disease.² It did not prevail from 1830 to 1838; from 1839 to 1849, or from the latter year to 1852. Indeed, it has seldom, at any epoch from 1699 to this day prevailed there in two successive years. In Mobile, no yellow fever occurred, except sporadically,³ for eight years previous to 1837. In Savannah, the fever did not prevail epidemically from 1820 to 1853.

Without dwelling here on the results of observation to the same effect in New York, Baltimore, and other cities of the middle States, it may be mentioned that Philadelphia, which was settled in 1683, was not visited by the yellow fever before 1699—that, from that period, forty years elapsed before the next epidemic appearance of the fever took place; and to say nothing of several intervals of comparatively short duration, the city remained exempt from the ravages of this formidable disease from 1747 to 1762; from the latter year to 1793; from 1805 to 1820, and from the last-mentioned year to 1853, without, however, ceasing in former times to be annually liable to bilious remitting, and other forms of the ordinary fevers of the season. When it broke out at Cadiz, in 1800, it had not been seen there for thirty-six years; the last visitation being that mentioned by Lind as having occurred in 1764.⁴ The city again suffered in 1810, 1819, and 1820, but at no other period. Barcelona, which suffered so severely in 1821, had not been visited by it since 1803. It had not been seen at Gibraltar for a century, at the time of its outbreak in 1804. Until the latter year, the city of Leghorn had never suffered from the yellow fever. In the summer and autumn of that year, the disease prevailed there with violence, but has not since made its appearance.

¹ A. Smith, Edinb. J., April, 1853, p. 186.

² Ramsay, Review of the Improvements, Progress, and State of Medicine in the 18th century, p. 30; Shecut, p. 98; Simons, Rept., pp. 6, 7; Ib., Charleston, Med. Journ., vi. 779.

³ Nott, New Orleans Journ., iv. 565.

⁴ The plagues which prevailed at Cadiz in 1599, 1649, 1680, and 1730, as also perhaps two of an anterior date recorded in the illustrated chronicle of that city from 1507 and 1582, were, in all probability, prevalences of a disease very like yellow fever, for we are told that “En uno de estas pestes se especifica el sintoma del vomito negro parecédo al que se tolera en el Nuevo-mundo, y en otra se destino la isleta de S. Sebastian para descontagiar los géneros—Alfonso de Maria, p. 10; Ib., El Contagio, Discutido, p. 15.

This irregular and periodic appearance of the yellow fever, and its disappearance during a series of seasons characterized by meteorological phenomena and local peculiarities, differing little, if at all, from those observed in seasons of epidemics; and the constant prevalence at every return of the summer and autumn, of fevers differing from the former, due evidently to the operation of the above influences and of local causes; and, moreover, admitted on all hands to be devoid of contagious properties, would seem to indicate, that the yellow fever, when it does occur among us or elsewhere, must—especially when other circumstances, presently to be mentioned, are combined—be ascribed to other sources than those adverted to; that it must be referred to importation from sickly parts, and if so, must be viewed as endowed with powers of propagation different from those possessed by the ordinary fevers of the season.

5. Some of those who believe in the contagious character and foreign origin of the yellow fever, derive support in favour of their views from the fact that the disease very generally commences in sea-ports, and especially in those that communicate with the tropics or other places within the yellow fever zone, or in localities between which and the others the communication is direct; while inland towns, which, as they aver, are equally exposed to the ordinary causes of fever, almost invariably escape. It is observed, besides, that, in places that suffer from the disease, the latter shows itself at first near the shipping, or on board of these, among individuals employed in or visiting them, or who reside in their immediate vicinity.¹ It is further found that, from the locality originally infected, the disease spreads, with more or less rapidity, to other parts; and that it not unfrequently happens that while such localities, the extent of which is at times very limited, are severely affected, the adjacent parts and the country around remain as healthy as in ordinary seasons, or are even found freer than usual from febrile complaints.

The frequent outbreak of the fever in such localities, its almost exclusive limitation there, and its radiation thence to adjacent districts, have been noticed as well within the tropics as in Europe and in the temperate regions of the American continent.

In this country, the fever has shown itself at New Orleans, Mobile, Charleston, Savannah, Norfolk, Galveston, Baltimore, Philadelphia, New York, Boston, New London, Middletown, and other sea-port towns; and if we find it breaking out and spreading in places situated far from, or having no direct communication with, the sea, and therefore not exposed to the immediate vicinage of ships from places within the yellow fever zone, or rather from places situated within the tropics, or which received the disease from these—if, I say, we find the fever appearing in places so located, we shall be certain that they are in communication with the others, and that the outbreak there has been consecutive to the introduction of the fever into some sea-port town. Thus, New Orleans gets the fever from the West Indies, or South America, or Africa—perchance from Philadelphia, Baltimore, or New

¹ College of Physicians, pp. 16, 24.

York; but Natchez, Vicksburg, Grand Gulf, and other places on the Mississippi, and other neighbouring towns, get it from New Orleans; Mobile receives the contagion from the West Indies or New Orleans, and communicates it to the neighbouring towns. New York gets it from abroad, but communicates it to Long Island, New Haven, &c. Augusta, Macon, &c., do not receive it from foreign regions, but from Charleston or some other seaport. Cadiz derives the contagion from America, and, in her turn, communicates it to Seville, &c.

The epidemics of this city demonstrate the fact of this influence in the most conclusive manner; for here the yellow fever has invariably made its first appearance on or in the vicinity of the wharves and shipping. Thence it extends to the adjacent parts of the city, and in almost every instance in which it has prevailed epidemically among us, or has appeared only sporadically, such localities have proved the principal foci of the disease. "Where," ask the College of Physicians, "do we see the first appearance of our pestilential fever? Is it amongst the marshes to the southwest of our city, or in the neighbourhood of the wharves? Is it in the confined alleys, or on the salubrious banks of the Delaware at Kensington? Is it not always near those places where vessels from foreign countries are found?"¹ Such was the case in the years 1793 and 1797. On these occasions, the disorder, as always, broke out near the river, and spread up and down; going from house to house, and from street to street, until it extended from the river quite to the westward part of the city. In 1798, it spread over the whole city in the way it had done before, although it appeared in different parts of it nearly at the same time;² and if the reader will refer to what has been recorded in a former part of this volume, he will find that in other epidemics the same regular march of the disease, from the spot originally affected, has generally been observed. Having in a former chapter alluded to these circumstances somewhat in detail, I need not again dwell upon them here. It will be sufficient to say that the College of Physicians, in their memorial to the Legislature, already mentioned, strongly insisted on these facts in support of their favourite doctrine, and called attention to the circumstance that, in 1794 and 1797, the suburbs of Philadelphia and the country adjacent were more healthy than usual at the same season; and that, at the commencement of the disease, all the parts of the city, excepting the small spaces to which it was confined, were remarkably healthy.³

6. The yellow fever appears at times, and prevails extensively, when nothing peculiar, as regards meteorological phenomena and those circumstances of locality to which domestic causes of febrile diseases are attributed, presents itself, capable of accounting for the occurrence of the disease. Thus, in reference to the epidemic of 1798 in Philadelphia, it was insisted upon, by the College of Physicians, "that the weather had not been more sultry that season than in many other years in which not even a sporadic case of the dis-

¹ Facts and Obs. by the College, p. 15.

² Condie and Folwell, pp. 1 and 11 of Appendix.

³ Rush, Fever of 1797, p. 6.

order was met with, and that the police of the streets was then vastly better than it had been formerly, especially during the period that the British troops were here, and immediately subsequent to their abandonment of the city."¹ The seasons in 1797 and 1798, the College again maintained, were of opposite character, and yet the fever prevailed on both occasions with great severity; a circumstance never noticed as regards fevers due to domestic causes, and spreading without the aid of contagion.² It is further urged that, at the time of the early epidemics of our city, the latter was thinly built—the houses scattered over a large surface, and generally separated from each other by open spaces; that in 1699 it was in fact a small village, and consequently well ventilated and devoid of filthy alleys and lanes, which the advocates of domestic origin regard as a principal source of infection. While the disease thus attacked the city in its infancy, and spread desolation among its small and scattered population, it did not prevail from 1762 to 1793, though the place had become more thickly built and more thronged with inhabitants, and though in 1778 it was in possession of the British army, by which it became surcharged in population, and, as already stated, in a less satisfactory hygienic condition. If, therefore, the fever arises from domestic causes, and does not spread through means of contagion, it ought assuredly to have appeared in 1778, and not in 1699, 1741, or even in 1762.

Nor is it less certain that at the time of the famous epidemics of 1793, 1798, and of the less extensive one of 1820, Philadelphia, being one of the cleanest, best aired cities in the Union, and indeed of the whole world, and containing no greater sources of infection than usual, ought not, were the disease of domestic origin, to have been affected, while other places, less favourably situated in these respects, remained exempt from the calamity. It is difficult, if not impossible, to account for the difference otherwise than by admitting the introduction of a contagious poison, by which the disease is produced and disseminated. The supposition must appear the more probable when we bear in mind that while the fever does not, as a general rule, spread in the country, it prevailed on several occasions in the villages of Kensington, Marcus Hook, Chester, and Wilmington, which all enjoyed advantages of ventilation and air almost equal to those obtained in the open country, and that it has even been known to spread in the latter.

This argument, which has been insisted upon by contagionists, derives support from the results of observation made elsewhere, and has not been neglected by the supporters of the doctrine under consideration, in this and other countries. On several occasions, the yellow fever has prevailed in small, well-situated, and clean towns and villages in the southern parts of the United States and Europe, and spared large cities, where sources of infection, which some would make us believe are the causes of the disease, abound.

7. The exotic origin of the yellow fever, and its complete independence of the causes which give rise to the ordinary fevers of our cities, and of the country generally, may be further inferred from the fact that the appearance

¹ Condie and Folwell, p. 27.

² Facts and Obs., p. 16.

of the disease in an epidemic form occurs often in seasons unmarked by any thing peculiar in the character or extent of prevalence of those fevers either previous to, during, or after the epidemic occurrence of the former. Indeed, instances are to be found, in which this malignant fever has been preceded by a more than common healthfulness of both town and country, or co-existed with a like condition in the surrounding districts, thereby indicating the absence of an epidemic constitution of atmosphere. And on the contrary, places may be pointed out which, though sometimes or even often visited by the yellow fever, have remained free from it on occasions when the existence of that epidemic constitution was evinced by an uncommon prevalence of summer and autumnal fevers. Dr. Rush, while discarding the idea of importation, admits that the noted epidemic of this city in 1798, as also that of the year following, was preceded by an uncommon and general healthiness (iv. 113). We are told that during the spring and early summer months of the year 1803, the inhabitants of Philadelphia enjoyed a state of health uncommonly favourable. Except among children, who were as usual affected by the disorders of the season, physicians found but little to call forth the exercise of professional skill "till between the 19th and last of July, when the fever broke out" (p. 144).¹ Dr. Hosack informs us that, during many visitations of yellow fever—in 1791, 1795, 1798, 1805, and 1819—New York was proverbially healthy; and that while the disease was there co-extensive with the foul atmosphere of the district in which it had been introduced, other parts of the city, equally if not more foul, were proverbially free from it.² "We look in vain on the list of diseases," says Dr. Townsend, in reference to the epidemic of 1822, "for the local and general affections with which yellow fever, if we are to believe the romantic doctrines of some writers, is said to inosculate so freely, some of which are averred, before the appearance of this disease, to have become epidemical, and to be, as it were, the forerunner, or *avant couriers* (*coureurs*) of this fatal malady. No unusual affections of this kind were observed to prevail either before, at, or after the time at which this foreign disease was brought into the city" (pp. 274–5).

Dr. Hosack further states that when, in 1820, Philadelphia and other places suffered sorely from yellow fever, New York enjoyed an exemption from the pestilence, while in other respects it proved an unprecedented season of general sickness (*op. cit.*, p. 74); and the history of the negro fever of this city, in 1820, and of the remittent fever of that and several succeeding years, when the yellow fever did not prevail, shows that the two forms of disease are independent of each other.

Dr. Rochoux says the same of his "amaril typhus," and draws from the supposition of that disease not being preceded by any other form of febrile complaints, an argument in favour of the distinction between it and the yellow fever of the West Indies (p. 546).

¹ Caldwell, *Med. Repos.*, vii. 144; *Ib.*, p. 186; Stringham, *Obs. on the Yellow Fever of America*, *Edinb. Journ.*, i. 144.

² *Med. Police Essays*, ii. 43, 74.

In 1820, at New Orleans, the vernal fevers were uncommonly mild in character.¹ In Charleston, the yellow fever prevailed extensively in 1824, though the country was healthier than usual. The same thing occurred in 1837, '8, '9, and '40.² Dr. Kelly reiterates the same fact, and says that a like occurrence was noticed at New Orleans in 1832, 1833, 1835, 1837 and 1839. On the contrary, in 1840 and 1841, Mobile was particularly healthy, though the surrounding country was very sickly.³ The epidemic of Dominica, in 1841, burst out suddenly, without giving intimation of its approach, and ceased without the prevalence of intermittent fever.⁴

8. The communication of the disease from the sick to the well may, in general—though more particularly in the commencement of an epidemic, or when the disease prevails to a limited extent—be traced in a satisfactory manner. In other instances, the fever is found to be due to exposure to articles of merchandise, clothing, bedding, or furniture impregnated with the seeds of the contagion. That such a communication is not always apparent, is admitted. "Your memorialists," say the College of Physicians, "are aware that cases may be adduced where the disease has occurred in persons who were not known to have been exposed to imported contagion; but such is the subtle nature of this power that it often exists unsuspected, and similar difficulties occur respecting the smallpox and other contagions, allowed by all to be of foreign origin."⁵ But, however this may be, it is usually found that the disease steals on insensibly, infecting one person after another in a family and in a neighbourhood,⁶ and that the communication to nurses, children, parents, or persons about the sick takes place in localities, and under circumstances which preclude the possibility of attributing the occurrence to any other cause than contagion.⁷ "Though the disease," says Dr. Currie, in his account of the fever of 1793, "was highly contagious, the influence of the contagion was circumscribed to a narrow sphere, for none but those who approached near to the sick, or to such articles as had been in contact with them, or within the sphere of their effluvia so as to be impregnated thereby, were affected." "I have known lately," he remarks in another place, "several instances that could not be accounted for upon any other principle than that of contagion. The most striking of the cases I allude to is, where several of a family living in a healthy and uninfected part of the city, took the disorder from a person in the family whom they had for several days closely attended, without having themselves been in any part of the city that could even have been supposed to be infected."⁸

In 1813, at Cadiz, "when any individual stranger in a house was taken ill, the fever was commonly confined to that particular house, unless there were

¹ Report of the Committee of the Phys. Med. Soc., p. 5.

² Dickson, i. 337.

³ Kelly, Am. J., N. S., xiv. 375.

⁴ Imray, Edinb. Journ., lxiv. 336.

⁵ Rush, p. 61.

⁶ Facts, by College of Phys., p. 16.

⁷ Additional Facts by Col., p. 45; Currie, Fever of 1798, p. 28; Condie and Folwell, p. 61.

⁸ Fever of 1798, p. 28.

fresh objects for it to act upon; and when it did spread, it was through the medium of visitors and others who frequented the chambers of the sick." If the disease did not spread as generally as it had done formerly, it is because most of the people had passed through it on former occasions.¹

The case of Dr. Valli is well known. He lay in the bed of an individual who had just died of the yellow fever, put on his clothes, rubbed himself with the matter of the black vomit, and thereby took the fever and died in a short time. Dr. Bally, while at Barcelona in 1821, received the contagion from an individual whom he visited in the immediate vicinity of that city. Other cases, just as strong, occurred in the same place on the same occasion, as the reader will find recorded in the report of that epidemic by Pariset, Bally, and François (pp. 49-50). The reader may recollect that one of the early epidemics of this city was the effect of the exhalations issuing from a trunk of clothes belonging to a person who had died of the disease. Cases in point are related as occurring at Barcelona.²

9. Nor is it alone within the usual sphere of prevalence of the disease, that instances of positive and direct communication have been satisfactorily pointed out. In numerous instances, the contagion has been carried to distant villages or towns, or to public establishments situated in parts of the city which otherwise remained free from the calamity.

In 1798 the fever was communicated to some of the inmates of the Pennsylvania Hospital by the assistant nurse of the men's ward, and others who received the contagion in the city. In all the former epidemics through which Philadelphia had passed, the establishment had, by proper care, been perfectly preserved.³ Such had also been the case with the jail, which heretofore had escaped the disease, but was this year, like the hospital, invaded.⁴

Dr. Addoms, in a dissertation on the yellow fever of New York, in 1791, states on good authority that many of the boatmen who frequented the city from Long Island, and the upper part of Hudson River, carried the disease home, where it proved fatal to several (p. 7); the same author mentions on the authority of an eminent physician of New York, that a gentleman from Linn (Conn.), was on a visit to that city and there went through the disease in a severe form. He relapsed on his return home, and died. "The greater part of his family caught the contagion, and soon became affected with a similar disease, which proved fatal to his mother and some other persons in the family." (*Ibid.*, note.) The fever was communicated in October, 1799, to the north side of Staten Island from New York, by an individual who had lately arrived from the city. The physician in attendance took the disease and died, and was soon followed to the grave by his wife. Three years after, the fever was introduced in the island from the city.⁵ Kensington, in 1798,

¹ Sir James Fellowes, p. 287.

² Pariset, p. 84, &c.

³ Park's Communication to the College, Additional Facts, p. 33.

⁴ Condie and Folwell, Fever of 1798, p. 75.

⁵ Channing, Facts relative to the Contagious Nature of Yellow Fever in the pure Air of the Country, Med. and Phil. Register, ii. 22.

received the contagion from Philadelphia from which it was then separated.¹ So did Chester, Marcus Hook, Wilmington, Petersburg, and Richmond (Va.).² The next year the disease was conveyed again from Philadelphia to Wilmington,³ Swedesborough, &c.⁴

Dr. Lyman Spalding relates the following as occurring in 1798, at Dartmouth. "My brother, hearing of my sickness, came twenty miles to my assistance, remained in my chamber almost continually for sixteen days, taking the sole care of me. About two weeks after his return, he was attacked with the fever; no other person, within three miles, being affected. In Windsor, a Miss Bayley was very severely attacked in the sickly part of the town three miles from her father's; an elder sister came to her assistance, and was immediate nurse till she recovered; then returning home, she was herself attacked and died. Another sister, who was her immediate nurse, and had seen no other person labouring under the disease, nor been in the infected part of the town, was also attacked, and died."⁵

The late Dr. Hosack, of New York, in a letter addressed to Dr. William Currie, of this city, mentions several facts relative to the communication of the disease at Huntington (L. I.), in 1795 and 1798.⁶ In 1853, the fever prevailed extensively at Mobile. But it was not limited to that city; it extended not only to all the little settlements within five or six miles of it, but to Citronelle, the present terminus of the Ohio Railroad, thirty-three miles from town; "on to the various towns on the rivers tributary to the bay as far as steamboats have gone, and no further—to Montgomery and Demopolis, for example, to say nothing of many intermediate points."⁷ The yellow fever of Washington (La.), in 1853, was undoubtedly introduced from New Orleans by individuals and merchandise, and spread by contagion.⁸ The same may be said of many other towns, and even of plantations along the Mississippi River and its tributary streams, or in the interior—Thibodeauville, Donaldsonville, Plaquemine, St. Francisville, Pattersonville, Vicksburg, Port Gibson, Grand Gulf, Alexandria, Opelousas, Rodney, Franklin, New Iberia, Natchez, as also along the bay of St. Louis, Shieldsborough, Biloxi, Tampa, and Pascagoula Bays, Woodville, Washington (Miss.), and Coonville⁹ (Ala.).

The following cases are worth mentioning: "A Jew peddler, recently from Port Gibson, was seized with fever while on his rounds, and evidently infected three families. He was literally driven from one to the other till he

¹ Facts, &c., by the College of Phys., pp. 22, 45.

² See Currie, Fever of 1798, pp. 109, 136, 138, 141; Facts, &c., by College of Phys., p. 23.

³ Currie, Fever of 1799, p. 51; Tilton, Med. Repos., iii. p. 128.

⁴ Additional Facts, p. 74.

⁵ New York Med. Reposit., iii. 8 (note).

⁶ Facts relative to the Contagious Nature of the Yellow Fever in the Pure Air of the Country, Med. and Philos. Reg., iii. 191.

⁷ Nott, N. O. Journ., x. 571.

⁸ Cooke, N. O. Journ., x. 603.

⁹ Fenner, Report to Med. Assoc. Trans., vii. 512, &c.; Drake, ii. 214, &c.; Monette, pp. 64, 65, 66, 67; Strobel, pp. 117, 123, 130, 187; Carpenter, pp. 17, 22, 23, 25, 26, 27, 28, 29; Kirkpatrick, N. O. Journ., ii. 49.

got to Heath's, where, being too ill to get back to town, he was put in a back shed-room of the dwelling, and died of black vomit on the 20th of September. So much were the people of the house alarmed, that the corpse was hurried into a coffin without dressing; his pocket-book of papers, purse of money, and everything on his person, were buried with him. The weatherboarding and gable-end of the room were knocked off to let in air and rain; the bedding and furniture were burned, and only a few pieces of the latter were allowed to lie out one or two hundred feet from the dwelling for *two weeks*; meantime, no one sickened. At the end of two weeks, the bedclothes were brought in, boiled, wrung out, and dried about the house, Mrs. Heath seeing to it. Within eight days from that time, and about twenty from the burial of the poor peddler, Mrs. Heath, her husband and son, the woman who washed the clothes, and several other servants, sickened; the first two died, and the others recovered." The peddler communicated the disease on another plantation to a black boy, who was seized sixteen days after the former had left. Sixteen days after the time of his death, his mistress was attacked and died similarly.¹ The following case was communicated to Dr. Fenner by the late Dr. George Banks, of Clinton (Miss.): "A Mr. McManus, who lived on the hills of Hinds County, four miles from Clinton, and more than forty miles from Vicksburg, went down to the latter place (during the epidemic of 1841), and remained there several days. He returned home, and was very soon attacked with yellow fever, of which he died, after throwing up black vomit. A few days after his death, his wife, who had nursed him assiduously during his illness, and had not been away from home, was attacked in a similar manner, and died with black vomit and yellowness. Their residence was in a high and ordinarily healthy locality, where yellow fever was never known to prevail."²

In Spain, the disease has been carried from Cadiz, Barcelona, Carthagena, Malaga, Gibraltar, and other seaports, where it had been introduced from abroad, to some thirty neighbouring towns and villages,³ as every one may know who consults the works of Pariset, Berthe, Arejula, Fellowes, Pym, &c.

In the year 1798, it was introduced into the village of Germantown, situated on high ground about five miles from the city of Philadelphia, by a person, Betsey Johnson, who had remained some days in the city; and it spread to some extent among the family, and in the neighbourhood. "These melancholy circumstances," says the writer to whom we are indebted for an account of the facts in question, "occurred in a village which has long been remarkable for its salubrity, at a time when the other inhabitants enjoyed their usual health. In most of the cases, the disease appears to have

¹ A. P. Jones, in a Communication to Dr. Fenner, Trans. of Med. Assoc., vii. 523.

² Fenner's Rept. in Trans. of Med. Assoc., vii. 539.

³ Seville, Espera, Ubrique, Moron, Puerto Real, Rota, San Fernando, P. St. Maria, Arcos la Rambla, Le Brisa, Carlotta, Xeres, San Lucar, Cordova, Medina Sedonia, Ximena, Los Barrios, Algesiras, Ayamonte, San Roque, Espejo, Mahon, Palma, Lebreia Tortosa Mequinenza.

been contracted at the house of Mrs. Johnson, which, before this distressing period had been eminently distinguished by the health and longevity of its inhabitants. The family were extremely neat, and it may be asserted with confidence, that the premises were never more clean than they were at the time of this truly affecting catastrophe. What cause but contagion is adequate to the production of such a disease among persons so situated?"¹

CHAPTER XII.

FACTS AND ARGUMENTS IN FAVOUR OF THE CONTAGIOUS CHARACTER OF YELLOW FEVER—CONTINUED.

10. As an additional and strong proof of the exotic origin and contagious character of the yellow fever, it must be mentioned that while almost every epidemic of the disease in this and other cities, has been traced to communication with vessels or individuals from unhealthy places, and while, with some few exceptions, individual cases may be traced to intercourse with the sick, or objects of clothing, &c., the introduction of the fever may be and has often been guarded against by quarantine regulations; and the danger of infection obviated by seclusion from places already affected, or by avoiding objects or persons contaminated by the contagion. In 1798, the city of Baltimore was evidently preserved by such means; while Philadelphia and Boston, which (and this is particularly the case with the latter city) are not as much exposed by their situation to the dissemination of tropical diseases, but where the health laws were not so efficient, suffered considerably.² Utrera in Spain, between Seville and Xeres, was saved in 1819 by a strict enforcement of quarantine restrictions, while all the neighbouring places, San Fernando, Cadiz, Chiclana, Seville, and Xeres were severely scourged. In 1800, on the contrary, when no precautions were taken, it suffered severely.³ Pariset, to whom we are indebted for the preceding facts, informs us that in 1800 and 1820, Lebreja was saved by the same means.⁴

We are told that, in the year 1793, all the prisoners in the jail of Philadelphia, amounting to more than two hundred, the pensioners in the almshouse, and the patients in the Pennsylvania Hospital escaped the disease, owing to the precaution of preventing the admission of any sick or suspicious persons, as well as every infected article, into those places. All the families, also, that remained in the city, confined themselves and their domestics to their own houses, permitted no sick person or any one lately recovered from the disorder,

¹ Wistar, *Additional Facts by College of Physicians*, p. 36. See also Condie and Folwell, p. 97; Currie (1798), p. 114.

² *Facts and Obs. by College of Physicians*, pp. 15, 20.

³ Pariset, *Fever of Cadiz in 1819*, p. 57.

⁴ *Ibid.*, p. 66.

or any material from an infected house, to come near them, and employed some trusty person to furnish them with marketing and other necessities from sources free from infection, escaped the disease, without exception. This was also the case with all the farmers attending the market, that cautiously avoided sleeping at or going into any of the chambers of the taverns or places where they put up.¹ Dr. Rush also says that "several families who shut up their front and back doors and windows, and avoided going out of their houses, except to procure provisions, escaped the disorder" (p. 101).

The military hospital and citadel of Barcelona were preserved, in 1821, through means of a strict seclusion.² The orphan asylum was treated in the same way, and with results equally satisfactory.³ Such, also, was found to be the case as regards the arsenal of Cadiz in 1800,⁴ and the prison of Malaga.⁵ "The dock-yard (of Gibraltar)," says Dr. Gilpin, "is situated under the line wall, and a drain very offensive to the smell passed through it, which has been covered over since the existence of the disease. Besides this drain, there is a stagnant piece of water, called the Lamber, in which boats are hauled up to repair; this, it is said, has three or four feet of mud in it, and receives a great part of the filth of the shores at the south, and emits a very offensive *effluvium*. Of five hundred persons who were confined there during the whole sickness (of 1813), there was not an instance of a person being attacked. It should be observed that these persons held their communication with the garrison through the medium of health guards from the Pratique office; and the same precautions were used as are pursued in cases of plague. These facts have been ascertained by the Pratique Master himself, a man of talents and great observation, and militate strongly against the idea that the disease originated in noxious effluvia."⁶ This place suffered equally with others in 1804, in consequence of the communication not having been cut off.⁷ The same occurred in 1814.⁸ Sir William Pym relates some striking examples of the advantage of separation as observed in Gibraltar. Speaking of an early epidemic, he informs us that Col. Fycers, of the Royal Engineers, with seven persons in his family, besides servants; Col. and Mrs. Darby, with Capt. and Mrs. Wilkinson, of the 54th regiment; Capt. Dodd's family, and Mr. Straith and family, were the only persons that cut off communication, and the only persons that kept clear of the disease, until the beginning of December, when Col. and Mrs. Darby, with Capt. and Mrs. Wilkinson, fatigued with their quarantine, and considering the disease so far got the better of as to be safe from its attack, returned into town to their quarters, where they all, as well as their servants (with the exception of Col. Darby, who had had the fever in the West Indies), were attacked with the disease, of which Capt. Wilkinson died.⁹ The following is also in point: "Two families in the town

¹ Condie and Folwell, p. 72; Currie, Fever of 1793, p. 8; *Ib.*, Fever of 1798, p. 68; Carey, p. 83.

² Pariset, Rep., pp. 145-160.

⁴ *Ib.*, Fever of Cadiz, p. 60.

⁶ Med.-Chir. Trans., v. 309, 310.

⁸ Guyon, Ann. Mar., p. 751.

³ *Ib.*, p. 44.

⁵ Arejula, p. 364.

⁷ Pym, p. 56.

⁹ Pym, 2d edition, p. 24.

cut off all communication, and were not attacked with the disease, with the exception of a servant-maid, who went to visit her sister, labouring under the epidemic. This maid was attacked, and passed the disease in a room apart from the others, under the most strict observation of some persons who previously had passed the disease. With this precaution, four individuals of the family of Trotabas avoided it. This house is not well ventilated, and is situated in the lowest part of the town. The other family which resided in the town was that of Mr. Martinez, composed of eight individuals, one of whom had passed the epidemic, and the others were not attacked on this occasion.¹ It was the opinion of Arejula, who had much experience in these matters, that the "separation of the sick from those in health, and avoiding all communication between one and another, is the only measure which, in such cases, can be depended upon to prevent the propagation of the malady."²

11. It is inferred that the fever of Philadelphia and other cities of temperate regions does not originate from domestic causes, but is derived from the importation of a contagious poison, from the circumstance that, when it prevails epidemically, other cities not far distant often remain exempt. Thus, in 1798, while this city suffered to an unprecedented extent, Baltimore, Georgetown, Alexandria, Wilmington (N. C.), Charleston, and Savannah, where at other times it has prevailed, remained free from its effects. "The disease," says Dr. Currie, "prevailed, and was attended with a degree of mortality unexampled in this country (considering the comparative smallness of the number that remained within the sphere of the contagion), in all the commercial towns north of Baltimore; whereas all the commercial towns south of Baltimore have escaped, Petersburg excepted, and the disease was introduced into Petersburg by the ship *Nestor*, from Philadelphia."

"Is it possible to believe that the late fever could originate and become epidemic on the high, dry, and sandy banks of Kensington? in the paved, clean, and open streets of Philadelphia, where every wind that blows has a free course? on the high and gravelly hills of Wilmington? on the elevated shores of York, washed on either side by the ocean's briny waves?—and not make its appearance on the flats of Baltimore or the putrid fens of Charleston and Savannah, where bile flows in incessant streams, and where the debilitated inhabitants faint under the insufferable blaze of a vertical sun?"³

While, in 1793, it raged with uncontrollable fury in Philadelphia, all the other towns of America remained free from an epidemic visitation. In 1794, it prevailed in New Haven, Baltimore, and partially in Philadelphia, but in no other of our sea-port towns. In 1795, New York, Baltimore, Norfolk, and Charleston, and in 1796, Newburyport, Haddam, Boston, and Charleston

¹ Pym, Answers to queries from the Royal Med.-Chir. Soc. of Cadiz, Edinb. Journ., xxxv. 33.

² Breve descripcion de la Fiebre Amarilla, p. 215.

³ Currie, Fever of 1798, pp. 140-1; Condie and Folwell, iv., Appendix.

alone suffered. In 1797, it spread in Philadelphia, as also in Norfolk, Baltimore, Bristol, and Providence, and spared all other parts of the Union; and in 1799 it appeared in Newburyport, New York, Philadelphia, Edonton, and Charleston, while Boston, Dover, Baltimore, and many other towns escaped. During the summer and autumn of 1805, Philadelphia and New York suffered severely from the disease, but Baltimore, Boston, Charleston, and other cities were free. Our city remained healthy, on the contrary, in 1817, as did also New York, Baltimore, and most other places, while New Orleans, Charleston, and Natchez were severely visited. In 1809, it was introduced into the town of Brooklyn (L. I.), when New York, only 800 yards distant, enjoyed the most perfect health.¹ In 1819, the disease was severe in New York and Baltimore, and slight in Philadelphia. In 1820, on the contrary, it assumed the character of an epidemic in the latter city, as also in Charleston, Savannah, New Orleans, and Middletown, while New York and other places escaped entirely. In 1822, New York and New Orleans experienced the disease in a violent form, and Philadelphia, Baltimore, &c., remained untouched. Since then, it has often prevailed, more or less extensively, in New Orleans, Charleston, Natchez, Mobile, Augusta, &c., and spared Philadelphia and other cities of the Union. In 1853, it prevailed extensively in New Orleans and many other parts of the South; it visited Philadelphia, but spared Charleston, Baltimore, New York, and other cities.

If we turn to the old continent, we shall find the same limited extension of the disease. In the year 1800, it was restricted to Cadiz and other cities of Andalusia. Leghorn, Gibraltar, and Malaga alone suffered in 1804. In 1814, Gibraltar was severely visited, while other cities on the Mediterranean escaped. In 1819 and 1820, Cadiz and other parts of Andalusia again were the only sufferers. In 1821, the fever was in great measure limited to Catalonia; and in 1828, Gibraltar alone suffered from its ravages.

How could all this be explained otherwise than on the supposition that the disease was introduced into those suffering places, while all others, far or near, escaped, owing to the absence of communication, direct or indirect, with tropical regions and with the former? Were this not true—were the disease the product not of a contagious and importable poison, but of a dis-tempered condition of the atmosphere, arising from domestic exhalations or otherwise—it ought to spread all over a large tract of country, and affect not one place or a few places, but all within the sphere of the contaminated region.

12. It may be remarked, in proof of the contagious character of the yellow fever of this city, that whenever the disease appeared among us, “it has been confined, for the first week or more, to the particular neighbourhood where it was first observed, or to persons whose occupations led them to frequent that particular part,” and that after the death or recovery of those first attacked, it has made a remarkable, though a partial pause—after which it has gradually spread or been conveyed into more distant parts, and has rapidly

¹ See Med. Reg., i. 101, 256; Hosack, Med. Pol. in Essays, ii. 28.

advanced with the advancing season.¹ This pause, which was noticed by Dr. Rush, and other correct observers, has been dwelt upon in an especial manner in reference to the question at issue, by Dr. Hosack,² who remarks that it occurs in every epidemic visitation of the fever, and who fixes its duration at from eight to twelve or fourteen days. A similar interval, it is added, has been frequently noticed in the history of the plague, as will be seen in Russel's account of the famous epidemic of Marscilles in 1820, when, from the 12th of July to the 23d, there was so decided a pause in the progress of the disease, that the popular apprehensions began to subside, and the physicians were reproached with ignorance in having mistaken ordinary fever for the plague.

It is further remarked, that whenever the yellow fever has been introduced into this or other cities of the United States, its first extension has always been slow and gradual—so much so, that its boundaries could be and were accurately defined by the boards of health at the various periods of the epidemic. “For awhile,” says Dr. Hutchinson, port-physician, speaking of the fever of 1793, “the fever was confined to the above-mentioned parts of the city (Water St., between Race and Vine), but the disorder is spreading, and now appears in other places, so that several are affected in other parts of Water Street, some in Second Street, some in Vine Street, some in Carter's Alley, some in other streets; but, in most cases, the contagion can be traced to Water Street.”³ On this subject, Dr. Hosack remarks that proofs of the same kind might be taken from the most authentic accounts of the yellow fever as it has prevailed at other seasons, and in other cities and other sea-ports of the United States, “proofs wholly irreconcilable with the assertions of those who have declared that the malignant yellow fever arises at distant and unconnected points; that no relation is observed between the source of the supposed contagion and the spreading of the disease to individuals or families, and who have maintained that there never was any successful attempt to trace, in irregular series, the propagation of it to any number of persons from the first case, or from any single point of infection.”⁴

13. The contagious character of the yellow fever may be inferred from the protection which one attack affords against further danger. That second attacks occasionally occur may be, and doubtless is true; but, as we have already seen, they are not frequently noticed, and when so, constitute exceptions to a general rule. This peculiarity is one attached to smallpox, and other diseases originating from and spreading by contagion; and the fact of its being observed to hold in regard to the yellow fever, affords a strong reason for viewing the latter disease as endowed with the like properties.⁵ “Every one is aware,” says Dr. Dickson, “of the immunity of the acclimated resident of localities where yellow fever is endemical, and of the extreme rarity of second attacks of this terrible pestilence—immunities well known and observed

¹ Currie, 1799, p. 11.

² Laws of Contagion, Essays, i. 307.

³ Rush, 1st ed., p. 19.

⁴ Laws of Contagion, note k, Essays, i. 361.

⁵ Fellowes, pp. xxii., xxiii.

in Europe as well as America. In character it offers a strong analogy with contagious febrile diseases, and is in equally strong contrast with ordinary endemics, such as remittents and intermittents. These, if they do not *prefer*, certainly do not spare the most habitual resident, but attack him again and again.”¹ Many of those who have taken notice of this peculiarity, have derived from it a proof of the contagious character of the disease.

14. By those who advocate the doctrine of contagion and importation it is remarked, that the appearance of the yellow fever has always coincided with the prevalence of the disease in parts of tropical regions and other places with which commercial and other relations are entertained. Thus, in regard to this city. In 1699 it prevailed in Martinique, as also at Barbadoes, St. Domingo, St. Christopher, and Guadaloupe.² In 1741, St. Domingo suffered severely.³ So also in 1747.⁴ In 1762, the disease raged at the Havana.⁵ At the time of the famous epidemic of 1793, the islands of Grenada, Dominica, St. Vincent’s, Jamaica, Tobago, Trinidad, Antigua, St. Christopher, St. Thomas, and Santa Cruz, as well as Demerara, were visited by the disease to an unprecedented extent.⁶ Our fever of 1794 coincided with the prevalence of the disease in Dominica, Guadaloupe, Martinique, St. Domingo, St. Lucia, Demerara, Havana, St. Thomas, and Santa Cruz; and that of 1797 with a similar occurrence at Martinique and St. Lucia.⁷ As regards the fever of 1798, it is remarked by the College of Physicians, “that the yellow fever, or *maladie de Siam*, prevailed in a very great degree in the different ports of Hispaniola during the last year, and more particularly in Port-au-Prince, Jeremie, and Cape Nicholas Mole; and that a very great number of vessels arrived at this city from those ports during the months of June and July, 1798.”⁸ It prevailed also in Boston, New York, Baltimore, Norfolk, and Alexandria. In 1799 it existed in Martinique, Baltimore, Charleston, and Norfolk. In 1802 (*Bally*, p. 77), it prevailed in Carthagen, Vera Cruz, Cayenne, and particularly at St. Domingo, besides New York, and other parts of this country. In 1805, the fever appeared and spread in Philadelphia; it prevailed with violence at Barbadoes and New York; and finally, it will be found that, in 1820, the ports of Cuba and other West India Islands, as also the city of Savannah, were sorely affected with the fever, while in 1853 most of the West India Islands, and a large extent of our southern States, were sorely visited by pestilence.

The same coincidence will be found to present itself between the fever of other parts of this country or of Europe and that of tropical climates, for whenever it has prevailed in either of the former—as New Orleans, Charleston, Baltimore, New York, Cadiz, Gibraltar, Barcelona, &c.—the occurrence

¹ Letter to Dr. Strobel, pp. 128–9.

² Labat, iv. 251–2; Bally, p. 36.

³ Pouppé Desportes, i. 110.

⁴ Ibid., i. 183.

⁵ Redman, Facts, &c., by College of Physicians, p. 21.

⁶ Chisholm, ii. 74.

⁷ Ibid., ii. 74, 114; J. Clark, Fever of Dominica, p. 1, &c.; J. Halliday, A Short Account of the Yellow Fever as it appeared in the city of the Havana, p. 7.

⁸ Facts, by College, p. 25.

is sure to have been preceded by the appearance of the disease in some of the West India Islands, while few, if any, instances have occurred where the fever of any one of the cities or towns of this country or of Europe has not been accompanied with a similar occurrence in some other place of greater or less proximity, and with which it held commercial or other intercourse.

Such being the case, there can be no difficulty—admitting the disease to be contagious and transmissible to distant parts—in foreseeing the probability of its having been introduced here or elsewhere from the West Indies, or other places previously affected, and of its deriving the infection from the former.

In addition to what has been said to show the connection between the existence of the yellow fever in various parts of this country and intercourse with sickly parts of the West Indies, it may be mentioned—and the fact has been regarded as at once “vanquishing the speculations of the adversaries of the doctrine of contagion”—that during the years in which our commerce with those ports was interdicted by the embargo, and during the subsequent years of war with Great Britain, when our communication with her possessions in those climates and countries, the usual seat of fevers of this description, was altogether suspended, the cities and towns along the whole of our seaboard were totally exempt from the fever of the tropics. And yet a recurrence to the meteorological observations of that period, will show a thermometrical range frequently higher than in those years when the yellow fever prevailed; that the rain fell as in ordinary seasons; and that, judging from the prevalent diseases, a general constitution of atmosphere and condition of localities, favourable to the generation of this malignant form of fever if such causes could engender it, existed as heretofore. As Dr. Hosack remarks,¹ the local circumstances of our seaports were the same, nay worse, for they were crowded with shipping; the made-ground remained unchanged in its condition; the same offensive vapours arose from our slips, our market-places, our privies, and from the soil where the latter did not exist—for the same authorities ascribe the same disease in one year to privies, and in another to the want of privies; yet, unfortunately for the doctrine of the believers and supporters of the domestic origin of yellow fever, the health of our cities remained undisturbed by the deadly visitor.

Proofs of the communicability of the disease from the sick to the well, whether in places infected, or in situations remote from these, numerous, positive, and satisfactory as they are, would be still more so, were not the virulence of the contagion like the efficient cause of other kindred diseases so far under the control of external influences as to be by them greatly blunted, or even, at times, completely destroyed. So true is this, that while recognizing in the disease the possession of a power of transmission from individual to individual—the result of a specific virus—and while denying its origin from the operation of local or domestic causes, some writers maintain that facts show

¹ Hosack, *Med. Police Essays*, ii. 29, 30.

that the contagion, though inherent in the fever, remains ordinarily in a latent state, and requires, in order that it may display itself, the concurrence of a peculiar condition of atmosphere. In the absence of this peculiar atmosphere, patients or their effects may be approached and handled with impunity; during its existence, the contagious effluvia issuing from a diseased body, or carried in fomites, acquire greater power or new victims, and become a source of danger. "We learn from past observation," says Dr. Currie,¹ "that a certain condition of the atmosphere must concur with the matter of contagion, before the disease can be communicated from one to another, or be propagated by contagion (which is the only way it can be propagated). What that precise condition of the atmosphere is, which favours the communication of the disease from one to another, has not yet been unequivocally ascertained; but from the circumstance of the disease not being communicated in high, dry, open, and airy situations in the country, during the season when it is most highly so in the city, which is a fact, with a few extraordinary exceptions, well established, it amounts to a degree of probability almost equal to certainty, that the atmosphere where the disease prevails, and is readily communicated from one to another, must not only be of a certain temperature, but at the same time calm, confined or motionless, and perhaps contaminated with exhalations from putrefying vegetable or animal substances, or from living animal bodies deprived of fresh and purer air in close and confined situations, which render it a fit conductor for the matter of contagion, at the same time that the air thus contaminated, by impairing the inherent powers of the body, predisposes it to be more easily acted upon by the contagion, not only of pestilential, but of ship, hospital, jail, or nervous fevers." In a subsequent part of the same work, Dr. Currie says that his observations incline him to believe "that although the yellow fever is never generated in this country, it is communicated from one to another more readily and certainly when the atmosphere is replete with putrid exhalations, than when it is more pure or free from such exhalations."²

The modifying effects of such agencies, by which the contagious power of the yellow fever is in part, or completely concealed, are not admitted by this distinguished physician only; but will be found equally recognized by numerous writers of this and other countries, who nevertheless deny, in most positive terms, the possibility of the disease originating from the operation of domestic causes, or of its spreading through any other agency than the one in question. There are not wanting facts, indeed, to show, according to some, that though the disease is always of exotic origin, it never exhibits contagious properties, except in an atmosphere rendered impure by imperfect ventilation, or by exhalations of the kind mentioned.

The late Dr. Griffiths, of this city, entertained an opinion somewhat similar to this; for while admitting that the fever was not apparently contagious

¹ Fever of 1799, p. 11; Barton's Med. and Phys. Journ., ii. pt. i. 43.

² Ibid., p. 77; see also Fever of 1793, pp. 65, 69; Fever of 1798, pp. 66, 67.

in the West Indies, he thought it became so during the passage from the Antilles to the United States, from the crowding of the vessel and the want of cleanliness and defective ventilation. Hence, he denominated it ship fever of tropical climates. When once introduced in our ports, it was propagated, he thought, from individual to individual, from house to house, and thus became general.¹ The fever which prevailed on board the *Eclair* in 1845, may be cited in point. It was not primarily contagious, and therefore not the true Bulam fever. It acquired that quality in the ship. Such a disease could not have been contracted at Seabar; for in that case, other vessels visiting that part of the station would have been similarly affected. Nor at Sierra Leone, for some of the many Europeans, from the numerous shipping there, would, in all probability, have been attacked with a fever manifesting contagious qualities.

While the *Eclair* was there, there was nothing unusual either in the amount or nature of disease in the colony. The same may be said of the Gambia. Assuredly, as Dr. McWilliams remarks, the remittent is quite destructive enough of human life; and in the late Niger expedition, where its fatal effects were manifested in a terrible degree, there was no reason to believe that its spread was due to contagion. The whole of the surviving sick were landed at Fernando Po, after the ship got clear of the Niger, yet none of the residents there suffered in consequence, although many officers and men died at Clarence Cove. Now, why did this malignant disorder rage on board the *Eclair* and not in other vessels that were with her? Simply because her circumstances were peculiar, and it is entirely to this peculiarity and unwonted combination of circumstances that the contagionality of the fever with which her crew was affected, was due. There is no proof that the fever in question was in any degree contagious before the vessel reached Boa Vista, and we have a right to look for proof both at Sierra Leone and the Gambia, where the evidence is against contagion. At Boa Vista the reverse was the case; for the whole history of the progress of the fever subsequently to the landing of the crew on the small island proves it to have then possessed highly contagious qualities. "I would say, then," Dr. McWilliams adds, "that the contagious properties which marked the *Eclair* fever at Boa Vista were acquired or contingent, and not primarily or essentially belonging to it."²

Not very different is the opinion of Dr. Hosack, of New York, one of the principal champions of the contagious character and exotic origin of the fever in this country. By this distinguished physician, the yellow fever is considered "to be the disease of the northern man suddenly removed to the tropics; in other words, the effect of high temperature upon the unacclimated constitution, and this frequently aggravated by other exciting causes, as violent exercise, ardent spirits, free living, exposure to night air, &c." "This disease," he says, "is not limited to Siam, in the East Indies, as its birth-

¹ Document furnished to Dr. Chervin, *Opinion des Méd. Am.*, p. 105; Report of the Academy of Medicine, p. 16.

² Report on the Fever at Boa Vista, p. 110.

place, as has been believed by some writers, nor to the coast of Africa, as urged by others, but extends to the tropics in general." When the yellow fever, thus generated, attacks the individual, it frequently destroys without being communicated to others surrounding him, that is, in a pure air where attention is paid to ventilation, personal cleanliness, and the removal of all excrementitious and foul materials from the apartment of the sick; but when an individual is attacked in a crowded and filthy dwelling, as on board of ship, in a garrison, &c., the disease assumes such a degree of malignity, that the excretions from his person, mingling with the virulent materials composing the offensive condition of atmosphere around him, readily communicate the same disease to those who respire such atmosphere, or are exposed to the poison by contact with, or near approach to the sick, the wearing apparel, or other material impregnated with the excretions of the diseased body.¹

Sensible of the importance of these views—which he entertained from an early period in his professional life, but publicly taught after they had been suggested in this city by Dr. Currie and Dr. Barnwell—Dr. Hosack, in a letter to Dr. Chisholm on the classification of contagious diseases, written in 1808, and originally published a year after, in an early volume of the *Edin. Med. and Surg. Journ.*, again issued in 1814 in his additional observations on the laws of contagion, as well, indeed, as in almost every one of his essays, enlarged on the subject.² He placed the yellow fever, together with the plague, typhus (jail, ship, hospital, and lake fever), and dysentery, in a separate class, distinct from those diseases which are communicated exclusively by contact and under all circumstances—as itch, syphilis, and the like; as also from another set of diseases which are transmitted both by contact and through the atmosphere; and in all seasons, in a pure and impure condition of air, though more particularly the latter. The diseases of this separate class, Dr. Hosack remarks, "are only, in general, communicable, through the medium of an *impure* atmosphere; in a pure air, in large and well-ventilated apartments, when the dress of the patient is frequently changed, all excrementitious discharges immediately removed, and attention paid to cleanliness in general, these diseases are not communicated, or very rarely so, from one [individual] to another. But, in an impure air, rendered so by the decomposition of animal and vegetable substances, as takes place in low marshy countries, or by concentrated human effluvia, as in camps, jails, hospitals, or on shipboard, they are rendered not only extremely malignant and mortal in themselves, but become communicable to others who approach the sick, and breathe the same atmosphere which has become *assimilated* to the poison introduced, insomuch that *the same specific disease* is communicated, whether it be the *plague, yellow fever, typhus, or dysentery*."³

¹ Letter to Dr. Townsend on the Nature and Treatment of Yellow Fever, *Med. Essays*, iii. 419, 420.

² See the Letter, *Edin. Med. and Surg. Journ.*, v. 427; *Med. Reg.* ii. 14; *Laws of Contagion*, 4to., p. 6; *Ibid.*, in *Med. Essays*, i. 265; *Med. Police*, p. 6; *Ibid.*, in *Med. Essays*, ii. 14; *Ed. Med. Journ.*, xii. 357.

³ *Essays*, i. 258, 259.

This class of diseases, therefore, like the former, has a law peculiar to itself; *i. e.*, the diseases composing it are communicable or otherwise, the difference depending upon the condition of atmosphere in which they occur or are introduced; whereas, those of the second class are conveyed from person to person, through a pure as well as an impure medium.¹

Palloni insists on the fact that the "infection of that fever was so feeble, that pure and renewed air decomposed it at a small distance from the patient." It was only in a stagnant air, filled with animal exhalations, that it became manifest. Hence, it was only found in the dirtiest and less ventilated parts of the city, and in the houses of the poor: "L'infezione di questa febbre è di tal indole, che l'aria pura, a rinnovata ne decompone il fomite a piccola distanza del malato: all'opposto un'aria stagnante, e ripiena di esalazioni animali diviene facilmente un veicolo per esso. Egli è perciò che avunque e insorta questa malattia si è veduta specialmente infierire nelle strade più sudice, è meno ventilate della città; ed in particolar modo nelle case dei poveri, tra i quali, oltre la indisposizione individuale necessaria all'azione di qualunque contagio, la ristrettezza delle stanze, la poca pulizia delle medesime, e le molteplicità degli abitanti ravvicinano i punti del contatto, e facilitano l'infezione."²

A similar view of the necessity of an impure atmosphere to call into play the contagious character of the disease, and of its exotic origin, is advocated, not only by the writers mentioned, but likewise by Seaman (*Repos.*, ii. 331), Monette (pp. 55, 61), Strobel (207), Whitmarsh (in Bancroft's *Seq.*, 166), Barnwell (374), Townsend (225), and others, and is not very different from that entertained in reference to the plague by Mead and subsequent writers. "A corrupted state of the air is, without doubt, necessary to give these contagious atoms their full force; for otherwise it were not easy to conceive how the plague, when once it had seized any place, should ever cease but with the destruction of all the inhabitants; which is readily accounted for by supposing an emendation of the qualities of the air, and the restoring of it to a healthful state capable of dissipating and suppressing the malignity."³

But, whatever be the limitation thus set to the contagious character of the yellow fever, founded on its prevailing, as a general rule, only in crowded impure places, and principally among the poor who inhabit such localities and are inattentive to the precepts of cleanliness, and never in the country, &c., this character is fully admitted to exist and to show itself whenever circumstances favourable to its manifestation present themselves. It is inherent in the disease, and forms part and parcel of its elemental properties. By many, cases are even adduced in which, as we have seen, such a manifestation has occurred in the pure air of the country, and where it might least have been expected; and by all, the domestic origin of the fever is denied, and its tropical birth warmly supported.

¹ Essays, pp. 259, 260, 289, 290.

² Osservazioni Mediche sulla Malattia Febrile Dominante in Livorno, &c., p. 29. See also Edin. Journ., ii. 88, and Med. Rep., viii. 426.

³ Mead on the Plague, in works, 8vo. ed., p. 192.

But the doctrine of the contagion of the yellow fever receives support from others than those who maintain the disease to retain always and under all circumstances its power of transmission from the sick to the well, or those who, though firmly convinced of the permanency of that power, and discarding all idea of local origin, so far as regards the fever of temperate, and often of warm climates, think that it requires for its development and transmission from one body to another the intervention of an impure air. The belief is advocated by many who, so far from admitting the necessity of an exotic origin, refer the disease to the action of local causes, but think, at the same time, that under peculiar circumstances, in an impure atmosphere, in crowded rooms, and in low, damp, and filthy places, the disease, though in its natural condition or fundamentally devoid of contagious properties, may and does often acquire those properties so as to be communicated to those who come within the sphere of its influence. In other words, they think that to the morbid processes going on in the systems of individuals labouring under the yellow fever, another is superadded, through the agency of an impure atmosphere, and that by means of this new process a poison or virus, which did not exist before, is produced, which when formed assumes the office of the original cause of the disease, imparts to the latter the power of propagating itself, independently of that cause, to those who approach individuals so affected. This doctrine, which, as may readily be seen, is somewhat allied to the former, has received the denomination of doctrine of contingent contagion. Its advocates are perhaps more numerous than those of the doctrine of exclusive or limited contagion, already adverted to; while its origin may be traced to an early period in the history of the fever. Desportes (i. 40), if a contagionist at all, was one of that class, and so were Lind, Dancer, and Gilbert; while the writings, reports, and certificates of Humboldt, Bally, J. Warren, Bayley, and others, in this country; of Ralph, Lind, Lefoulon, Leblond, and many more, in tropical climates; and of Amiel, Hennen, and Burnett, in Europe; besides a large number of high authorities, to whom reference will be made in a subsequent chapter, will show to what an extent the doctrine has been advocated. In this city, it was upheld, so early as the year 1766, by Dr. Th. Bond, who, in the introductory lecture already several times referred to, after remarking that some of the epidemics he saw originated from local causes, says: "The contagion of these malignant fevers lies in the air confined and corrupted by a neglect of cleanliness about the helpless sick, and is harmless without such aid."¹ It was warmly espoused by Dr. Rush, and adhered to by that eminent physician from 1793 to the period of his retirement from the ranks of the contagionists. It was in like manner adopted, in 1793, by Dr. Leib, Dr. Foulke, Dr. Hutchinson, and Dr. Redman, and, some years later still, constituted the creed of the Philadelphia Academy of Medicine.²

This conversion, under certain contingencies, of a non-contagious to a

¹ N. A. Journ., iv. 271.

² See the letter of thirteen physicians (who afterwards formed the Academy) to the Governor; Rush, *Fever of 1797*, pp. 46, 47.

contagious yellow fever, is, according to its advocates, established by positive facts and analogical inferences; for while the disease must, for reasons which will presently be laid before the reader, be admitted to arise from the operation of local causes, and independently of the transmission of a contagious principle, many facts of an indisputable character are on record, and present themselves at every recurrence of the fever in an epidemic form, showing, as they maintain, the power of transmission alluded to. Nor is this all. The reality of the conversion is, they say, rendered probable, and the statement of its recurrence must cease to appear unfounded, when we recur to the example of other diseases which arise from terrene or atmospheric causes, but which nevertheless assume generally, or at times, the contagious character. Every medical reader knows full well that typhoid or typhus fever is the product of such local causes—vitiated secretions and excretions, under particular conditions of atmosphere and locality, &c.—and yet the power of transmission of the disease thus formed is fully recognized.¹ The oriental plague—once supposed to be always contagious, and never the product of local causes—has, in modern times, been stripped of so highly pestilential a character, and is admitted to be oftener non-contagious than contagious; but though generally, if not invariably, the effect of the agency of local causes, it is found, when spreading extensively and assuming a highly malignant character, to acquire the power of transmission from one person to another.²

The epidemic fever with lymphatic swelling, which arose in Rajputana, in India, in 1836, and to which the name of Pali plague was applied, commenced at a small village called Taiwali, near Pali, in Marwar. Its importation could not be proved, or, rather, was completely disproved. It arose evidently from domestic causes, and yet there were evident proofs of its being possessed of contagious properties. The latter were, apparently, originally produced by certain causes, which being removed, the poison ceased to be contagious, or its reproduction was so reduced as no longer to be appreciable.³ The sweating disease was evidently in former days, and continues to the present time, to be produced by local causes, atmospheric and terrene; and yet it was regarded by Bacon, Caius, and Friend, as endowed with contagious properties.⁴ Erysipelas may be cited as an example of a disease which, arising from local or atmospheric causes, assumes at times contagious properties.⁵ So

¹ See *An Account of a Contagious Fever which occurred among the Danish and American Prisoners of War at Chatham, in 1813 and 1814*, by Sir W. Burnett (Lond., 1831); Blane, on Seamen, pp. 237–9; *Ib.* Diss., i. 305; Johnson, p. 21.

² See *Report to the Academy of Medicine*, by Prus, pp. 73, 127 (1818); Hancock, p. 204; Desgenette, Fodéré, Pugnet, Cholet.

³ *Brit. and For. Med.-Chir. Rev.*, i. 376, note, from Rankin's Report.

⁴ Caius, *De Ephemera Britannica*, p. 8, &c.; *Ib.*, *A Counsell against the Sweat*, in Appendix to Hecker, pp. 363–4; Hancock, p. 266; Hecker, p. 216; Rayer, p. 376.

⁵ Williams on Morbid Poisons, ii. 61; Blane, Diss., i. 318; *Ib.*, ii. 257; *Ib.*, *Med.-Chir. Trans.*, xxii. 213; Stevenson, *Med.-Chir. Trans.* (Edinb.), ii. 128; Wells, *Trans. of a Society for Improvement of Med.-Chir. Knowledge*, ii. 213; Pritchard, *Rep. of Bristol Infirmary*, p. 91; Gibson, *Med.-Chir. Trans. of Edinb.*, iii. 94; Travers, *Constitut. Irrit.*, ii. 7.

may dysentery,¹ influenza,² cholera,³ puerperal fever,⁴ the bilious jungle fever of India,⁵ the simple gastric fever of writers,⁶ cynanche tonsillaris,⁷ croup,⁸ catarrh,⁹ consumption,¹⁰ certain forms of intermittent fever,¹¹ and the remittent bilious fever of this country, of Europe, and elsewhere.¹² All these, as well as the breakbone fever,¹³ hospital gangrene,¹⁴ porrigo,¹⁵ and other diseases not originally contagious, have been found at times to assume that character; and surely what has taken place in them may, without impropriety, be admitted to occur in the yellow fever.

The following passage from Dr. Rush's first publication on the fever of 1793, will show the state of his mind on the subject before us at that time, and some of the facts upon which, in addition to the cases of communication adduced, the doctrine of contingent contagion was then, and continues to be, upheld in this and other cities: "If anything could surprise me, after reading the report of the College of Physicians that our late fever was imported, in spite of every possible evidence to the contrary, it would be the opinion, which was delivered publicly by some leading members of the College, that no fever produced by vegetable putrefaction and exhalation had ever been contagious. It is scarcely possible to open a practical book upon medicine without meeting with facts which establish a contrary opinion. The fever generated by putrid

¹ Hildanus, Sennertus; Blane, *Dis. of Seamen*, pp. 113, 244; *Ib.*, *Diss. Int.*, i. 324; Pringle, pp. 20, 235, 256; Cheyne, *Dublin Hosp. Rep.*, iii. 12, 19; Pitcairn, in Cheyne, p. 9; Bracker, *ib.*, p. 7; Cullen, ii. 55; Bateman, *Dis. of Lond.*, p. 114.

² Hecker, p. 216; Blane, i. 316; Hancock, p. 346.

³ Blane, i. 320; Report of College of Phys. of Lond., pp. 219, 222; Gray, *Med. Communications*, i. 65; *Trans. College of Phys.*, iii. 60; Hamilton, *Med. Soc. of Lond.*, ii. 438; Haygarth, *Of the Manner in which the Influenza of 1775 and 1782 spread by Contagion in Chester and its Neighbourhood*; Du Gard, Hall, Yeo, Burroughs, Bond, Hobbs, and Harness, in *Med. and Phys. Journ.*, x. 216-291; Fieldhouse, *Med. Soc. of Lond.*, vi. 296-7; Bardsley, *ib.*, vi. 267-9; Carrick, *Ann. of Med.*, viii. 410; Nelson, *ib.*, viii. 424; Bryson, *Ann. of Influenza*, p. 361.

⁴ Douglass, *Dublin Hosp. Rep.*, iii. 144; Gordon, *Epid. Fev. of Aberdeen*, p. 63; Gooch, *On the more important Diseases of Women*, p. 75; Robertson (of Manchester), *Med. Gaz.*, 1831, No. 214; Copland, iii. 506; Churchill, *Sydenh. Coll.*, p. 41; Condie, *Trans. of Phil. Coll. of Phys.*, July, 1842; Ramsbotham, *Med. Gaz.*, May, 1835; Blundell, *Lectures on Midwifery*, p. 395; Iöry, *sect.* 747, p. 702; Phil. Pitt Walsh, p. 18, &c.; Armstrong, p. 73; Robertson, p. 439; McClintock and Hardy, p. 29; O. W. Holmer, *Puerperal Fever as a private pestilence*; Boston, 1855.

⁵ Johnson, in *Tr. Cl.*, pp. 81, 123; Nicholl, in Johnson, p. 101; Balfour, p. 74.

⁶ R. Jackson, *Sketch*, i. 35; Fordyce, p. 65, *Am. ed.*; Lind, pp. 35, 36; Tweedie, *Cycl.* ii. 192; John Clark, i. 123-4; Hancock, p. 339; Bateman, p. 12; Short, i. 303; Pritchard, *Fev. of Bristol*, p. 94.

⁷ Hancock, p. 346.

⁸ *Ibid.*

⁹ Hecker, p. 216; Williams, ii. 686.

¹⁰ Hancock, p. 348.

¹¹ Lancisi, *De Nox. Palud. Effl.*, pp. 151, 158, 267; Cleghorn, p. 132; Short, i. 303; Audouard, *Rech. sur la Cont. des F. Interm.*, 1818.

¹² Letter of Acad. of Med. of Philadelphia, already cited, Rush, *Fev. of 1797*, p. 46; Denman, *Fever of the Medit.*, *Med.-Chir. Trans.*, vi. 317; Hamilton, *Obs. on Marsh Fev.*, p. 39; J. Clark, on *Long Voy.*, i. 151.

¹³ Ruan, *Med.-Chir. Trans. of Edinb.*, iii. 459.

¹⁴ Boggie, *ib.*, iii. 4.

¹⁵ Bateman, p. 167; Williams, ii. 15, 196, 399; Heberden, p. 102; Plumb, p. 52.

cabbage, mentioned by Dr. Rodgers, and by putrid flax, mentioned by Dr. Zimmerman, were both contagious. Dr. Lind ascribes the yellow fever everywhere to marsh or putrid vegetable exhalations; and this fever, we know, spreads by contagion. Dr. Lind, Jr., establishes the contagious nature of the marsh fever of Bengal in the year 1762. I shall transcribe his words upon the subject: 'Although marsh miasmata,' says he, 'first bring on the disease, yet contagion presently spreads it and renders it more epidemic. Thus, the Drake Indiaman continued free from the disorder for two weeks together, when she had no communication with other ships; whereas, as soon as the disorder was brought on board, many were seized with it, within a few days, in such a manner as to leave no room to entertain the least doubt concerning its pestilential nature.'

"Dr. Clark mentions a contagious malignant fever from marsh miasmata, which prevailed at Prince's Island in the year 1771, and which afterwards infected the crew of the Greenville Indiaman. The contagious pestilential fever in France, so accurately described by Riverius, was produced by an exhalation from putrid vegetables, particularly hemp and flax. Even intermittents, the most frequent and the most numerous offspring of marsh exhalation, are contagious. Of this there are many proofs in practical authors. Bianchi describes an intermittent which was highly contagious at Wolfenbuttle in the year 1666. Dr. Clark mentions a number of cases in which this mild species of fever was propagated by contagion. Dr. Cleghorn has established the contagious nature of intermittents by many facts. After mentioning numerous instances of their having spread in this way, he says: These tertians have as good a right to be called contagious as the measles, smallpox, or any other disease. The United States, in common with other countries, have in many places exhibited proofs of the contagious nature of fevers produced by putrid vegetable exhalations. The bilious fever which prevailed in Philadelphia in the year 1778 was eminently contagious; so were the bilious fevers which prevailed during the last autumn in Weatherfield, Harrisburg, and on the South Branch of the Potomac" (pp. 160, 161, 162).

It has been stated that no argument can be founded on the fact that the disease, when actually imported, does not spread; for the same remark applies to diseases positively known to be contagious—smallpox and other eruptive fevers. The former is brought to our shores every year, and yet for many years dies away with the person who brought it; so also of scarlet fever. Of this disease we see every day solitary cases. In the Antilles, the latter cannot be transported at all. We are told that it is unknown there; and the authority of Rochoux is appealed to for the fact, that the natives of those islands must live in France eighteen months or two years before they can become so acclimatized as to acquire a susceptibility to the disease.¹

The opinion of the contagious character and power of transportation of the yellow fever is not sustained only by the facts and arguments which have been laid before the reader. Many other facts of like import have been adduced, while both points in question are supposed to be settled by the

¹ Nott, N. O. Journ., iv. 591.

history of the epidemics that have occurred in various parts of temperate climates, and of many of those of tropical regions, where the disease is said to have been introduced by means of vessels from distant infected ports, or through the agency of individuals labouring under or convalescing from it, or of fomites—clothes, merchandise, &c.—and to have spread from the sick to those around. On this subject, the records of the disease, as it has occurred in this city at various periods, from 1699 to 1854, inclusive, as well as in other parts of this country and in Europe, have been appealed to as furnishing ample and convincing proofs, some of which will be adverted to in the closing portion of this volume.

CHAPTER XIII.

FACTS AND ARGUMENTS AGAINST CONTAGION.

IN the two preceding chapters I have, as briefly as possible, stated the views formerly and at present entertained by a respectable number of physicians regarding the origin and mode of propagation of the yellow fever of this and other countries. In doing so, I have enumerated the principal facts and arguments by which the doctrine of contagion, in its application to the disease in question, has been and continues to be upheld. I need scarcely remark that, entitled as such facts and arguments are to the serious consideration of every medical inquirer, the opinion founded upon them has not been left in undisputed possession of the field. While by some among such physicians the disease is regarded, in temperate regions, as contagious and invariably the offspring of importation from tropical climates; while by some nearly the same views are entertained respecting the mode of propagation of the fever and its frequent transportation from place to place in the latter regions also; and while by others, who view it as endowed with the power of self-propagation, it is thought, nevertheless, to arise from the action of domestic causes, not a long time elapsed before sentiments of an opposite character began to be promulgated. In this city, for example, physicians were found—few in number, it is true—who, so early as the period of the memorable epidemic of 1793, ventured to declare not only that the causes of the fever were to be sought within the limits of the localities infected, and should in no case be referred to foreign parts, but that it was, under all circumstances, devoid of contagious properties. Hence arose among us the doctrine of non-contagion, which, gradually gaining proselytes, especially among those who had originally disbelieved the foreign origin of the fever, finally succeeded in supplanting almost completely the views of its adversaries.

The history of the doctrine of non-contagion and non-importation in reference to the fever of this city—its rise, progress, and issue—bearing, as it does, on the great questions of the origin and mode of propagation of the

disease everywhere, is deserving of the attention of the profession, or of those who, from taste, curiosity, or duty, are led to investigate the subject; for, to whatever conclusion experience may lead a large body of physicians of acknowledged intelligence and attainments in reference to matters of this kind, such conclusions cannot fail to aid in the settlement of the point at issue; and if, in the case before us, we find the doctrine in question gaining the ascendancy in proportion as the field of observation enlarges, we cannot greatly err in viewing it as entitled to our regard. Arising from small beginnings, opposed by all anterior to 1793, and numbering among its defenders at the period of the epidemic of that year but few names of weight, it gradually gained proselytes in 1794, 1797, 1799, and 1805, and finally became the predominant doctrine. So decided and complete, indeed, was the change, that Dr. Chervin, during his visit here, some thirty years ago, could not find more than four or five contagionists among us, out of upwards of two hundred physicians, the greater number of whom had passed through the epidemic of 1820, while many had seen the disease in 1798 and 1805. Since that period, the decease of several of those who furnished him with certificates in favour of the contagious character of the fever—Drs. Griffiths, Park, James, and Parrish—has so far reduced the list of the advocates of the latter doctrine, as to render it doubtful whether a single physician of note can be found among us disposed to espouse decidedly that side of the question. In view of the extent of this revolution, and of the interest it naturally excites, it would be no loss of time to devote some pages to a detail of the steps by which the result has been obtained; but want of space admonishes me of the necessity of being brief on the subject.

Origin of the Doctrine of Non-contagion among us.—By whom the opinion of the non-contagiousness of the fever of this city was originally started, has been a matter of contention among medical writers. By some, the honour has been assigned to Dr. Rush, who was long viewed as not only the father of the doctrine of non-importation in this country, but as the parent of that of non-contagion also. Whether he himself wished to be held up as leading the way in reference to this question, may well be doubted, inasmuch as he not only says nothing himself calculated to lead to such belief, but makes an allusion to those who preceded him in advocating the doctrine in question. Be this, however, as it may, it is plain that this philosophical and truly great physician is not entitled to the latter distinction; for he himself acknowledges that, so late as 1798, he was a believer in contagion, while the contrary opinion was upheld by others long before that period.

The late Dr. Potter, of Baltimore, in a valuable essay on contagion,¹ remarks that, in October, 1793, he wrote to Dr. Rush on the domestic origin and non-contagious character of the epidemic of the Eastern Shore of Maryland, and expressing the opinion that fevers arising from vegetable decomposition are not contagious, suggested the idea that the yellow fever, which then raged in Philadelphia, was also devoid of that property. He further states that, in the summer of 1795, believing that he was at that

¹ Preface, p. ii.

time the only person in America who denied the contagion of the disease, and that the opinion could be sustained by facts, he proposed to select this question and maintain the negative in an inaugural thesis, but desisted from doing so at the earnest solicitation of Dr. Wistar. The claim of priority here set forth is explicit, and if sustained by something more than a letter, which until 1818 was not made public, would entitle Dr. Potter to the credit sought by this announcement of its contents. That Dr. Potter may have attached value to the fact of his having entertained such views at so early a period, seeing the turn which the professional mind took on the subject, was naturally enough to be expected; but as such opinions were, during a space of more than twenty years, allowed to remain concealed from the knowledge of every one but his correspondent, his claim must yield to that of others who were less tardy in stating their views publicly. On that score, he is on a footing with Dr. Monges, and several other French physicians of this city, who, disbelieving the doctrine of contagion during our earliest epidemics, either did not write at all, or did so at a very late period.

Dr. Potter, as if aware of this, does not assume for himself all the credit as the leader in the change of views which took place shortly after. But unwilling, as it would seem, to allow that they originated in this city, he claims the priority of publication for one of his colleagues in the medical school of Baltimore. "From the fever of 1793 at Philadelphia," he says, "to that of Baltimore in 1797, the contagion of yellow fever was not called in question till the paper written by my colleague, the learned professor of anatomy (Dr. Davidge), was issued in the *Federal Gazette*, of this city (Baltimore), on the 30th November of that year. This was the first printed enunciation in the United States in the negative, protesting against an unphilosophical opinion which Dr. Davidge never believed. This communication was afterwards enlarged, and embodied in a pamphlet, and has never been answered till this day." In the pamphlet in question, Dr. Davidge himself lays down his claim in emphatic terms: "So far as accessible authority will justify a conclusion, the author is the first writer of America who has publicly advanced the opinion, that the yellow fever is not a contagious disease; and he would feel no inconsiderable degree of pleasure in perceiving many of the most impassioned opposers of his sentiments among its warmest admirers, were it not that these learned gentlemen, with his treatise in their hands, pretend and assert—with how much justice may be left to the reader—that the discovery is the result of their own observations."¹

But claimants to the merit of peculiar sagacity in enlightening the minds of the profession here to the truth of the views in question are not limited to physicians of our own country. Some have started up in Europe. Thus, we find Dr. Charles Maclean, whose name has become identified with the subjects of epidemics, non-contagion, and quarantines, unhesitatingly ascribing

¹ An Essay on the Autumnal Endemial Epidemic of Tropical Climates, commonly called Yellow Fever, &c., in volume of Physical Sketches, &c., p. 77 (note). The essay was originally published in 1798, and the reprint from which I quote appeared in 1813.

the belief in non-contagion, which took place here about the year 1798, to the influence of a dissertation by him on the sources of epidemic diseases, which originally appeared at Calcutta in 1796, and was reprinted in Philadelphia in 1797.¹ "If," he says, "it shall appear that the illustration which I originally employed to disprove contagion as the cause of pestilential diseases was fitting—that a similar one had not been previously used by others—and that no arguments essentially different, or more convincing, were subsequently adduced, it will inevitably follow that to the circulation of my opinions and proofs in America, is due the principal credit of having produced this conviction."²

Plausible and explicit, however, as the claims set forth by Dr. Davidge and Dr. Maclean may appear, they are easily disproved by a reference to writings published in Philadelphia soon after the epidemic of 1793, and having for their object a description of that awful calamity. We might, perhaps, award some degree of merit on the subject in question to Dr. Nassy, who, in an essay published in 1794, gives an accurate description of the disease; for though, in alluding to the causes of the latter, he speaks of contagion, it would appear from the context that he used the word in the sense we now attach to the term infection. But whatever may be thought of the true claims of Dr. Nassy in relation to this matter, I shall not urge them in consequence of his having used the word contagion. Setting him aside, however, we shall find that to Dr. Deveze is due the honour of being the first in Philadelphia to proclaim publicly, and, in the clearest possible manner, the non-contagious character of the yellow fever. Dr. Deveze arrived in this city from the West Indies at the commencement of the epidemic of 1793, and was soon after placed at the head of the hospital opened at Bush-Hill, where, as well as in the city, he had ample means of investigating the causes of the disease. In 1794—one year before the *intended* thesis of Dr. Potter—three years before the newspaper article to which he alludes—two before the publication of Dr. Davidge's pamphlet, and three before the American reprint of Dr. Maclean's work alluded to above—that distinguished physician published a description of the memorable epidemic of 1793.³ In this essay, which, I regret to say, has seldom, if ever, been alluded to—far less quoted—by our writers, the author boldly proclaimed the non-contagious character and the domestic origin of the disease, assigning facts analogous, and adducing arguments similar, to those adduced in subsequent writings. "It will, perhaps, appear strange to many," he says, "that contrary to the public opinion, I dare assert the malady was not contagious. But the truth being unalterable, could

¹ A Dissertation on the Source of Epidemic and Pestilential Diseases, in which it is attempted to prove, by a numerous induction of facts, that they never arise from contagion, but are always produced by certain states, or certain vicissitudes of the atmosphere. Published in a volume, with "A View of Science of Life," by W. Yates and Chas. Maclean; and "A Treatise on the Action of Mercury upon Living Bodies," by the latter.

² Epidemic Diseases, i. 72-3.

³ An Inquiry into, and Observations upon the Causes and Effects of the Epidemic Disease which raged in Philadelphia from the month of August till towards the middle of December, 1793. By Jean Deveze. (In French and English.) Philadelphia, 1794. pp. 145.

I refuse to believe circumstances proved to me by continual observation? It is to the well-informed, and those uninfluenced by party, I leave to pronounce upon the proofs I will give, and which seem to me to establish my opinion in an incontestable manner" (p. 12). After explaining what he means by contagion by distance and contagion by contact, he says: "It results from this definition, that maladies contagious at a distance are communicated to other bodies surrounding the infected persons, by breathing the same air, which alternately passes into the lungs of those who live in the same place, particularly in close apartments; and then the malady may very well be characterized under the title of contagious disease. But if, in an epidemic disorder, those who continually touch the sick—those who, without any preservative, listen only to their courage and love of humanity, give themselves up to the care of the diseased, live in the midst of them, and breathe the same air—if these persons are not infected, it is clear the disease is merely epidemic, and not contagious. This was precisely the case in that we are now speaking of" (p. 24).

These extracts from a work which ought to have been known to those of the physicians of this country who have since undertaken to investigate the causes of the yellow fever, and was far from meriting the silence with which it has been treated by our non-contagionists, establish the claim of the author in regard to priority of announcement, and show most conclusively the error of Dr. Potter in pointing out the publication of Dr. Davidge as the first printed enunciation in the United States of the non-contagiousness of the fever in question.¹ Be this as it may, although no notice was taken

¹ The pretensions of Dr. Davidge on the subject are the more extraordinary and unpardonable, as the claim is made by him not only in his *Essay on Yellow Fever*, but, at a much later period, in the appendix of his edition of Bancroft's work on the disease, the text of which contains, at p. 258, a reference to Dr. Deveze as author of a dissertation, in which he states, as Bancroft says, "his reasons for believing the disease to have been neither imported nor contagious." It is true, Dr. Deveze was not an American writer, and that Dr. Davidge could find no reference to non-contagion in any work published by natives of this country. But though not an American by birth, Dr. Deveze wrote in Philadelphia—his subject was the yellow fever of Philadelphia, and he published his dissertation, as Bancroft himself states, in Philadelphia—not in French only, but also in English. His work may, from these facts, be held, to all intents and purposes, in the light of an American publication, and his claims to priority should not be allowed to yield to that of writers who confessedly published their views three years after he had laid those of a similar kind before the public. It may be mentioned, also, that on August 27, 1797, Dr. Deveze addressed a long letter on the subject to General Mifflin, then Governor of Pennsylvania, in which the same doctrine is strenuously supported. This letter, which appeared in the newspapers of the time, and is inserted in the last edition of that author's work,* came out anteriorly to that of Dr. Davidge, to the reprint of Dr. Maclean's essay, or to the public enunciation of Dr. Rush's adoption of the doctrine of non-contagion, and would alone suffice to justify the credit assigned to its author. It may be mentioned, in addition, that even had Deveze remained as silent as other non-contagionists on the subject, neither Dr. Rush, Dr. Davidge, nor Dr. Maclean, could claim the

* *Traité de la Fièvre Jaune*, p. 22. Paris, 1820.

by our American physicians of the dissertation of Dr. Deveze—which, as already stated, appeared in 1794—or of his letter to Governor Mifflin (Aug. 1797), it is not improbable, as Dr. Bancroft (p. 373) has suggested, that these publications—as well, I may add, as the influence of several French practitioners who had much experience in the fever, who enjoyed the confidence of the public at large, and openly and distinctly avowed orally their disbelief in contagion—helped to induce some at least of the physicians of this city to inquire and think with greater freedom on the subject; and, by laying before them facts and arguments confuting the generally received doctrine, induced some of those who had embraced the latter at full length, to regard the fever as less frequently and generally contagious than they had believed it to be, and finally to alter their views completely on the subject, as well as to lead those who had not yet formed an opinion to adopt the non-contagious side of the question.

But, whether from the influence of these publications or not, certain it is, that soon after, a change was perceptible, in the opinion of the leading members of the profession. Dr. Rush, who, though an early advocate of the domestic origin of the yellow fever, was, in 1793, a decided supporter of unconditional contagion, so far modified his views in his account of the partial epidemic of 1794, as to refuse to admit contagion to be one of its characteristic marks; deeming this quality to be rather accidental, and to depend on circumstances of season, country,¹ &c. In 1797, Drs. Rush, Ch. Caldwell, W. Dewees, J. R. Coxe, P. S. Physick, and eight others, who, like the first named, embraced the doctrine of domestic origin, being called upon by the governor to state their views respecting the origin, progress, and nature of the pestilence of that year, declared² that “the common bilious and yellow fever (which they supposed to be identically the same) are alike contagious, under certain circumstances of the weather and of predisposition of body,” adding, however, in the second letter, dated March 20, 1798, that many recent facts and observations render it probable, that the reports of its contagious nature have been exaggerated, and that it is not so often propagated by contagion as has been supposed, especially in warm weather, when sick-rooms are open night and day to the constant accession of fresh air (p. 31). In the following year, the same physicians and others being

credit of priority in the matter in question, as they were all anticipated by Dr. Valentin, of Norfolk, who, in September, 1795, openly advocated the doctrine of non-contagion in an article inserted by him in the public papers of that place,* and referred to in his treatise on the yellow fever published a few years after (1803).

¹ Med. Inq. and Observ., iv. 63. It may be proper to remark that all this disappeared in the later editions of this work.

² See Proofs of the Origin of the Yellow Fever in Philadelphia and Kensington, in the year 1797 from Domestic Exhalation, &c., in two Letters to the Governor, p. 3. Philadelphia, 1798; Rush, Fever of 1797, pp. 46, 47.

* See the “Herald,” and the “Norfolk and Portsmouth Advertiser,” 4th Sept. 1795, p. 34, note.

then incorporated as "the Academy of Medicine," so far modified their views as to declare in a letter of the same import "that the disease is not contagious in the West Indies, and rarely, if ever, so in the United States." "So general," they say, "is this opinion, that some physicians have unfortunately refused to admit the existence of the fever in its commencement in our city, only because it was not contagious."¹

We here perceive a further approximation to a doctrine which had been insisted upon three years before by Dr. Deveze, and subsequently by Valentin in Norfolk, by Davidge in Baltimore, by the editors of the *New York Med. Repository* and other physicians of that city and other parts of the United States,² and it is probable, as Dr. Bancroft remarks, that several, at least, of the members of the Academy of Medicine would have then completely renounced all belief in the contagion of yellow fever had they not been deterred from so doing by Dr. Rush, who exercised a strong influence over the minds of the profession and could not easily relinquish the theory of an occasional or accidental occurrence of that quality. But this influence, if it really produced the effect in question, could not continue to do so forever. Hence, Dr. Caldwell, one of the signers of the afore-mentioned letter, and who, at first, had regarded the fever as contagious and imported, and expressed his opinion in newspaper articles which appeared under the caption of "Areteus, Jr.," so far changed his sentiments in 1797 as to view it not only as of domestic origin, but as being very rarely and feebly, if at all, contagious.³ As his field of observation expanded, his views became more fixed, and he, at last, was a firm and able advocate of the non-contagious character of the disease.⁴ Other seceders from the ranks of the contagionists fol-

¹ New York Med. Repos., ii. pp. 354, 355.

² E. H. Smith, *Fever of New York*, in 1795; Webster's Collection, pp. 109, 143; Seamen, *ib.*, p. 14; Taylor and Hansford, *ib.*, p. 150.

³ Mem. and Phys. Mem. (1801), pp. 76, 77; Mem. (1826), pp. 84, 85.

⁴ See the above Memoir, as also his Account of the Fever of 1803, in *Med. Reposit.*, vii. 152, and of 1805, p. 66.

In Dr. Caldwell's Autobiography, to which I have already had occasion to refer, I find a passage which conflicts somewhat with the statements made in the text respecting the original opinions of that eminent writer on the subject before us, and which, consequently, must be noticed in this place, lest I might be accused of having been unjust to him, by misrepresenting his true sentiments. Dr. C. says: "The opinion maintained chiefly by Dr. Rush and his immediate followers, was that the disease originated from the filth of the city, and when thus produced, was propagated by contagion, and that it was, therefore, essentially different from common bilious fever, not only in degree, but also in kind. And for that doctrine he strenuously contended, in his lectures and publications, for I know not exactly how long, but for upwards, I think, of ten years.

"Another opinion was, that yellow fever is only a higher grade of common autumnal fever, and arises, therefore, from the same causes, and is subject to the same laws. Or, that if it be, in some shades of modification, different (as most probably it is), it, notwithstanding, arises from a higher and more deleterious form or modification of what was then called marsh miasmata, and is not contagious. That was the opinion defended by myself. And, as far as I knew at the time, or yet know, I was the first to frame, defend, and endeavour to prove and propagate it in Philadelphia and elsewhere. I certainly de-

lowed, until at last Dr. Rush enrolled himself among the adversaries of that doctrine which, from that day, met lessening opposition, and succeeded in a few years in acquiring the supremacy to which I have alluded.

rived it from no one; I found it out by observation and reflection on what I witnessed in the yellow fever hospital established a few miles from Philadelphia in the unmalarious atmosphere of the country. I perceived that neither myself nor any other person, exposed only to the atmosphere of that institution (inspiring it, and being constantly involved in it both by night and day, whether eating, sleeping, or otherwise employed), was attacked by the complaint, provided he kept strictly aloof from the atmosphere of Philadelphia; but that if he exposed himself to that atmosphere, especially during the night, he almost certainly contracted it, however carefully remote he held himself from the wards of the hospital.

“In that confined spot commenced my first observations on the subject. But they subsequently extended over a much wider field, and still communicated to me invariably the same result. An atmosphere surcharged only with the exhalation from patients labouring under yellow fever, but free from common malaria, produced no pestilential disease; while the atmosphere of the city in which yellow fever prevailed, though no person was sick in the immediate neighbourhood, engendered it. And during seven terms of the epidemic prevalence of yellow fever, through which I passed in Philadelphia, the same was true” (pp. 420, 421). In the next page, he taxes Dr. Rush with having “ministered to an error of magnitude and mischief, in attempting to establish and propagate the notion that yellow fever is a contagious complaint.”

In this passage, the author appears to claim credit for having adopted and defended the doctrine of non-contagion from the time of the epidemic prevalence of the yellow fever of 1793—his belief on that subject being founded on the facts observed by him at the fever hospital, and confirmed by his experience obtained during subsequent epidemics. In the text I have stated, on the contrary, that, in 1793, he was a contagionist, and that he changed his views on that point several years after. If his statement is correct, I am evidently in fault. But I think it can be shown that my assertions are founded on fact, and that the doctor’s memory, while penning the passage transcribed some fifty years after the occurrences referred to, had failed him. 1. In a newspaper article, written by Dr. Caldwell, signed Areteus, Jr., and which appeared in 1793, the author says: “Under the present state of general alarm and painful apprehension excited in the mind in consequence of the contagious fever now prevailing in this city,” “the citizens ought to be informed that the present fever, though certainly contagious, is nevertheless communicable only under particular circumstances.”* 2. In a “Semi-Annual Oration on the Origin of Pestilential Diseases,” delivered before the Academy of Medicine in December, 1798, Dr. C., speaking of the plague and “the American pestilence,” says: “That they are occasionally propagated by contagion, is a position to the truth of which physicians of the highest eminence have, at all times, afforded the testimony of their assent” (p. 45). In the same year (1798), Dr. Caldwell signed, with other members of the Academy, a letter addressed to the Governor of the State (his name being second on the list), in which we find the following passage: “The common bilious and yellow fevers are alike contagious, under certain circumstances of the weather and of predisposition in the body. That the common bilious fever is contagious, we assert from the observations of some of us, and from the authority of many physicians, who have long commanded the highest respect in medicine.”† 3. Finally, in another publication the same author says: “Know, then, that I was formerly a skeptic with regard to the origin and nature of our autumnal pestilence; or to speak with more correctness, I was strongly inclined to consider it as a contagious and

* Dunlap’s Daily Advertiser for 1793.

† Proofs of the Origin of the Yellow Fever, &c., pp. 3, 4.

The non-contagion and domestic origin of the yellow fever have been and are to this day considered by their advocates to be fully established on the following grounds:—

an imported disease.” And after stating that he determined, from the first, to suspend his final belief till time should furnish him with facts sufficiently numerous and unequivocal to warrant a decision, he adds: “In the epidemic of *ninety-seven* this occurrence took place. During that period of calamity, facts so luminous and circumstances so weighty obtruded themselves on my observation, as convinced me that my original suspicion was unfounded, and constrained me to believe that the disease in question was not imported, and was but very rarely and feebly, if at all, contagious.” This opinion became more and more strengthened and confirmed by the progress of time. “Every observation I have made and every fact I have collected respecting the origin and nature of the epidemic of ninety-eight and ninety-nine, have contributed to its further and more permanent establishment.”*

Nothing, it appears to me, can be more positive. It follows from these extracts from the writings of Dr. Caldwell himself, that in 1793, so far from being a non-contagionist, he advocated a contrary belief—that he changed his creed, not in that year, from the facts he saw at the hospital, but four years later, and that it was only from the observation he made in '98 and '99, he became fully settled in the opinion of the non-contagious character of the disease. Dr. Rush was not the only one, therefore, to minister to the error of magnitude and mischief alluded to.

As Dr. C. states, in his Autobiography, that he was house surgeon of the Fever Hospital in 1793, there can be no doubt of the fact, though no mention is made of it in any other of his numerous writings, or in any of the publications extant, relative to the events of that year. Admitting it to be so, I may remark, that the author was in the proper place to imbibe the precepts of the doctrine of non-contagion; for the physician of the establishment was Dr. Deveze, to whom I have referred as being the first to proclaim that doctrine in reference to the fever of this city, and we may presume that those who were employed under him, or who frequented his wards, could scarcely fail to learn his views on the subject, and the grounds on which those views were founded.

So far as regards the opinion attributed by Dr. C. to Dr. Rush on the subject of the non-identity of the yellow and common autumnal bilious fever—that the former was essentially different from the latter not only in degree but also in kind, and that while the bilious was not contagious, the yellow fever was propagated in that way—I have only a few words to say. This information, if correct, is new. But here, again, Dr. Caldwell's memory seems to have failed him; for there is scarcely a page in Dr. Rush's works on the different epidemics of this city which does not disprove the statement alluded to. He over and over again insists on what he held in the light of an undoubted fact—that the yellow is only an aggravated form of autumnal bilious fever, and, to all intents and purposes, the same disease in kind. Besides, every one knows the pertinacity the doctor displayed in the defence of his favourite theory of the unity of disease. “Science,” he says, “has much to deplore from the multiplication of disease. It is as repugnant to truth in medicine, as polytheism is to truth in religion. The physician who considers every different affection of different systems in the body, or every affection of different parts of the same system, as distinct diseases, when they arise from one cause, resembles the Indian or African savage, who considers water, dew, ice, frost, and snow, as distinct essences; while the physician, who considers the morbid affections of every part of the body (however diversified they may be in their form or degrees), as derived from one cause, resembles the philosopher who considers dew, ice, frost, and snow, as different modifications of water, and as derived simply from the absence of heat” (iii. 79). “Why should it sur-

* Medical and Physical Memoirs (1801), pp. 77, 78.

The number of Non-contagionists very large.—Large as may be the number, eminent the talent, and extensive the experience of those who regard the disease as of exotic origin, endowed with contagious properties, and capable of being carried from place to place in ships or otherwise, it is certain, that if the question of the origin and mode of propagation of the disease could be settled—or if that settlement could be promoted—by an appeal to the amount of support which each of the contending parties has received and continues to receive, certain it is, I say, that the anti-contagionists would have nothing to fear from the results of the comparison. For such a comparison will show that the number of the advocates of the latter doctrine far exceeds that of physicians who have embraced the opposite view either to the full extent or contingently; while in point of intelligence, research, and experience, they are in no way inferior to their adversaries. Having already stated that, in Philadelphia, this doctrine meets now with little if any opposition, it will be sufficient to mention in this place, that, of those who have published their views on the disease, the majority are anti-contagionists. Thus, to oppose to the names cited in the preceding chapter we have those of Deveze, Caldwell, Rush, Chapman, Monges, Jackson, Emlen, Ffirth, and Mitchell (Th. D.).

In New York, where in consequence of the influence exercised by Dr. Hosack, and the zeal and activity of a few devoted partisans, the doctrine of contagion has flourished more luxuriantly than in other parts of this country, we nevertheless discover that the supporters of the negative, so far from being inactive, have published as much as, if not more than, their opponents. Of this the reader may readily convince himself by referring to the writings of Miller, S. L. Mitchell, E. H. Smith, Seaman, Rodgers, Pascalis, Beck, Yeates, Hardie, Ramsay (of Edinburgh), Watts, Coventry, J. M. Smith, and to the volumes of the Medical Repository. No less decided in favour of non-contagion will be found to be the opinion of the majority of physicians who have observed and described the fever of Boston (Brown, Warren, Rand), New London (Hull, Channing), and Providence (Wheaton).

In Baltimore, the same doctrine has been for many years in the ascendant, and enumerates among its advocates Drs. Drysdale, Davidge, Chatard, Moore, Revere, Potter, Jameson, and Reese, besides being adopted in the

prise us to see a yellow fever generated amongst us? It is only a higher grade of a fever which prevails every year in our city, from vegetable putrefaction" (iii. 110, 111). This certainly does not look like regarding the two fevers as different, not only in degree, but in kind. Nor did Dr. Rush ever draw a line of demarcation between these fevers on the score of the contagiousness of the one, and the non-contagiousness of the other. He believed them all to arise from the same cause. He at one time believed them all to be occasionally contagious; and subsequently denied the possession, by either, of the property in question. He signed, and probably drew up the afore-mentioned letter to the governor, in which the contagiousness of both is affirmed. In a letter to Dr. Edward Fisher, inserted at the head of an essay by the latter on yellow fever, published in 1795, Dr. Rush says: "I agree with you, that the yellow fever, though sometimes contagious, is not generally so. In this respect, it is upon a footing with the common bilious fever."*

* *Tentamen Medicum Inaugurale de Febre Flava*: Edin., 1795.

various reports of the Board of Health, and the official statements of the District Medical Society of Maryland, and forming the creed of eighteen out of the nineteen physicians who responded to the call of the Mayor for information respecting the epidemic of 1819.¹ In Wilmington (Del.), we find Dr. John Vaughan—at Bladensburgh, Dr. Baker—at Alexandria, Dr. Dick—in Norfolk, Drs. Valentin, Taylor, Hansford, Selden, Whitehead, Archer—in Wilmington, N. C., Drs. Rosett and Hill—and in Augusta, Dr. Smelt, Drs. Rodil, and the authors of a report submitted to a meeting of physicians of that city on the epidemic of 1839, lending their support to the same belief. If we proceed to Savannah, we shall find Drs. Waring, Daniel, and Arnold, advocating the doctrine of non-contagion. In Charleston, with the exception of Drs. Dickson, Strobel—and, perhaps, a very few others—the same unanimity exists, or rather existed till within a short time, as may be found in the writings of Moultrie, Ramsay, Tucker, Harris, Shecut, Whitehead, Simons, Johnson, and in the report on the epidemic of 1839, signed by Dr. A. Lopez, J. Moultrie, E. Gedding, J. M. Campbell, H. Winthrop, and E. Holbrook.

Again, in Mobile, the non-contagiousness of the fever has been insisted upon in public documents relative to the epidemic of 1819, as well as by Toulmin, Lewis, and subsequent writers.²

In Natchez, we find Dr. Cartwright, Dr. Merrill, Dr. Perlee; in Woodville, Dr. Stone; in Pensacola, Dr. Barrington and Dr. Hulse; at Key West, Dr. Ticknor, and Dr. Dupres; at Galveston, Dr. Ashbel Smith; finally, in New Orleans, the same view is sustained by Drs. Thomas, Gros, Chabert, Barton, Harris, Taillefer, Baxter, Dupuy de Chambery, Hort, Harrison, Stone, Axson, McFarland, and Dowler; as well as in the various reports on the epidemics of that city, and other published documents.

In Spain, if the contagionists present a strong array of names, and a bold front, their adversaries are scarcely less numerous. Thus, among the native physicians, Drs. Piguilem, Salva, Duran, Lopez, Campmany, Porta, Oller, Calveras, Meyner, Sahuc, Recio, Alfonzo de Maria—and we presume that to these, other names might have been added had not the expression of opinion on the subject been at one time prohibited by law—advocate that side of the question, and enforce it by numerous facts and arguments. While such is the case relative to Spaniards themselves, we shall find that the number of English, French, and American physicians who have investigated the fever in Cadiz, Barcelona and Gibraltar, and pronounced in favour of its non-contagion, greatly exceeds that of writers who advocate a different doctrine. This will be seen by a reference to the writings of Maclean, R. Jackson, Amiel, T. Smith, P. Wilson, Frazer, Gillkrest, O'Halloran, Lassis, Paradie, Doughty, Chervin, Proudfoot, Pascalis, Blin, Jourdain, and Campbell; while so far as relates to the limited epidemics and sporadic cases of Mar-

¹ A Series of Letters and other Documents relating to the late Epidemic of Yellow Fever, &c. Baltimore, 1820.

² Report of Committee appointed to investigate the Causes and Extent, &c., of the Epidemic of 1819, in Letters on the Fever of Baltimore: pp. 207.

seilles, and the severe visitations of Leghorn in 1804, and of Rocheford many years before, we have in Segaud, Sue, P. M. Roux, Lacoste, Tommasini, Le Roy, Chirac, and others, able advocates of the doctrine in question.

If anti-contagionists need not be apprehensive of the result of a comparison of the number and merits of the supporters of their favourite doctrine with the adherents of opposite views in this country and Europe, they will find themselves on still safer grounds if they turn to tropical climates; for there the preponderance is much stronger in their favour. In such a list will be found, besides the names already mentioned, those of the majority of physicians who have written on the fever of those regions. Despérrier, Dazille, Hillary, Hume, John Hunter, Walker, Grant, Moseley, Lempriere, James Clark, Huck, Hector McLean, Bancroft, Ferguson, Humboldt, Musgrave, François, Ralph, Savarésy, Lefort, O'Halloran, Osgood, Mortimer, McCabe, McArthur, Madrid, Jones, Gillkrest, Valentin, Dalmas, Rochoux, Wilson, Rufz, Chervin, Pinkard, Veitch, Dickinson, Maher, Chabert, Dariste, Catel, Monson, Chambolle, Furlong, Crichton, Dyott, Fournier Pescay, Vatable, Dubreuil, Bone, King, H. Dickson, Doughty, Imray, Blair, Joubert, Cornuel, Pouvreau, S. Henderson, Amic, Chapuis, De Carvallo e Souza, Lallemant, Rochoux.

It may be mentioned, that many of those whose names have here been enumerated, state not merely their own views on the subject under consideration, but express the conviction of their professional brethren. Dr. Blair informs us that, in reference to the non-contagious nature of the fever, no difference of opinion existed at Demerara in 1838 (p. 54). Dr. H. McLean says: "I never conversed with any medical gentleman at St. Domingo, who did not form the same judgment" (p. 77). Dr. Pouvreau, of Martinique (Fort Royal), states: "Not one of the physicians of the colony believes in the contagion of the yellow fever." Sir W. Burnett, himself a contingent contagionist, informs us that he caused the medical reports of the Jamaica Hospital for more than twenty years to be examined, and though fever has frequently occurred in that establishment, "not one of the medical officers of the hospital has ever hinted at the disease being contagious."¹

In several yellow fever localities, the public at large are convinced of the non-contagiousness of the disease. "It is an undoubted fact," says Humboldt, "that the *vomito* is not contagious in Vera Cruz. In most countries, the people regard as contagious diseases which do not possess that property; but in Mexico, no popular opinion prevents the non-acclimatized stranger from approaching the bed of individuals labouring under the vomito. No facts are adduced of a nature capable of inducing the belief that immediate contact with, or the breath of those dying of the disease, can prove prejudicial to non-acclimatized persons who nurse the sick. On the continent of North America, the yellow fever is not any more contagious than are intermittent fevers in Europe" (p. 769).

To these lists of non-contagionists in regard to the yellow fever of tem-

¹ Official Correspondence on the Boa Vista Fever, fol. p. 81—see also Edinb. Journ., lxxviii. 501.

perate and tropical climates, might be added others scattered through the medical journals, or mentioned in Dr. Bancroft's *Sequel*.¹ They might also have been swelled by the addition of many who, though expressing the opinion that the disease may, under peculiar circumstances, acquire the property of transmissibility from the sick to the well, adduce no fact in support of the hypothesis, and only speak of the event as being within the range of possibility; and are in fact, so far as their own observation extends, strictly speaking, anti-contagionists. But such as they are—aided especially by the fact that out of 541 MS. documents obtained by Dr. Chervin (*Rapport*, pp. 6, 7), all but 48 were favourable to the opinion in question—they show conclusively that, so far as the number of authorities is concerned, the preponderance is decidedly in favour of the doctrine of non-contagion.

The Doctrine of Non-contagion traced to remote times.—No argument in favour of the contagiousness of the yellow fever can be derived from the antiquity of that doctrine. That, in very early times, the idea of the transmissibility of the disease, and its importation from abroad, prevailed in the West Indies among physicians, governments, and the public at large, is true, and cannot to us, for reasons already assigned, be a matter of surprise. But, however certain this may be, and also that at Barbadoes, in 1647 and 1695, it was thought by some to have come from some distant place in the way mentioned; that Trapham attributed the epidemic of Jamaica in 1679, or Labat that of Martinique in 1688, to importation; that Du Tertre referred the origin and propagation of the *plague* of Guadaloupe to a like cause; or again, that Vines, and afterwards, Warren and others regarded it as contagious; however certain, I repeat, all this may be, it affords no support to the doctrine in question. In the first place, the early rise of a doctrine, and its adoption and propagation by even men of science, is not a proof of its correctness, for opinions have often been accredited and long maintained, which a more mature experience has caused to be abandoned as destitute of foundation. Again: in the instance before us, even if the antiquity of the opinion could be of any avail, the circumstance would lose much of its

¹ Pp. 97, 155, 178, 440, &c. “Having stated the opinions delivered in the reports transmitted to the Privy Council, it may be proper to give a summary of them; and I will therefore mention that, of the twenty-four gentlemen from whom these reports were obtained, three (Mr. Gregory, No. 12, Dr. Keim, No. 15, and Dr. Magrath, No. 17), have omitted the statement of any opinion on the subject of contagion, as connected with the fever in question. Three others (Dr. Weir, No. 1, Dr. Blair, No. 2, and Mr. Tobin, No. 21), have expressed their opinion that it is contagious; one of them (Mr. Brien, No. 20), declares his belief that in individual or solitary cases, it is “incapable of communicating itself to those who are contiguous; but that when several were labouring under the disease at the same time, he believes it to be highly contagious; and another gentleman (Dr. Gardiner, No. 9), appears to think, that local causes contributed at least as much to the production of the fever at Gibraltar, in 1813, as contagion. Of the remaining sixteen, the majority have absolutely and positively denied the existence of any contagious property in the fever; and the rest have declared their belief that it is not *naturally* or *properly* a contagious disease, although several of them are inclined to believe that it may—as they suppose to happen in most other diseases—acquire a contagious property by crowding, filth, &c.”—*Bancroft, Sequel*, p. 178.

force, unless it had been held by individuals competent to decide in matters of the kind. Now, some of the authorities cited in proof of the early origin of the doctrine of contagion have either connected the fact of the commencement of the disease with the time of arrival of a particular vessel or vessels without saying one word of contagion, or have only repeated—and that, too, years after the outbreak of the fever—the current reports of the time among the public at large. I shall not stop to point out the little reliance that can be attached to such reports for coming to a decision on such subjects especially in an age when the simultaneous attack of many individuals was regarded as sufficient evidence of the contagious nature of any disease. Labat's statement, which was founded on hearsay evidence, and is, besides, loose and unsatisfactory, amounts to little more than what was reported by the public several years after the appearance of the fever at Martinique. He only informs us of the statements current at the time of his arrival—perhaps of the surmises of the few—and nowhere tells us that the physicians or government had adopted a belief in contagion and importation; and, finally, he accounts for the origin of the disease in a way which, as we shall see as we proceed, is not entitled to the consideration of scientific inquirers. Hughes's account of the fever of Barbadoes in 1695 was also founded on hearsay evidence, and written some time after the occurrence of the events he records. The same may be said of Du Tertre's account of the plague of Guadaloupe; both Hughes and Du Tertre giving their own impressions and those of the public at large, and neither of them were physicians.

But even had the opinions, entertained at that early age, been expressed by men competent to investigate subjects of the kind, the influence would be neutralized by the fact, that contrary views were as early promulgated by many as fully able to arrive at correct conclusions in such matters; and very soon after became generally advocated in some, if not all, the West Indian colonies. Thus, we find that, in 1673, Ligon, who had witnessed the epidemic of 1647 at Barbadoes, disbelieved the importation of the disease and attributed it to causes of a domestic character;¹ and we are told by Lefort that the opinion of the non-contagion of the yellow fever is not only general, but traditional in the West Indies. “In the immense collection of laws and ordinances by which the colonies have been governed for nearly two centuries (and this is a very remarkable fact), there will not be found one prescribing the most simple measure against the propagation of that disease. Never, and nowhere, has the intercourse been interrupted between persons affected with it and the well; between the cities where the disease prevailed and those that were free from it. Would this have been the case if the yellow fever had been the evident or even probable result of such a freedom of intercourse? Would the governors, the intendants, or the chief magistrates, who have succeeded each other for the last 150 years, have all been sufficiently blind not to perceive the contagion, if truly the latter had existed; or sufficiently culpable to allow the people confided to their care to

¹ Hist. of Barbadoes, p. 21.

be invaded by that contagion if it had been recognized? Both suppositions are absurd."¹

If we appeal to the published records of professional opinion on the subject, we shall find that, at a very early date, the doctrine of non-contagion had already made converts. Dr. Town, who practised in Barbadoes in the early part of the last century, and whose work, already referred to, was published in London in 1726, was apparently no contagionist or importationist, for he says not one word about the former, and attributes the fever to home causes. In like manner Dr. J. Williams, in his essay on the fever of Jamaica, ignores the power of transmissibility of the disease from individual to individual, and speaks of it as originating in the place.² Makettrick, in his inaugural dissertation, published a full century ago, avows similar views. Nasmyth was a non-contagionist.³ Desportes, it is true, speaks of the contagiousness of the fever as he observed it in St. Domingo. But, like many other writers, he uses the word contagion in the sense we now attach to the word infection—as denoting the power of the disease to spread. This, he maintained, was due to a certain condition of the air arising from local exhalations; and he nowhere gives us to understand that the fever could be communicated from the sick to the well. He denominated the disease *Maladie de Siam*; but gives no credence to the opinion of its being imported in the West Indies from Brazil or Siam—contenting himself simply with the remark that it first attracted attention at Martinique at the time of the arrival of vessels from the last of those countries.⁴ Hillary practised in Jamaica from 1752 to 1758, and published his observations somewhere about 1760 (2d edit. is dated 1766). In this work he avows himself a non-contagionist (pp. 145, 146). I have already stated reasons for believing that the fever which prevailed at Rocheford (France), in 1694, was truly the yellow fever, or approximated to it very closely. It may not be improper to remark in this place that Chirac, to whom we are indebted for an account of that epidemic, which he investigated by order of government, was neither a contagionist nor an importationist.⁵

Even our own country furnished evidence of the early adoption of the doctrine of non-contagion and of domestic origin; for long before the epidemic of 1793 in this city—when the first manifestation of an inkling in that direction is supposed to have appeared—Dr. John Moultrie, of Charleston, in a dissertation on the yellow fever of that city, published in 1749, expressed himself in terms which, though somewhat at times ambiguous, leave no doubt, especially when taken in connection with his views respecting the true efficient cause of the disease, to which reference will be made in a future chapter, as to his disbelief in contagion, properly so called. Almost everybody, he says, is persuaded that this disease is contagious. Doubtless, all individuals are obnoxious to it where it prevails; since they are surrounded by and breathe the

¹ *Mémoire sur la non-contagion de la Fièvre Jaune*, pp. 36.

² *An Essay on the Bilious Fever of Jamaica*, pp. 16, 17.

³ Letter to Dr. Lind; see *Work of the latter on Seamen*, p. 54, note.

⁴ *Maladies de St. Domingue, &c.*, p. 191.

⁵ *Traité des Fièvres Malignes; des Fièvres Pestilentiellles, et Autres*, i. 191.

same air; and acrid and semi-putrid exhalations continually proceed from the bodies of the sick, and the air of the place where those who have the disease lie, is filled in a short time with these. This air being infected, cannot be breathed by the healthy or the sick without great risk of injury. But I have seen very many who sat daily by those labouring under the disease; but being defended from the injurious effects of the air and over-fatigue, escaped. "I cannot believe," continues Dr. Moultrie, "that this disease is propagated by contagion from one locality to another, unless the air is suited for producing it. In the year 1745, this disease raged very severely in South Carolina, where it was very plain that it did not owe its origin to contagion, for the first person seized with the disease was a sailor, who, having been drunk for two or three successive days, afterwards fell into the fever. From that time, excessive heat lasting for a month, the disease began to prevail extensively in Charleston. And when the heat is so great, there is danger lest a common high fever should merge into this more violent disease, which also, I doubt not, may happen from improper treatment and a bad regimen; indeed, unless I greatly err, I myself have seen such results.

"In a letter which I received from a surgeon who sailed to the West Indies with Admiral Vernon, the commander of the fleet, the writer says: 'The troops that embarked at Bristol to go to the West Indies were affected there with a severe fever, very similar to those accounted contagious. The care of the ship in which the sick were removed was committed to my care, and I alone escaped untouched. When we approached the hot latitudes, the fever assumed another character. It became bilious, and before we reached Jamaica it presented all the symptoms of the American bilious (yellow) fever'" (p. 9).¹

¹ Omnibus fere persuadetur hoc malum contagiosum esse. Omnes, sine dubio, morbo huic grassanti obnoxii sunt, quatenus, eodem ære ambiuntur, eumque inspirant; et ex ægrorum corporibus, exhalationes æres et semiputridæ continuo exeunt, et ær loci in quo decumbit æger brevi hisce repletur, quem infectum sani æquè ac ægri non sine magno damno inhauriunt. Plurimos vero vidi qui quotidie hoc morbo laborantibus assidebant, sed ab acris injuriis, et nimio motu denfensi, incolumes evasere.

Crederè cum aliis non possum, hoc malum contagione ex una ad aliam provinciam propagari, nisi ær ad id producendum aptus sit. Anno 1745, hic morbus in meridionali Carolina maxime sæviebat, cum contagionem ab alia provincia, non esse in causa plane appareret. Nam primus hoc morbo correptus eo anno, nauta erat, qui per duos vel tres dies continuos inebriatus, exinde in febrem hanc incidit, et ab eo tempore, nimio per mensem darante æstu, morbus in Caroli-oppido maxime grassari cæpit. Et quando tantus adest æris æstus, periculum est, ne febris ardens communis in hunc morbum sæviorem mutetur, quod etiam a medicamentis minus aptis et malo regimine fieri minime dubito; immo tale quid ipse ni fallor vidi.

In epistola quam Chirurgus, qui cum D. Vernon classi nostræ præposito in Indos Occidentales navigabat, ad me misit, hæc scribebantur. "Legio quæ ad Indos Occidentales profectura, naves Bristolæ conscendit, ibi febre ardenti afficiebantur, omnimodo simili his quæ contagiosæ habentur; cura navis quæ ægri vehebantur mihi commissa, solusque in ea intactus evasi. Quum regionibus calidis propinquaremus, febris aliam induit speciem, biliosam nempe, et omnia symptomata febris biliosæ Americanæ prius nacta est, quam ad Jamaicam appellere nobis contigerit."

CHAPTER XIV.

FACTS AND ARGUMENTS AGAINST CONTAGION—CONTINUED.

Conversions from Contagionism very numerous.—Not a little stress, as we have seen, is laid on the fact that physicians of eminence have been found who, having believed in and defended the doctrine of non-contagion, have, on more mature deliberation, and after acquiring greater experience, changed their views and become converts to the opposite doctrine. Sure it is that such changes have occurred in this and other countries, and that among those cited we discover names entitled to the respect of the profession. But, on this subject, as on those that precede, a reference to the annals of the yellow fever will not lead to any result advantageous to the advocates of the latter doctrine, inasmuch as examples of an opposite change are far more numerous than any to which they can point out. The occurrences in this country are strikingly illustrative of the truth of this statement. Sir Gilbert Blane, with characteristic inaccuracy—a defect particularly inappropriate when exhibited in a work on medical logic—states, as an example of the influence of knowledge in dissipating error, that, “in America, during the last years of the last century, the majority of opinions were in favour of non-contagion, and even public medical bodies gave their opinion on this side; but, in the year 1805, the College of Physicians of Philadelphia, as a body, gave their opinion in favour of contagion, asserting that the evidence of this was as strong as for that of plague.” “Such is the meliorating and maturing influence of time.”¹ In a later edition of the same work, Sir Gilbert contented himself with presenting his readers with the outlines of the controversy between the two parties—first, because he no longer thought it his *duty* to enter into particulars with the same amplitude and earnestness he had before done, when he felt that the errors of the non-contagionists had “occasioned, and were then occasioning, the death of thousands, and that thousands of lives might be saved by the adoption of regulations founded on a more correct view of the subject;” and, secondly, because he saw that, in so far as he could learn, there was a general conversion of opinion to what he deemed the cause of truth and humanity.²

Where the zealous baronet derived his information, it would puzzle him to say; for it is a fact well known to every American medical reader, and which the references made in preceding pages must have shown, that the very reverse of what he states is the case, “the meliorating and maturing influence of time” having resulted not in the conversion of anti-contagionists into contagionists, but of contagionists into anti-contagionists. We have seen that, in the early epidemics of this city, the opinion of the contagious-

¹ Elements of Medical Logic, p. 186.

² Ib., 2d ed., pp. 234-5.

ness of the disease was almost universally admitted; that, in all their reports, the College of Physicians strongly advocated that opinion, and carried it to the utmost limits; and that the members of the Academy of Medicine, while believing in the domestic origin of the disease, did not divest themselves completely of the idea of its communicability from the sick to the well. Similar was the state of opinion in New York and other cities visited by the fever. Even in Charleston, Lining and the physicians of early times regarded the fever as contagious and imported. Gradually, however, a change was effected in the sentiments of medical men. Every succeeding epidemic caused a diminution in the number of contagionists; and while, as Ramsay informs us, there was, in 1800, but one opinion among the physicians and inhabitants of the latter city—namely, that the disease is neither imported nor contagious¹—in New York, a similar change, not so general, perhaps, but still very decided, occurred about the very time when, according to Sir G. Blane, the “general conversion of opinion” in favour of contagion began to take place.

This revolution in the sentiments of the profession was effected not only by the accession to the ranks of the anti-contagionists of physicians who had heretofore abstained from expressing an opinion, but by a change among many of those who had, during preceding years, advocated, with more or less earnestness, the contrary doctrine. We have seen that Dr. Caldwell, who in 1793 was a contagionist, became about 1797 a decided convert to the opposite opinion. Our late celebrated physician and surgeon, Dr. Physick, who for a long while was President of the Academy of Medicine, and, as such, signed the reports of that body,² as also nearly if not all the members of the latter, followed a similar course; and, about the same time, or a little later, Dr. Rush, who, as we have seen, had originally espoused the cause of contagion, which he warmly advocated in his lectures, his writings, his correspondence,³ and his conversations, at last relinquished his former views. Not satisfied with doing so silently, as many others had done, this eminent physician published a formal recantation of what he considered his errors, stating that he was aware of the influence which such changes in medical opinion have upon a physician's reputation, but that he would consider as small the total sacrifice of his, “could it avert the evils which are connected with a belief in the importation of pestilential diseases, and insure to the world the benefits which would necessarily flow from the establishment of the doctrine of non-contagion.”⁴ Dr. Rush, in a subsequent part of his work, went even so far as to beg forgiveness of the friends of science and humanity, if the publication of the opinion in question had exercised any influence in increasing the misery and mortality attendant upon that disease; adding that indeed such was the pain he felt in recollecting that he entertained or propagated it, that it would long, and perhaps always, deprive him of the pleasure he might otherwise

¹ Medical Repository, iv. 218, 219.

² Rapport fait à l'Acad. de Méd. sur les Documents de M. Chervin, p. 23, note.

³ See Potter, p. iii.

⁴ See his letter to Dr. Miller, of New York, under date of October 8, 1802, Med. Rep., vi. 155. See also Med. Inq., iv. 143, and Ramsay's Eulog. of Rush, p. 28, note.

have derived from a review of his attempts to fulfil the public duties of his situation.¹ This relinquishment of a long-cherished opinion by so careful and experienced an observer, the tone of deep conviction in which it is made, the high and responsible position held by Dr. Rush in the profession and in society, as also the circumstance that, whatever may have been asserted or surmised to the contrary, he never wavered in his newly-adopted views,² may

¹ Med. Inq. and Observ., Preface.

Dr. Caldwell, in his autobiography, recently published, says, in speaking of some of the facts which induced him to embrace a belief in non-contagion: "Such evidence on the subject I regard as conclusive, and have always so employed it. Nor could Dr. Rush resist it; though he strenuously exerted himself to annul my belief in it, and induce me to become the advocate of his hypothesis. But I was fortunate enough so far to reverse the condition of mind he endeavoured to produce, as to make him the advocate of my opinion, instead of being myself persuaded into the advocacy of his. And that was far from being a common event; for though Dr. Rush was noted for changing his notions and theories himself, he was said never to have previously changed even one of them in concession to the mental influence of an opponent" (pp. 421-2). The editor of the above work alludes to this subject (p. 182), and, after mentioning that Dr. Rush repudiated his former opinion of the contagiousness of yellow fever and plague, quotes a passage from the writings of that eminent physician, in which he says: "For the change of my opinion upon this subject, I am indebted to Dr. Caldwell's and Mr. Webster's publications upon pestilential diseases, and to the travels of Marini and Sonnini into Syria and Egypt." Those who peruse Dr. Rush's text attentively, will perceive that in the passage quoted, reference is made solely to plague, and not to the yellow fever. Dr. R. nowhere alludes to the influence which anything Dr. Caldwell said or wrote exercised on his opinions relative to the origin and mode of propagation of the latter disease.

² Many years after the date of the relinquishment by Dr. Rush of the doctrine of contagion, the authors of the article "Yellow Fever" in the *Dictionnaire des Sciences Médicales* (xv. 351), asserted, on the authority of M. Moreau de St. Méry, that Dr. R. had on his death-bed retracted the declaration in question, confessing that he was induced by personal motives (*considérations particulières*) to make that declaration, never having ceased to believe in the contagion of yellow fever, and disavowing everything written by him in favour of opposite views. This statement was, as may well be imagined, eagerly seized upon by the contagionists. Dr. Chisholm, in the *Edinburgh Medical Journal*,* says: "We are enabled to appreciate the value of the observations of those men who have given their opinion against the existence of contagion in the malignant, pestilential, or Bulam fever, called by them yellow fever, by adverting to what we are told were their *real* sentiments on this subject;" and while admitting that there are no grounds for believing that Dr. Rush was guilty of the insincerity of denying the contagion of yellow fever for a particular reason, reiterates the charge, and adds that his information, which he cannot disbelieve, shows that the death-bed confession and contrition stated by M. de St. Méry are true, and that the last, and we may suppose the firmest, because the first, opinion of Dr. Rush was that the fevers which devastated New York, Philadelphia, &c., were highly contagious, and not derived from domestic or local causes. Dr. Strobel, of Charleston, repeating and indorsing the same statement, insinuates that Dr. Rush was guided in the announcement of a change of opinion by motives of policy, and thinks that the charge in question derives confirmation from Dr. Hosack, in whose *Medical Lectures* (p. 224), published a few years ago, is found the following passage: "Dr. Rush, had he lived,

* On Statistical Pathology of Bristol and Clifton, *Edinb. Med. and Surg. Journ.*, xiii. 276, note.

well be held up as an event calculated to neutralize the converse changes cited by contagionists. Dr. Bancroft has very properly said, that if "the conviction of any one man can reasonably influence the opinions of others on this subject,

would, I verily believe, have returned to his former faith in the contagiousness of the yellow fever, as he had done with religion. At one period, he varied his religious creed several times. He was a Quaker, Anabaptist, Presbyterian, and Churchman, alleging that change was the characteristic of the human mind, and that God alone was immutable. So in medicine. During one of my visits to him, in the last years of his life, I submitted to him my views of the qualified contagiousness of the yellow fever; when he returned it to me, he observed: 'Doctor, you and I can now shake hands and unite nearly in the same doctrine.' "

Drs. Bancroft (*Sequel*, p. 467) and Deveze (p. 260) in Europe, and Dr. Pascalis (*Journ. Gén. de Méd.*, 1818) in this country, soon after contradicted and refuted this slanderous attack in such a way as to render any enlarged examination of it nearly unnecessary. We find, also, that a physician of this city, Dr. Nancrede, in a letter referred to by Dr. Deveze (p. 260), denied in the most forcible manner the allegation, which he stigmatized as an abominable slander on the memory of his master, at whose death scene he appears to have been present. Nevertheless, the charge having been reiterated in 1840, by an American physician, a few words of comment may be admissible.

To the physicians of Philadelphia—both those who were living at the period of Dr. Rush's demise, and those who, though having located themselves here since, have become acquainted with the traditions of the times—it is unnecessary to say that there is not a word of truth in the statement of Moreau de St. Méry. They all know full well that no retraction of the kind was ever made by the illustrious American physician, at the time mentioned, or at any other; and they are unanimous, therefore, in viewing the charge advanced by the former, and reiterated and adopted by Chisholm and Strobel, as an injurious and groundless imputation upon his memory. How Moreau de St. Méry acquired a knowledge of this supposed fact, and from what source Chisholm derived the confirmation of it, I am at a loss to discover, nothing being said on the subject by either of them, and no American authority having as yet been found to substantiate their assertions; and, as Bancroft says truly, some such authority ought to have been referred to, considering the importance of the charge, "as connected with medical truths, and with the otherwise unimpeached character of an eminent physician, who is no longer able to repel an injurious accusation." Moreau de St. Méry resided some time in Philadelphia about the close of the last century, and there published his large description of St. Domingo; but he left for Europe long before the death of Dr. Rush, at which time he held office under Napoleon, in the city of Parma. He could not, therefore, have heard of the retraction otherwise than by correspondence or by the verbal report of some one who was here at the time and had met him in Europe. It would be extraordinary, indeed, that such a statement, if true, could have reached him at Parma, and not be even whispered among the friends and opponents of Dr. Rush in the very city where he died. But it cannot be too often repeated that there is no truth in the assertion; the recantation was not alluded to in any medical or public print of this country; it is not mentioned in Ramsay's eulogy of Dr. Rush, or in the writings of the late Dr. Hosack (his lectures excepted) and of other contagionists, prior to the publication of Dr. Strobel's essay; and in a word, Dr. Rush died in the firm belief of the non-contagiousness of the yellow fever; and neither during his last illness, nor at the moment of his death, uttered a word of all that has been attributed to him in reference to this matter.

In conclusion, I may add that the statement made by Moreau de St. Méry, being, as we must presume, the result of a vague report which had reached his ears in a distant land, can in no way be viewed as a matter of astonishment. Nor is it difficult to understand the course pursued by Chisholm, for his animosity towards anti-contagionists was unbounded, and anything that reflected injuriously upon them was naturally welcome to him; but that Dr. Strobel should have been so blinded by party

the preceding declaration by Dr. Rush ought to produce that effect, made, as it was, not precipitately or capriciously, but with slow and cautious deliberation; not from the impulse of former prejudice, but in direct opposition to it; not from a vain desire of appearing to be infallible, by vindicating opinions inconsiderately promulgated, but with a *conscientious* and humble (I had almost said humiliating) purpose of condemning and renouncing such opinions, and of atoning for them, if necessary, even by the sacrifice of his reputation at the shrine of truth."¹

Dr. Thomas D. Mitchell, formerly of this city, and for some years professor in the medical faculty of the Transylvania University, assumed the duties of Lazaretto physician as a decided contagionist, but changed his views on becoming familiar with the operation of the quarantine machinery.²

Nor is it in Philadelphia alone that examples of the kind are to be found. Dr. Pascalis, of New York, once a contagionist, became one of the most zealous advocates of the opposite doctrine. In accordance with his former belief he signed, in 1797 and 1798, the reports of the Academy of Medicine of Philadelphia, where he then resided, and published an account of the epidemic of the former of those years, besides furnishing a long letter appended to Folwell and Condie's history of the fever of 1798. His efforts as a non-contagionist are fully recorded in the *Medical Repository* of New York, of which he was long an able editor. Professor S. L. Mitchell and Dr. J. B. Beck, of the same city, may be cited as distinguished examples of a similar change. The latter, originally a contagionist, and as such, having reported on the epidemic of Middletown in 1820, publicly retracted his views after having had an opportunity of observing the disease on a larger scale, two years later in the city of New York.³

Dr. John Vaughan, of Wilmington (Del.), so early as 1800 (Jan. 4), and consequently before the public announcement of Dr. Rush's conversion, gave in his adhesion to the doctrine of non-contagion. In a letter addressed to Dr. Miller, of New York, and descriptive of the epidemic of his town in 1798, he says: "This hasty sketch will serve, I hope, to show the difference between implicit assent and investigation. To the former I have confessed myself a victim; to the latter I avow myself a convert; believing that a man deserves more credit for acknowledging than persisting in an error, particularly when the

zeal as to reiterate, without a shadow of proof, a charge of so offensive a kind against a man of whom, as an American, he should have been proud, must be painful to every member of the profession in this country. Nothing short of clear evidence of the truth of the allegation could have justified that course; and the reader will coincide in the conclusion that the appeal made to Dr. Hosack's lectures is not of a nature to afford support to the charge, considering that the extract from these lectures contains a statement which does not harmonize in any way with the silence observed on the subject in all the works prepared by that writer for publication.

¹ An Essay on the Disease called Yellow Fever, p. 378.

² Notes on Quarantine, Transylv. Journ. of Med., xii. 166.

³ See his Review of Townsend's Work on the Epid. of 1822, New York Med. and Phys. Journ., ii. 472.

welfare of society is, in some measure, involved in his opinion."¹ The epidemic which occurred in that town in 1802, and was described by Dr. Vaughan, furnished him with additional evidence of the propriety of the change.²

Cross we the Atlantic, we shall find Dr. Piguilem, Professor of Clinical Medicine in the Medical School of Barcelona, and formerly a decided contagionist, adopting the opposite doctrine, when the epidemic of that city in 1821 had furnished him additional means of observation. "I have never," he says, "hesitated, and never shall hesitate to abandon the opinion of yesterday, and adopt that of to-day, when the latter will be sustained by irresistible and convincing facts. When experience speaks, the imagination must be silent."³ M. Thiebault de Berneaud addressed a letter to Prof. Desgenettes from Leghorn, on the epidemic of that city, and therein avowed his opinion in favour of contagion and importation. Subsequent experience, however, on the *same spot* convinced him of his error, which he candidly and publicly renounced.⁴ Dr. Strobel, of Charleston, enters at large on the statement of Mr. Thiebault de Berneaud—whom he calls a clear and judicious writer—relative to contagion; but says nothing of his change of sentiment on the subject.

The results obtained in tropical climates are, perhaps, still more striking, for it annually happens that physicians go to the West Indies from Europe deeply nurtured in the doctrine of contagion, and find reason to adopt opinions diametrically the reverse of those they had before entertained, as soon as they have occasion to examine, by themselves, the origin and progress of the fever. The example of the medical men who accompanied Sir Ralph Abercrombie's expedition to St. Domingo in 1795, deserves notice. The fever that attacked the troops prevailed to an awful extent, and was regarded as non-contagious by every physician of the army—not one of whom (Sir William Pym, perhaps, excepted)—saw cause at any period of the epidemic to view it in any other light.⁵ "On the contrary," says Dr. Ferguson, "it unmade us, I may say, to a man, and cured us forever of the delusion that had been infused by all the schools and authorities at home."⁶ In another place, Dr. Ferguson remarks: "At first, every new comer, whether medical or otherwise, had his fears; or I should rather say, had the firmest belief in contagion; but with none did that prejudice remain beyond the year. It vanished, infallibly, as soon as he saw, and had

¹ An Account of the Origin and Causes of the Yellow Fever, as it appeared at Wilmington (Del.), in the Autumn of 1798, Med. Rep., iii. 368, 372.

² A Concise History of the Autumnal Fever which prevailed in the Borough of Wilmington in the year 1802: Wilmington, 1803, pp. 19, 22, &c.

³ Lettre adressée par M. Piguilem à M. Lâssis: "Sur les motifs de la rétractation comme partisan du système de la contagion;" Obser. des Sci. Med. de Marseilles, iv. 13; also, Ann. de la Méd. Physiol., i. 404.

⁴ Biblio. Physico. Econ., Sept. 1820, and Tablettes Univ. No. 1. See also Bonneau and Sulpici, pp. 98, 243, 244.

⁵ H. McLean, p. 78.

⁶ Med.-Ch. Rev., Jan. 1840, p. 302.

experience of the disease."¹ A similar course was forced on Dr. Bancroft, and almost all those attached to the hospital staff in the Windward Islands; on Mr. Young, Inspector-General; on Dr. Dickson, and, as he declares, generally, on others in the circles of his acquaintance; on Dr. Erly at Sierra Leone;² on Dr. O'Conner, of Trinidad, and Dr. R. P. Thomas, of Barbadoes.³

Such has been the ease with French physicians. Chambolle went to the West Indies a decided contagionist, and there soon found reasons to change his views.⁴ Dr. Lefort, physician of the Royal Navy of France, presented in 1815, a memoir to the Medical Society of Paris, in which he warmly defended the doctrine of contagion. This memoir was crowned by the society. Soon after, Dr. L. was placed at the head of the Military Hospital of Fort Royal, Martinique, where he had the amplest means to investigate the disease. Mark the result! On the first of August, 1819, he forwarded a second memoir to the same society, in which he formally retracted his former views, and, in all his subsequent writings, he has approved himself an ardent and able advocate of the non-contagion of the disease.⁵

Dr. Fournier Pescay regarded, in 1818, the yellow fever as a contagious and imported disease.⁶ After four years' practice in the West Indies, he followed the example of Dr. Lefort, and in a communication addressed to the Academy of Sciences,⁷ under date of 12th March, 1827, he reversed the conclusions he had adopted before seeing the disease. Dr. Vatable, at Guadeloupe, furnishes us with another example of the kind.⁸ Dr. Dariste states that, when he first reached the West Indies, it would have been difficult for him not to have believed the disease contagious; but adds, that as soon as he could regain his presence of mind and investigate calmly and accurately the facts which presented themselves to his observation, he became conscious of the error into which he had fallen, and all ideas of contagion were effaced from his mind.⁹

It may not be improper to remark, that the greater number of instances of change, if not all, which have just been cited, occurred under circumstances which tend to impart to them greater weight in the solution of the subject in question, and to prevent us from attributing them to any other cause than a sincere conviction of the absence of contagious property in the disease. The doctrine of non-contagion has never been regarded with a favourable eye by the public at large, or by governments, and he who adopts and defends it will nowhere find his interest to be thereby promoted. In some

¹ Report to James McGregor, Director-General of the Army Medical Department; Bancroft's Sequel, p. 181.

² Bancroft, Sequel, pp. 190, 243.

³ Rept. of Acad. on Chervin's Documents, pp. 23, 24, note.

⁴ Broussais, Annales de la Méd. Physiologique, xiii. 200.

⁵ See Journ. de Médecine, Nov. 1820.

⁶ Dict. des Sci. Méd., Art. Marais, xxx. 560.

⁷ Arch. Gén., xiii. 458, 459.

⁸ Report of the Academy of Medicine, p. 24, note.

⁹ Rech. Prat. sur la Fièvre Jaune, p. 41.

countries, indeed, he must expect to meet with opposition, and even with persecution, on the part of the public authorities. In France, until very recently, non-contagionists were regarded as little better than enemies of the human race, despised and pitied by the public, and opposed in every way by the government; and we all know the manner in which they were at one time treated in Spain; while their opponents were held up as the defenders of a salutary truth, and as such encouraged and rewarded. Even in this country, the former have but too often been held up, especially in former days, as wild theorists, and as such laughed at, ridiculed, and reviled. Taking these facts into consideration, we cannot but conclude that few, if any, physicians would feel disposed to subject themselves to such difficulties and forego the advantages resulting from pursuing a different course unless firmly convinced, by experience, of the erroneousness of the views they once entertained, and of the soundness of those of an opposite character. Add to this, that in all the cases mentioned, the change of opinion has been the result of a more extended sphere of observation and of a more mature experience in the disease, and is hence entitled to greater regard than if it had been the effect of after-thought, or based solely upon facts and arguments adduced by other writers. Now, how stands the case in relation to the changes of opinion adduced by the contagionists? Are the circumstances under which these changes occurred, the facts which led to them, and the reasons and arguments assigned in their support invariably and in all the instances cited of a character to inspire confidence?

A few references and statements will show that, in some, at least, of these instances, the attending circumstances are of a kind that contagionists cannot point to with effect, and regard as calculated to furnish strong aid to the cause they defend. Dr. François, who, as we have seen, was an anti-contagionist as late as 1818, became a violent contagionist on his being sent by the government to investigate the origin and nature of the epidemic fever of Barcelona in 1821. But, let it be remarked that the change must have taken place before his arrival on this new field of observation, and of course before he could have had an opportunity of investigating the point at issue; for we learn from the account of that epidemic drawn up by Dr. Pariset, and signed by Dr. François conjointly with the latter and Dr. Bally, that when the commissioners arrived, he (Dr. F.) had reason to regard the yellow fever as a contagious disease. Now, when the readers are told that, between 1818 and 1821, that physician did not see the fever anywhere, they will perceive that the change was not predicated on a more extensive and accurate knowledge of the disease; and as that physician was commissioned to Barcelona by the French Government, and as the latter, at that time, and long after, was not over-anxious that the fever should be found devoid of a contagious character, his motives for embracing views differing from those he had formerly advocated, may fairly be regarded as open to suspicion. Dr. Bally changed, after his visit to Spain in 1805, where he was sent with Dr. Nysten and others to study the disease then prevailing in some parts of that country. But he arrived *after* the epidemic had ceased—did not see a single case of the fever, and relied exclusively on the reports of the native physi-

cians.¹ Dr. Girardin who, as stated in the last chapter, affords another instance of conversion from the ranks of the non-contagionists, did not change until after his return to France from New Orleans, and necessarily without his having had further experience in the disease. The effects which this change produced on his former colleagues of the Medical Society of New Orleans are on record, and are expressed in terms calculated to show the little influence it can afford in the settlement of the question; while the tampering with the archives of the society for which Dr. G. was expelled from that body, does not exhibit a moral character which, even had his experience been more ample, could have entitled him to our respectful consideration.²

The circumstance under which Dr. Rochoux adopted views different from those he had before advocated and ably defended, are certainly not of such a nature as to inspire us with much confidence in the correctness of his conclusions even were they supported by stronger proofs than those he adduces. At the same time, we must do him the justice to say, that his conduct does not appear to have been dictated by motives of interest; inasmuch as while becoming a convert to contagionism, he denied then the fact of importation which the French Government that had sent him to Barcelona were desirous of establishing, and soon after his arrival in that city ceased to form part of the commission—thereby excluding himself from a participation in the remuneration and rewards which awaited the members thereof at home. Be this as it may, the following extract from the report of that commission will furnish us with a clue to his conduct: “As we approached Barcelona, the instances of contagion which were related to us in his presence, began to make him waver. Several times during the journey he proposed to us this singular alternative. The fever that prevails at Barcelona is either the yellow fever, or it is not. If it be the yellow fever it is not contagious, as we shall see together. If it be not the yellow fever, and the reigning disease be in some degree conta-

¹ Valentin, *Journal Universel des Sci. Méd.*, xi. 143.

² Extract from the Communication of the Med. Soc. of New Orleans to the French Consul; in Chervin, *Méd. Am.*, p. 176. See the document itself, in Chervin's *Petition*, &c., p. 109.

Extract from the Report of Committee appointed by the Medical Society of New Orleans to inform the French Consul of the opinion of the Society relative to the contagion or non-contagion of the fever:—

“Quant à vous, messieurs, votre opinion, basée sur une multitude de faits irrécusables, est, ainsi que la nôtre, depuis longtemps bien connue. La plupart d'entre vous l'ont donnée par écrit, motivée autant qu'elle doit l'être, au Docteur Chervin, lors de son séjour parmi nous en 1820. Ceux qui ne l'ont pas fait n'exerçaient pas alors à la Nouvelle Orléans; c'est pour cela seulement que leurs noms ne se trouvent pas parmi ceux des médecins qui s'empressèrent de lui témoigner qu'ils partageaient entièrement son opinion sur la non contagion de la fièvre jaune. Déjà la Société Médicale l'avait proclamé dans un rapport imprimé qu'elle publia sur l'épidémie de 1817; et à cette occasion, messieurs, nous dirons que ce n'est pas sans un grand étonnement, pour ne pas dire quelque chose de plus, que nous avons appris qu'un des rédacteurs de ce rapport, maintenant à Paris, s'était rangé sous la bannière des contagionistes, quoique depuis son départ d'ici, où il était d'une opinion opposée, il n'ait plus en occasion d'observer la fièvre jaune.”

gious, as I was not sent to study a disease of that nature, I shall separate from you and immediately retire.

“From the first moment of his residence at Barcelona, what he saw, appeared to him so conformable to what he had heard—he was so much struck with the rapid communication of the disease, and especially with the fate of M. Mazet, that, as early as the 14th (the commission had arrived on the 9th of October) he made up his mind to retire. He sought refuge at Gracia, where he proposed to place himself in quarantine, with a view to his immediate return to France. Since then, he has changed his mind several times, and has, finally, felt that he no longer formed part of the commission.”¹

From this strange narrative, the conclusion is natural, that M. Rochoux, who, though having seen the yellow fever in the West Indies, had never witnessed a wide-spreading and devastating epidemic, became alarmed at the intelligence of the rapid diffusion of the disease he was about confronting, and consequently lent a willing ear to the reports circulated, concerning its communicability; that his alarm increased tenfold when he found himself in the midst of the sick and dying, and that his fear getting the better of his judgment, he fled without taking time to inquire into the cause of a diffusion more rapid than he has been accustomed to. The result was inevitable. Having fled from the scene of danger, before the expiration of a week, from the impulse of an alarm which had begun to exercise its influence even before his arrival, and which attained the culminating point soon after he was placed in the focus of the infection; and having hastily concluded that the quick succession of cases could only arise from contagion, he was bound—whatever may have been the result of his subsequent experience—in order to sustain his character, to maintain that the disease was really propagated in that way, and as he did not wish to be accused of inconsistency in admitting, that a disease could be non-contagious in the West Indies, and contagious in Europe, he adopted the plan of maintaining that the fever of Barcelona was a totally distinct disease from the yellow fever of tropical climates.²

¹ *Annales de la Médecine Physiologique*, i. 547.

“M. Rochoux had accompanied M. Mazet to the house of the individual from whom the latter took the disease. While M. Mazet experienced the malaise, forerunner of the fever, M. Rochoux felt a sensation of constriction in the throat, which was well calculated to make one think he was about being attacked. Alarmed at the occurrence, he ran off and abandoned the commission, of which he henceforward refused to form part. Before leaving, he thought it his duty to declare, in opposition, let it be said, of his original ideas, that the yellow fever was, after hydrophobia, the most contagious disease with which he was acquainted.”*

² See *Dissertation sur le Typhus Amaril ou Maladie de Barcelone improprement appelée Fièvre Jaune*, par J. A. Rochoux: Paris, 1822, p. 60.

Recherches sur les Différentes Maladies qu'on appelle Fièvre Jaune: Paris, 1828. See also *Dict. de Méd.*, 1st edition.

M. Rochoux has endeavoured to explain the course he pursued at Barcelona in 1821. He states, that he was requested by Pariset to proceed to Tortosa to investigate the disease of that place; that he left Barcelona for that purpose and slept at San Gervario where

* Henry, *Malheurs de la Catalogne*, pp. 140, 141.

On the 19th of July, 1822, M. Guyon furnished the following certificate to Dr. Chervin: "After a residence of eight years in the West Indies—a lapse of time during which the nature of my functions has furnished me with the means to see and closely study the yellow fever, I declare that I have never met with a single case calculated to raise a suspicion that the disease can be communicated in any way whatsoever; I therefore think—without any reserve—that the yellow fever is not contagious in the Antilles, and consequently that it is not susceptible of being imported in any place whatsoever. I think, finally, that examined in a general point of view, that disease originates from causes inherent to the climates where it has heretofore been observed—in the torrid zone, as also in the temperate zones of both continents."¹ Wishing to investigate the subject as thoroughly as he possibly could, M. Guyon instituted a series of experiments on himself,² the results of which were to confirm him in the opinion of the non-contagiousness of the fever which his clinical observations had led him to adopt. Four years after the date of the above certificate, 1826, while in Paris, he continued firm in the belief as therein expressed; but the events of the epidemic of Gibraltar, in 1828, caused him to discard suddenly a doctrine which his long experience and numerous experiments had led him to regard as decidedly settled; and in an essay which appeared in 1830, endeavoured to prove that the disease had been imported into that place from the West Indies, and had proved contagious.³

But the value of this complete change will be duly appreciated when it is found, that the events of the epidemic of Gibraltar were not witnessed by M. Guyon himself, inasmuch as he was at the time stationed at Cadiz, or on his way thence to France; and that so far from being sufficiently conclusive, on the score of contagion, to account for the effects which they seem to have produced upon him, those events exercised an influence of a very different kind on all but two of many English physicians and surgeons, both civil and military, in Gibraltar—Drs. Broadfoot and Barry—to say nothing of Sir W. Pym, who never fails to see contagion everywhere.⁴

he awaited some time for a good opportunity to reach his place of destination. There he was taken sick and went to Sarria, a village in the vicinity of Barcelona. After his recovery, he discovered that he could not (owing to certain quarantine regulations) cross the Cordon. So he remained where he was till the 7th of November. On the 8th, he returned to Barcelona and remained. He admits, however, that, at all events, he would not have staid at Barcelona, where the commission had proceeded against his advice; and that had not the idea of the visit to Tortosa been suggested, he would have left the place a few days later; and it is a well-known fact, that though he did return to Barcelona, he did not do so before the cessation of the epidemic. This justification! was published in the *Nouvelle Bibliothèque Médicale* (May, 1824), under the title of *Notice sur l'Epidémie de Barcelone*, and republished in the *Marseilles Med. Journal*, viii. 97.

¹ Chervin, "De l'origine locale et de la non-contagion de la fièvre jaune qui a régné à Gibraltar en 1828," p. 6.

² Lefort, "De la non-contagion de la fièvre jaune," p. 126, &c.

³ Notice Méd. sur Gibraltar suivie de quelques réflexions sur l'origine de la maladie de cette ville en 1828. An. Mar. 1830. 2d partie, tome 1.

⁴ Chervin, loc. cit., p. 16, &c.

Nor can we think that the conversion of even Dr. Dickson (whom I am far from classing in the same category with the preceding) is likely to carry along with it the influence which might have been expected, considering the acknowledged talents, integrity of purpose, and high professional position of that gentleman. In his letter to Dr. Strobell, Dr. Dickson says that he believes the yellow fever to be transmissible or communicable from one city to another, provided the general circumstances are similar or analogous: "Of its contagiousness (yellow fever)—using the word in the limited sense—its propagation from one subject to another, I have never witnessed any example, and, until very recently, should have denied its possession of this property in our climate." "The events of the last summer (1839), however, have inclined me," he adds, "to entertain an opposite opinion"—and a little further on, he says: "But an impartial perusal of the statements of Pym, Blane, Arejula, Wistar, Hosack, and Monette, has satisfied me that it deserves to be ranked among contagious diseases" (p. 128). And yet those very events of 1839, which served to shake Dr. Dickson's long established convictions, were so little indicative of the reality of contagion in the minds of the medical men of Charleston who went through the epidemic, as to be discarded by them; while he himself, on whom they produced so important an effect, could not have seen anything to justify the change in question, since he acknowledges that he never witnessed a case of contagion of the disease during that season or before, *i. e.* its propagation from one subject to another.

Let it be remarked, that the statements and arguments of the above-mentioned contagionists, which have exercised so marked an influence on Dr. Dickson, have been over and over again successfully opposed and refuted by a number of writers, entitled to entire confidence, and who were perfectly conversant with the facts described and commented upon, and whose writings are doubtless familiar to him. Nor does it appear that the statements of the leaders in the cause which he now advocates exercised always the same influence in this sense upon him as they have recently done, for in a former publication, besides saying much in opposition to what they state in favour of importation, he remarks, in speaking of Hosack and Pym: "The ingenuity of the first, and industry and strength of reasoning of the latter of these writers, have brought forward a great mass of facts and arguments in support of their opinion. Yet, I cannot help thinking that, even from their own works, evidence sufficient to prove the contrary may be collected."¹ In other words, works which formerly were thought to contain evidence sufficient to prove that the yellow fever is not contagious, are now brought forth as having satisfied him that the same disease "deserves to be ranked among contagious diseases." Since the appearance of Dr. Strobell's volume, Dr. Dickson has become a decided contagionist; but whether the facts and reasons upon which his present belief is founded, are of a different order from those which induced him to change sides in the controversy, and whether they would afford the means of settling the point at issue, and thereby impart

¹ Dickson on the Yellow Fever of Charleston, 1817, Philada. Med. and Phys. Journ., iii. 270.

influence to his conversion, I am unable to say, as they have not thus far been communicated to the public. Even in his recent volume, we look in vain for any details on the subject, the author contenting himself with assuring his readers that the question of the contagiousness of the yellow fever "may be regarded as having been as fully settled in the affirmative as we can ever hope to see any question of a pathological character" (p. 260).

Contagionists not particular in the Choice of Facts.—In closing this chapter, it may not be out of place to notice, that the advocates of contagion and importation are not over fastidious as to the nature of the facts they adduce in support of their views, or to the sources whence they derive those facts; and, in some instances, as to the degree of fairness with which they make their selections. Pariset, Bally, and François, who, as already stated, were sent to investigate the origin of the epidemic of Barcelona in 1821, evidently derived all the historical part of their report from the members of the Royal Academy of that city, from the account published by the Ayuntamiento (municipal council), from the report of the Superior Junta of Health, and especially from the medico-political member of the last-mentioned body, Dr. Bahi. They, however, nowhere refer to the sources whence they derived their information, and presenting only such portions of the statements as suited their purposes and accorded with their particular theories, ignore completely all that was adduced to disprove those same statements. They take good care to avoid mentioning, or they slur over, the fact that the physicians of Barcelona were far from being unanimous on the question of the contagiousness of the disease; that the majority of the members of the sub-delegation of medicine admitted its local origin, and were non-contagionists; and that of the fourteen physicians composing the special commission selected from among all the medical corporations of the city and the Royal Academy of Medicine, nearly one-half were non-contagionists and advocates of a local origin.

Nothing embarrasses a decided contagionist. Let some cases occur suddenly in a quarter of an infected city where the disease has not yet prevailed, or let the fever break out in parts which before had been, for a longer or shorter space of time, healthy, and though there be no proof of communication, direct or indirect, between the persons so attacked and those already contaminated, he is ready with an explanation. If he does not account for the former in the way suggested in this city—*i. e.* by the assistance of an "epidemic influence" creeping stealthily from the original seat of the imported pestilence—he does so by pointing out, or stating it as very probable, that some one from the infected district has visited the place where the mysterious cases present themselves, and left there the seeds of the future sickness; or he shows that a case of real or supposed yellow fever, caught in a sickly place, had occurred in the neighbourhood months before, and there left the foundation for the subsequent development of the disease. It matters not what interval of time has elapsed between the supposed introduction of the poison and the advent of the new cases, or whether the disease has broken out in the house where the original case had occurred, or at a distance. As regards the unexpected outbreak of the fever in a place heretofore healthy,

some vessel can generally be found to explain the occurrence, or some luckless individual, or trunk of soiled clothes, or box of merchandise, is sure to have arrived from an infected spot. At all outbreaks of the disease, in any locality, it is possible to discover that some one has arrived there, with or without sickness, a few days, weeks, or months before, from an infected or suspicious spot, or after having communicated directly or indirectly with one who had been so exposed. Nor is it an uncommon thing for a vessel from southern or West Indian ports to arrive in this city or in other places within the yellow fever zone, during the summer season, with more or less foulness in her hold, or with bilge-water of no great purity. In the first case, the luckless traveller is easily convicted of having conveyed the poison; and in the other, as the fever, when it appears, usually does so along the wharves, it cannot be difficult to trace some connection, apparent or real, between the arrival of the vessel and the advent of the fever. But the etiological inquirer cannot rest satisfied with proofs of this kind. Something more is required to give foundation to the creed of importation, especially when we take into account the extraordinary nature of some of the facts adduced in evidence of it; for, on examination, we shall find that the advocates of this creed not only care little about the time which elapses between the supposed introduction of the poison and the manifestation of the fever, but are not usually very particular as regards the place whence that poison proceeds. A few specimens may not be inappropriately referred to in this place. In 1843, the yellow fever invaded a portion of Charleston Neck. Dr. Hume explains the occurrence by the arrival there, from the Havana, of an individual sick with fever *six weeks before*, and who, if he communicated the disease at all, did so not to any one in the house where he lodged and received attention, nor to those from the city who visited him during his illness, but to two Dutchmen who resided on the opposite side of the street, and had not had the least intercourse, direct or indirect, with him. The same writer tells us that Charleston was preserved in 1853 from the yellow fever by the energetic measures adopted by the Board of Health; and, in proof of this, cites the following facts: "Captain P——, of the *Ellen Goldsborough*, arrived from Baltimore on the 30th of August, sickened on the 4th of September, was sent to the Marine Hospital on the 6th, was declared to have yellow fever, and on the 7th was at the Lazaretto, where he died on the 10th. On the 23d of September, Captain P——, of the *Berkelew*, was reported under similar circumstances, and was immediately sent to the Lazaretto, where he also died. Thus ended both attempts at invasion."

Nothing is said to show that the vessels in question were infected, and that the individuals took the disease on board. We are not told whether the *Berkelew* had arrived from a sickly locality; but in reference to the *Ellen Goldsborough*, we know that she could not have brought the disease concealed in the body of the captain, inasmuch as no yellow fever existed in Baltimore at the time of her departure. We certainly have no proof, then, that the fever in these cases originated in the vessels in question, for no one else was taken sick on board before or after their arrival. We are positively sure that in one at least it could not have been derived, in any shape or form

whatsoever, from the port whence the vessel came. Why, then, are not these considered in the light of sporadic cases, which Dr. Hume himself, like every one else, acknowledges to occur at Charleston, as they do elsewhere, and which are of course of domestic origin? And why refer them to importation, seeing especially that they presented one of the peculiarities which Dr. Hume thinks characterize sporadic cases, that *they did not extend and infect localities?*¹

¹ The following extract from a late communication, by Dr. Hume, on the yellow fever of Charleston, is worth reading in connection with the subject before us:—

“There can be little doubt in the mind of the most non-contagious advocates, that the transportation of an infected house from Havana to Charleston, in August, would be a dangerous experiment. The visitors to that house would be as liable to take the disease as the visitors to Havana. It contains a portion of the same air, and the matter, whatever it may be, which is capable of producing the disease, and would be as efficient in producing the disease in Charleston as it would have been had it remained in Havana. Infected houses are common in Charleston during the prevalence of the epidemic; and, in fact, the epidemic depends upon the successive infection of few or many houses, and the infection is manifested by the more or less rapid succession of the disease in the residents, involving nurses and visitors. Infected cities and infected districts are commonly admitted terms, with a well-understood meaning; the same idea can be applied to individual houses with the same correctness. A healthy person, residing in a healthy portion of the suburbs of Charleston, visits a sick friend in the city, returns home, and within a week takes the same disease. Can it be said that the disease was taken from the sick person or from the infected house? There will be a diversity of opinion, and a single case of this kind, or even a number of similar cases, cannot positively determine the question. That the disease was contracted while there is evident, but whether from person or place is not certain. On the 18th of August, 1854, Mrs. Gorman, residing in Calhoun Street, nearly opposite Anson, died of yellow fever in the afternoon. On the morning of the same day, her friend, Ann Corran, residing in Mill Street, Cannonsboro’, visited her, found her assistance necessary, remained, nursed, laid out, and finally attended her funeral; she returned to Mill Street the next afternoon, and was herself laid out from the same disease on the 3d of September. On the first appearance of the disease, she was sent to the poor-house, where she died, leaving her own house uninfected, as was apparent from the continued health of the other inmates. Ann Corran’s disease was evidently contracted during her visit to Mrs. Gorman, and must have been from Mrs. Gorman, or Mrs. Gorman’s house. If she had visited the house without nursing or seeing Mrs. Gorman, the case would be decided—the house was infected, and she, Ann, took the disease from the house. Now it does appear to be incredible that a house should communicate a disease, yet the fact is of daily occurrence. An Irish woman, living opposite to the Marine Hospital, was in the habit of obtaining water from the pump in the yard; she never saw the yellow fever patients, she never entered beyond the basement, yet she was the first case in Franklin Street. She must have taken the disease from the infected premises, for she never came in contact with the sick sailors. That the infection of a building may have a wider range than its own yard, is evident from the case of the prisoners in the jail. The first prisoner infected resided on the south side of the jail, distant about one hundred and seventy feet from the hospital. He had been confined for eighteen months, and must have received the disease in his own quarters, as he had never been out of them, and there was no other more probable source of infection than the Marine Hospital, known to be infected from the number of cases, and the effects upon the inmates contained for other diseases. Immediately after the case of the Irish woman in Franklin Street, several other cases occurred in nearly adjoining houses, showing an extension of the infected region. Such extensions occurred in many parts of the city, and were frequently observed not only by the physicians, but by the inhabitants. The development of a case

As already stated, the fever of Bahia, in Brazil, was by many attributed to an arrival from Baltimore, where fever did not prevail that season. By others, it was unhesitatingly traced to a vessel which had arrived from Canada! Others, again, thought it came in a vessel from New Orleans, forgetting that this vessel had left the United States in the winter season. The fever of Rio Janeiro commenced in February, 1850, on board a vessel from this city, and soon spread in other ships and on shore. Of course, this vessel was confidently looked upon as having introduced the disease, though it left here in the winter, and it never had any sickness on board till after it arrived at Rio. I have elsewhere referred to the authority of Pariset for the fact that the epidemic of Cadiz, in 1819, came direct from Calcutta. True it is that the yellow fever poison is not generated at Calcutta, or, at any rate, that the disease did not exist there in 1819; it is true, also, that during the passage from Calcutta to Cadiz there was no one sick on board. But what of that?—if there was no yellow fever there, cholera prevailed extensively, and, according to the eloquent Frenchman, a disease which in India is cholera, and not contagious, may be converted into a contagious yellow fever when transplanted into Spain by healthy men in a healthy ship; and, in proof of the importation on that memorable occasion, he remarks that the offending vessel arrived at Cadiz on the 26th of June, and the yellow fever made its appearance on the 29th of July—*only* thirty-three days after—that those with whom the mate and two of the sailors lodged died, and that these men soon after met with a similar fate.

From the same apostle of contagion we learn that after the development of the yellow fever in the isle of Leon—a village near Cadiz, where it first broke out in the afore-mentioned year—the “disease appeared in such places only where it was carried by persons who had contracted it in its primary source.” He tells us, for example, that it was positively introduced into Cadiz from Leon and Chiclana, and into Santa Maria by a woman who escaped from Leon. Xeres was infected in various ways—first, by a soldier from Leon; secondly, by a woman also from that place, who communicated the disease to two men living in the same house, though she herself appears to have remained in health; and thirdly, by an Italian, likewise from Leon, who, on his arrival, sickened and died. In 1813, a vessel conveying the

in a house was followed by other cases of inmates of the same house; it then extended to the adjoining houses. Thirteen cases successively occurred in the jail, without direct communication among the prisoners, the partition walls and different stories offering no impediment to the multiplication of the cases.

“From these and other observations, we are induced to infer that the introduction of a case of yellow fever into a house infects the house in such a manner that the other inhabitants, with or without contact with the patient, if susceptible of the disease, will take it. That the inhabitants of the neighbouring house are also liable to it, and take it, either together, as if from a common cause, or in succession. That in the case of the introduction of yellow fever into a house, the extension of the disease is produced through the infected air, that the air of the house not only infects those in the room with the patient, but those in other rooms, and even other neighbouring houses. The spreading of cases through a whole neighbourhood can be explained on no other principle.”—*Charles-ton Med. Journ.*, x. 6, 7.

Viceroy of Mexico arrived at Cadiz from Vera Cruz, "in excellent condition." But some of the crew being sent to the nephew of that functionary, he caught the disease from them, and from him it spread over the whole city. In 1800, Xeres received the fever from a regiment of soldiers, who, on their march from Cadiz, remained one night within its walls: the soldiers were all in good health. A young lady, residing in a healthy part of Cadiz, was infected by conversing one night through a window with her intended husband, who lived in a contaminated locality and attended on his sick parents. She died, while he escaped the disease.

These facts, which form worthy pendants to the case of the Jew peddler already mentioned, and are recommended to the attention of Dr. Hume, have been considered as sufficient proofs that the fever, in its progress on the occasions stated, observed the march of a contagious epidemic, and that Cadiz, Xeres, and many other places were indebted for the disease to the circumstances enumerated. How can we refuse to admit such proofs of the contagiousness of the fever, when we are informed of the following facts? Pariset relates, on the authority of Dr. Flores, that the disease was introduced into Agamont in 1804, by a fisherman, who, having sold his fish to a ship-of-war from Gibraltar—which place was then infected—received in exchange some cheese and biscuit. He and his family were the first victims of the fever, and from them the disease spread and became epidemic. Contagionists of pure water are of opinion that the fisherman caught the disease by eating the cheese and biscuit impregnated with the poison, and not from the clothing of the seamen, since his intercourse with them was too transient and the air too rarefied (for the man does not appear to have gone aboard) to justify the belief. Such, indeed, is the subtle nature of the poison, as manifested in Spain, that even after the death of a patient, the effluvium arising from his remains was known to kill birds flying high up in the air!¹

CHAPTER XV.

SAME SUBJECT—CONTINUED.

The Disease appears at Determinate Periods of the Year.—The yellow fever appears under circumstances which would of themselves, were there no other proofs at hand, render the idea of its contagiousness very problematical, if not absolutely inadmissible; and is under the influence of some, at least, of the ordinary laws which govern epidemic and endemic diseases, and which, for the most part, are incompatible with contagion. Thus, the fever has always appeared, with greater or less precision, at determinate periods of

¹ See for all this, Pariset, *Obs. sur la Fièvre Jaune, Faites à Cadiz en 1819*, pp. 56, 65, 66, 78, 88, 89.

the year, and, after prevailing some months, disappears—sometimes in a gradual, at others in a sudden manner—with as much regularity as it had exhibited in its outbreak. The history of our epidemics, from the first to the last, furnishes ample proof of the reality of this tendency; while a reference to the occurrences that have taken place in other parts of this continent, as also in Europe and in tropical climates, will show that there also the regularity in question has invariably been observed. As much as any other it may be viewed as an autumnal disease, seldom, in temperate regions, appearing before the middle or close of summer, and ceasing as an epidemic at the accession of winter. Its existence in early spring has never been observed, and if it shows itself at the opening of summer, which has seldom been the case, the occurrence took place under peculiar circumstances of atmosphere which approximate that period to the usual condition of the succeeding months. In illustration of this it may be stated that, in this country it has only occasionally commenced as early as June. Such was the case in this city in 1699, 1799, and in Charleston in 1837. We even find that in the year 1732 it appeared in the latter city as early as May. In 1852, one death by yellow fever was reported in May, and another in June. In 1819, the disease broke out in this city about the end of June. Such was the case in New Orleans in 1848, 1853. But instances of this kind are rare, and when they occur, the progress is at first slow, and the disease presents more the character of a sporadic than of an epidemic visitation. More frequently, the fever breaks out about the middle or end of July, often in August, and sometimes as late as September. Thus it began its epidemic career in the course of July, in Philadelphia, in 1747, 1794, 1797, 1802, 1803, 1805, and 1820; in Norfolk, in 1800, and 1821; in Providence (R. I.), in 1805; in Brooklyn (N. Y.), in 1809; in New York, in 1799, and 1803; at Woodville (Miss.), in 1845; in New Orleans, in 1817, 1820, 1827, 1830, 1833, 1837, 1839, 1840, 1841, 1842, 1843, 1844, 1847, and 1853; Rodney, in 1847; Boston, 1798, and 1819. Our epidemics of 1762, 1793, 1798, commenced in August, as did also those of Charleston, in 1732, 1739, 1745, 1748, 1849, 1852; of Baltimore, in 1798; of New York, in 1791, 1797; of Providence, in 1798, and 1800; of New London, in 1798; of Boston, in 1819; of Wilmington (Del.), in 1798, and 1800; of Natchez, in 1817, 1819, 1823, and 1825; of New Orleans, in 1818, 1819, 1823, 1824, 1830, 1834, 1835, 1836, 1838; of St. Augustine, in 1839; of Mobile, in 1819, 1843, and 1847; and of Vicksburg, in 1847. In Gibraltar, Leghorn, and the various cities of Spain where the fever has prevailed—as Medina, Sidonia, Malaga, Antiquar, Cadiz, Barcelona, Xeres, &c., the period of appearance has varied from July to September; never earlier nor later.

In the West Indies, and on the African coast, it makes its appearance, as might easily have been foreseen from the greater precocity of the hot season, at an earlier period than it usually does in this country and in Europe; while in some years, owing to the greater encroachment of hot weather on the autumnal months, it breaks out later than it does in temperate latitudes, the period varying from April (or even earlier, as was the case at Granada,

in 1793, when it showed itself in February) to the middle or close of September.

But whatever be the difference of time at which the disease appears in the several latitudes within the zone of its usual prevalence—sooner in tropical, and later in temperate regions—the difference depending on meteorological conditions to which attention has already been called, certain it is that the period at which it breaks out in each of those regions is sufficiently similar to justify the statement made in reference to the regularity of its epidemic or even sporadic manifestations.

When once established, it continues to prevail in Europe or this country during three or four months, and ceases as an epidemic, or even altogether, in October or November—seldom earlier or later.

The Disease not appearing annually is no Proof of its Contagious Character.—Some of the advocates of the contagious character and exotic origin of the yellow fever among us, derive, as we have seen, an argument in favour of their views from the circumstance of the disease not appearing every year, though the nature and condition of the soil, the temperature, and other external influences remain, as they assert, much the same every year. The same reason has been assigned as regards the fever of other climates, as will be found on turning to the work of Sir W. Pym, who attaches much importance to this occasional or frequent exemption in his endeavour to show that it proceeds from Africa. If we except this writer, and a few others, every physician conversant with the yellow fever of the West Indies from the days of Towne and Desportes to our own—whether he be a contingent contagionist or not, has admitted that the disease, whatever be the degree of concentration or malignancy it assumes, is a native of those islands. Several of the most uncompromising contagionists themselves have recognized the fact; and the impossibility of connecting its appearance with a direct communication with Africa, conjoined with other circumstances which it is needless to repeat, can leave no doubt in the minds of all qualified and unprejudiced inquirers on the subject. And yet all know and acknowledge, that in tropical regions—the natural birthplace of the fever—it is far from showing itself annually in the same places.

Desportes, who, as already stated, does not appear to have believed in importation, was aware also that it was not a constant resident in St. Domingo; for we have seen, on his authority, that in the fourteen years he practised in the island, the City of the Cape was free from it five. If it appeared at all, it was only sporadically. Frequent, and much longer intervals have been noted in Martinique, and other West India Islands. Indeed, it is impossible to point out a spot within the tropics where the fever may fairly be said to be a constant resident. Not even is the western coast of Africa so afflicted. Fever may prevail there with more or less severity every year, but not true yellow or Bulam fever. And yet this should be the case before the circumstance of the exemption in question can be adduced in proof of the exotic origin of the fever when it appears in temperate regions—or, indeed, in any locality where it only occasionally makes its appearance. The ex-

emption is easily explained, for there is required a certain concurrence of circumstances, local and atmospheric, associated with a peculiar constitution of the air, to produce the fever; and we all know that these are not always present. Such being the requirements in tropical regions—it is difficult, if not impossible, to perceive why it should not be so in our latitudes. If, therefore, in the former regions, where the disease is acknowledged to spring up from the operation of domestic agencies, long intervals between the epidemic manifestations of the disease are found to occur, it would be unphilosophical to deduce from the fact of similar periods of exemption being observed here and elsewhere within the yellow fever zone, an argument in favour of the foreign origin of the disease when it happens to break out in the latter localities.

To affirm that the reputed causes of the yellow fever exist permanently, and are equally operative every year, whether the fever breaks out or whether it does not make its appearance, would be to advance what every one, who takes the trouble to investigate the subject, knows to be unfounded. But even were it otherwise, medical men are, or ought to be, aware that in order that certain diseases should appear, and especially that they should assume the epidemic character, other contingencies, not as appreciable to the senses as the causes above alluded to, must lend their assistance. Be this as it may, it is doubtful whether the exemption of this and other cities of temperate latitudes, or of any other place, from the yellow fever during one, or a few, or many years, is more easily explained on the supposition of the possession by the disease of contagious properties, and of the latter being due, when it appears there, to importation from some infected spot. Can we, admitting the latter doctrine to be well founded, understand how the commercial relations of our maritime cities, or those of the South of Europe, could continue uninterrupted with tropical regions, in some parts of which the yellow fever is endemic, and where it exists almost annually either in one or in many places, and yet that the fever should not appear in the former during many years, and then appear, as it did with us, several seasons in quick succession, though the means of prevention remained unchanged, or were more stringent at those sickly periods than they had been before? We are not to suppose that, during those periods of exemption, no vessels arrived from infected ports, or that those that arrived did not contain occasionally, if not often, persons labouring under the disease, or goods or cloths impregnated with the poison. We know that such things have occurred here and elsewhere over and over again; and if, notwithstanding, the fever—if contagious and always imported—has not extended, the explanation of this exemption is just as difficult as that offered by those who oppose the exotic origin of the disease.

Let it not be said in support of the contagious character and exotic origin of yellow fever, that even in many tropical climates where it now often prevails, there was a time when it did not do so at all, and that its first appearance coincided with the arrival of vessels from infected ports. I would remark, that the fact of such arrivals has not always been proved—that in the few instances in which it was otherwise, there is strong reasons for regarding the vessels as guiltless of the charge of having been the cause of the outbreak of

the disease. But admitting that there had always been arrivals, the exemption from the disease during a long or short period of time, may be explained without having recourse to the circumstance that such had not been the case.

Humboldt, though not a professional writer, may be advantageously consulted by those who wish to be enlightened on the subject. After very properly remarking that sporadic cases have probably occurred in tropical regions ever since persons born in northern latitudes have exposed themselves to the climate and exhalations of the torrid zone, the great traveller adds: "We cannot be surprised that at a period when communications between the old and the new continent were limited, and when the number of Europeans who frequented the Antilles was restricted, a fever which attacks none but non-acclimatized individuals should have attracted, to a very limited degree, the attention of European physicians. In the 16th and 17th centuries, the mortality was necessarily smaller; 1st, because at that period the equinoxial regions of America were visited only by Spaniards and Portuguese, who are less likely from peculiarities of constitution to experience the noxious effects of a very hot climate, than the English, Danes, and the other inhabitants of Northern Europe who now frequent the West India Islands; 2d, because in the island of Cuba, in Jamaica, and in St. Domingo, the early settlers were not collected together in cities as populous as those erected since then; 3d, because at the period of the discovery of continental America, the Spaniards were less attracted by objects of commerce towards the coast which is usually hot and damp, and settled in preference in the interior, on elevated plateaux where they found a temperature analogous to that of their native country. Thus, at the commencement of the conquest, the ports of Panama and Nombre de Dios, were the only ones where, at certain periods of the year, strangers collected in large numbers. Hence, so early as 1535, a residence at Panama was as much feared as is now a residence in Vera Cruz, in Omoa, or in Porto Cabello. It is impossible to deny, in presence of facts adduced by Sydenham and other excellent observers, that, under certain circumstances, the germ of new diseases may be developed; but nothing proves that the yellow fever has not existed for several centuries past in equinoxial regions. We must not confound the period at which a disease has been first described in consequence of its prevailing extensively in a short space of time, with the period of its first manifestation."¹

Contagious Diseases are not as regular in regard to Period of Outbreak, Duration, &c.—The regularity as to the period of outbreak, duration, and termination thus found to manifest itself in yellow fever, would seem to establish a line of demarcation between that disease and those arising from and propagated by contagion; for experience teaches, that if these exhibit this tendency at all, they do so in a much less marked degree. They appear and prevail indiscriminately at various seasons of the year—in winter or summer—or at any portion of these seasons. This fact is sufficiently attested with regard to the smallpox and other exanthematous complaints, which, though

¹ Nouvelle Espagne, p. 754.

prevailing, doubtless, more usually in cold than in hot weather—heat being a great destroyer of contagion—often exist throughout the year. I am aware that the plague will be cited as an example of a contagious disease, whose periods of outbreak and termination and the length of time during which it prevails, are as regular as those of the yellow fever. But, in view of the evident tendency existing at this day, in the minds of accurate and experienced observers, to strip that disease of the contagious property which had long been ascribed to it, or at least to circumscribe it within the narrowest possible limits, I do not feel disposed to regard that example as militating against the distinction I have endeavoured to establish.

The long intervals observed occasionally between the epidemics of yellow fever, not only in countries and towns situate beyond the tropics, and where the causes of a domestic nature cannot be supposed to be frequently in operation, but in those where such causes are admitted to exist annually, and where the fever has, at times, assumed the garb of an endemic, cannot be adduced in support of the contagious character and exotic origin of the disease. For, were such the fact—were irregularities in the epidemic outbreaks of the disease, its sudden appearance in some seasons after periods of repose of greater or less duration, to be regarded as leading to that conclusion—we should be obliged for the same reason to suspect other forms of fevers of kindred nature, to be endowed with a contagious character and to be of foreign origin too, while, nevertheless, they are admitted, by the almost unanimous consent of the profession and of the public at large, to be destitute of such a character, and to owe their birth to the operation of domestic causes. In a word, whatever truth there may be in the statement that a similar irregularity is observed in the epidemic manifestations of diseases, the contagion of which admits of no doubt, that irregularity is not the exclusive attribute of such complaints, inasmuch as it is observed in other fevers which are not contagious and not transportable from place to place.

Every one knows in this country that although the bilious remittent fever—the endemic of many localities from one extremity to the other of our vast republic—appears in such places, to a greater or less extent, every year at stated periods, seasons occur in which it does so in so trifling a degree as to attract little notice; and others again when it does not show itself at all. Besides this, it is a fact, placed beyond the reach of doubt, that the disease is sometimes observed to spread extensively over a large expanse of country, appearing in places that had been for several, or many years, almost or completely exempt from its ravages, attacking, wherever it shows itself, a large number of individuals, either simultaneously or in quick succession, and causing a great mortality. The extensive epidemic of 1780, described by Dr. Rush (ii. 231), as occurring in this city and the vicinity—the equally extensive one mentioned by Dr. Bond,¹ and the still more wide-spreading epidemic of 1822, and a few succeeding years, when the disease swept over large tracts of country, in many portions of which it had not been observed

¹ North Amer. Med. and Surg. Journ., iv. 269.

during a long series of years, are familiar to every American medical reader. We know, too, that in our southern as well as in our southwestern States, where the fever is more strikingly endemic, the autumnal months are much less sickly some years than at other times—the disease almost disappearing, and presenting a milder character; while, in other years, it assumes the appearance of an extensive malignant epidemic, and spreads far and wide. On the western coast of Africa—the birthplace of true yellow fever—according to some, the same changes have been observed. So also in different parts of Europe—England, France, Spain, Italy, as well as in Batavia, India and other parts of Asia. In the West Indies, many facts might be adduced in proof. Chisholm, in the 18th volume of *Duncan's Medical Commentaries*, describes an epidemic of remittent fever which occurred in the vicinity of St. George, Grenada, in 1792; on which occasion the disease spread in an unprecedented manner, and attacked spots heretofore viewed as salubrious. The following remarks by Dr. Imray, in relation to the endemic of Dominica, will apply equally to the fevers of other islands: “The causes of our endemic fevers reside permanently in some localities; but in certain seasons, remittent and intermittent fevers become prevalent throughout the island.” “Though the causes from which periodic fevers originate are permanent in the country, yet they do not always operate with the same energy. Sometimes these morbid agencies remain in almost a quiescent state, and fevers occur comparatively rarely. At other times, they manifest unusual activity, and then we have remittent and intermittent fever in abundance.”¹

We have seen in a former chapter that these fevers have been sometimes regarded as possessing contagious qualities; but we may safely affirm that no one, now-a-days, dreams of harbouring such a notion in this country or in the West Indies, and that few authorities can be cited in favour of it in regard to the fever of Europe. Nowhere do we find them believed to be transportable from one place to another. Everywhere they are viewed as due to causes appertaining to the localities where they appear and prevail. Regarding these points as placed beyond the reach of controversy, and taking into consideration the fact that the same irregularity noticed in the epidemic manifestations of the yellow fever, is observed in the remittent fever of tropical and extra-tropical regions, it would be the height of impropriety to cite that irregularity, so far as regards the former disease, as an argument in favour of its contagious character and exotic origin; for, if it proves these in the case of the one form of fever, it ought to prove the same in the other form, while, if it occurs in the latter, which we know to be destitute of contagious properties, to arise from local causes, and as a natural consequence to have no connection with the existence of such properties, it cannot indicate another and different result so far as concerns the yellow fever.

Let it not be said that the endemic remittent, if it does not exist epidemically every year, does so at least to a certain extent, or sporadically; that this

¹ Observations on the Character of Endemic Fever in the Island of Dominica, Edin. Journ., lxx. 284, Oct. 1848.

persistence indicates a persistence of causation, and that its epidemic manifestations are only the effect of an aggravation of causes always existing, and which may be pointed out; for in the West Indies and some sections of our southern States, the yellow fever exists in the same way every year, or nearly so. If the epidemic extension of the former affords no proof of contagion and importation, we have ground for entertaining the same belief as regards the yellow fever, while the persistence of the fever, as exhibited by its sporadic appearance, must indicate also persistence of cause. The fact in question is explainable on different principles, as we shall see in a subsequent chapter.

The Disease becomes milder by continuance.—By continuance, the yellow fever gradually expends its force and becomes milder, so that in the progress of time it is overcome by subordinate diseases, and ultimately driven out of the field. To this subject attention has been called in an early chapter, when it was shown that in the beginning of an epidemic, judging from the proportion of mortality to the number affected, the disease assumes its most violent character, that progressively it becomes more tractable, till at the close of the season it is comparatively mild and curable. That this was not observed in 1820, when the cases which occurred at the close of the epidemic were of a more violent and deadly character than those of the preceding months; that a similar result was obtained in New York two years after, in Baltimore in 1819, and in Natchez in 1837; and that other instances of the kind may be found to have occurred elsewhere, is true; but such instances are of rare occurrence, and form exceptions to the rule. Indeed, during the very epidemics cited, the proportional mortality diminished sensibly, the number of mild gaining ground on that of severe cases until the very close of the season, when, from some unexplained cause, the reverse took place. The greater malignity and severity of the disease at the commencement, which Schnurrer has pointed out as occurring in all epidemic diseases of kindred nature with the yellow fever (p. 17), was noticed by Barnwell (pp. 376, 388), in the visitation of this city in 1793, and has been referred to in the descriptions we have of our subsequent epidemics, as well as of the disease as it occurs in other parts of this country, in Europe, and in tropical regions.¹

In Georgetown, Demerara, the milder form of the fever, which Dr. Blair denominates simplex, and in which the disease terminates with the period of excitement, prevailed more extensively at the close of the epidemic of 1837 than it had done in the commencement (pp. 41, 45). Dr. Rufz, too, states that from September, 1839, to December, 1840, the epidemic, which had prevailed with great severity, assumed the remittent or intermittent type; which, we may presume means that the yellow fever became milder in its character and was supplanted by ordinary periodic fevers, which acquired the supremacy, and

¹ Caldwell, Fever of 1803, Med. Reposit., vii. 188; Ib., Fever of 1805, p. 51; Fever of Boston, Med. Reposit., ii. 333; Fever of New Orleans in 1839, Rev. Méd., Dec. 1840, p. 324; Fenner, Fever of New Orleans in 1853, Trans. Assoc., vii. 461; Roehoux, Fever of Barcelona, p. 468; Baneroff (Malaga), p. 470; Dufour (Leghorn), Marseilles Journ., iv. 55; Watts (New York), Med. Reg., p. 257; Chapman on Epidemics, Med. and Phys. Journ., xii. 107.

finally reigned alone (pp. 51, 74). Dr. Imray notices the same thing as occurring in Dominica in 1838; when, while every one who was attacked at the outbreak perished, the disease, following the law which is general in regard to epidemics, became less virulent as it extended.¹

While such is the case as regards the severity of the disease, it will be found that the cases are, at first, not only more violent, but also comparatively limited in number. Gradually they are greatly multiplied—in some instances to a vast extent; but as the epidemic approaches its termination, the number is again reduced, until finally the disease disappears completely.

The history of the epidemics of 1793 and 1802, in this city, shows that the greater number of cases occurred in October, after the disease had already continued about two months, the ratio of the progression, in the first year, being—July, 3; August, 14; September, 19; while during the visitations of 1699, 1797, 1798, 1799, and 1803, the cases were greatly multiplied in September, and from that period gradually diminished. The same thing occurred in Charleston, the larger number of the cases occurring in August and September.² Judging from the mortuary reports of the Board of Health of New Orleans, during the months of June, July, August, and September, 1853, the number of yellow fever cases must have increased to August, when it was at its height. It then decreased very sensibly. August gave 4,797; July, 1,380; September, 722; and June, 46.³ In the Charity Hospital, the number of cases admitted was 3,217: August, 1,495; July, 1,157; September, 350; October, 142; June, 50; November, 14; May, 5; December, 4.⁴ The following statements of the average monthly number of cases, in six years—1838–1843—founded on information derived from the registers of the Charity Hospital of New Orleans, show that though the yellow fever sometimes commences in July, and even earlier, and continues to occur in November, the true fever months are August, September, and October. In the order of prevalence, the months stand—September, 408; August, 225; October, 139; November, 59; July, 11; December, 6; June, 0.⁵

In Martinique, according to Moreau de Jonnès (p. 194), it was in the following ratio :—

					June.	July.	Aug.	Sept.
1803	3	4	11	27
1804	5	7	13	16
1805	2	15	18	34

I am not aware that anything like this takes place in truly contagious diseases, their progress not being governed by laws of the kind, and the extent of their diffusion depending in great measure on the number of individuals who come within reach of those already affected. Now as such is the case in contagious disease, and as we find that the yellow fever is placed in this

¹ Edinburgh Med. and Surg. Journal, liii. 94.

² Ramsay, History of South Carolina, ii. 85; Simons, 14.

³ Fenner, Trans. of Assoc., vii. 471.

⁴ Ibid., op. cit., 472.

⁵ Drake, ii. 179.

respect under the empire of laws governing non-contagious epidemic and endemic complaints arising from domestic causes, we may infer that that fever has a like origin and is non-contagious too.

Yellow Fever under the influence of certain Meteorological States, and usually associated with the prevalence and increased severity of Malarial Fevers—not so Contagious Diseases.—Contagious diseases, even when they assume an epidemic character, are not preceded by meteorological states of a peculiar kind, and by which their occurrence may be, to a certain extent, predicted, or without which they are not produced. If they should happen to prevail when such states of atmosphere exist, they may thereby be rendered worse; but by these they are not, and cannot be, announced or produced. Nor do we find them preceded or accompanied by anything peculiar in the nature, extent, and malignancy of the ordinary diseases of the seasons, and still less by the previous prevalence and coexistence in and about the infected place, of complaints of a character kindred to their own. These are attributes of endemic or epidemic diseases depending on domestic causes, terrene and atmospheric, and spreading through means independent of any contagious influence. Such being the case, we find therein reason to doubt the correctness of the doctrine which teaches that the yellow fever of this country and other parts of temperate regions must be derived from abroad, that the fever of tropical climates is often carried from place to place, and that everywhere the disease is propagated by contagion. For it results, from what has been stated in former parts of this work, that the yellow fever has seldom appeared anywhere without having been preceded by a high degree and prolongation of atmospheric heat, and peculiar hygrometric and electric conditions of the atmosphere, to say nothing of the existence of local causes of insalubrity. The same conclusion must naturally be reached when we find that in very many instances the disease has been ushered in by an increased prevalence and malignancy in endemic fevers of kindred kind within the limits of the place infected or in the neighbourhood, that the period of its prevalence is marked by an increase of fevers of a kindred class in the surrounding country, and that when it ceases its place is assumed by these fevers, which continue to show themselves during a longer or shorter space of time, after all signs of the pestilence have disappeared.

True as it may be that the yellow fever has been found to break out suddenly and unexpectedly, without being preceded by an increased prevalence or a more severe form of the febrile complaints common at that season; true, also, as it is, that the disease is not always accompanied or followed by an unusual or even ordinary amount of these; and, on the other hand, that seasons characterized by a wide prevalence of bilious remitting fevers are not always and necessarily visited by the yellow fever—no argument can be adduced from these circumstances in favour of the opinion that this fever, when it does appear, must be the offspring of causes unconnected with the localities; that it has no points of analogy, both as to origin and mode of propagation, with the other diseases mentioned; and that it has been imported and communicated by means of contagion. Admitting the statements made on that subject to be confirmed on every epidemical occurrence of the fever, the circumstance would lend but

little, if any, support to the doctrine of importation and contagion; for the disease, though breaking out suddenly and unannounced by other febrile complaints; though prevailing alone, and ceasing without being succeeded by some other form of disease, may still be admitted to arise from local causes, and to spread without the help of contagion. The one does not necessarily imply the other. Such a mode of appearance and independent existence is found to present itself as regards epidemic remittent and bilious fevers, and other complaints of the local origin and non-contagious character of which there is not a particle of doubt. But, on the subject before us, the statements of the contagionists are far from being applicable to all epidemics.

Much more frequently the fever is preceded, often it is accompanied, and some times it is followed by a greater amount or severer character of sickness than common, not only in the place where it breaks out, but also in the adjacent districts, or even in the open country. The diseases giving rise to such sickness are almost invariably of local or atmospheric origin, and devoid of contagious properties; and their greater spread previous to, contemporaneously with, or immediately after the occurrence of the yellow fever, in a large number of instances, points out a close relationship between them and the latter, and must, by itself, justify the conclusion that this fever is, like the other complaints in question, due to local causes, and, at the same time, free from contagious properties. The inference appears natural, for we cannot presume that a lighter epidemic, of a non-contagious character, can be, except casually, the forerunner and associate of a more serious and contagious one. Indeed, in few instances are diseases of acknowledged contagious properties preceded by an increased prevalence and by an aggravated character of ordinary and local complaints. Let it be remarked that sickness in cities of temperate regions—Philadelphia for example—including that part of them most usually infected, as well as in the surrounding districts and country, may prevail to a considerable extent, but so long as such domestic causes as have been found adequate to the production, or are usually connected with the manifestation of yellow fever, do not exist, the disease will not make its appearance; or, if it does, will do so in the form of straggling and sporadic cases. I may add that the existence of such causes in a portion of the city, together with the meteoration requisite for their development, is not incompatible with an absence of the circumstances to which the prevalence of the sickness alluded to is to be ascribed. But whether or not the connection pointed out may be dispensed with, sure it is that its existence, in most instances, is a sure indication of the prevalence of a general cause of disease, and puts a bar to the idea of the possession of contagious properties by the fever of which the others are precursors or successors, or contemporaneous attendants. The fact will appear the more admissible when we find that the statement made relative to the epidemic of New York in 1795, with a view to show the absence of this connection, is disproved by those of Drs. Bayley and Seaman, two of the chroniclers of that epidemic; that the report of Dr. Miller on the occurrences of 1805 (p. 118) proves that in that year the surrounding country, even to a considerable distance from that city, was

sorely visited by fevers of a malignant character; and, again, that on some of the occasions when epidemics of yellow fever were not preceded by an unusual degree of sickliness, the disease broke out at an earlier period than common, and before the ordinary time of development of summer and autumnal complaints. To this let me add that it would be wrong to regard the sanitary condition of large cities as the criterion of that of the country at large, and to argue that because the yellow fever, when it breaks out in the former, has not been preceded by considerable sickliness there, therefore the statement respecting the usual occurrence of the event is not correct; for bilious remittent and intermittent fevers, to which reference is made, are not usually diseases of cities, from an absence there of the local causes to which they owe their existence.

That the facts are such as I have stated, may easily be shown. In speaking of the premonitory signs of fatal epidemics, Dr. Rush says: "The first external premonitory sign that I shall mention, is an unusual degree of violence in the diseases of the previous year or season. Many proofs of the truth of this remark are to be met with in the works of Sydenham. It has been confirmed in Philadelphia, in nearly all her malignant fevers since the year 1793. It would seem as if great and mortal epidemics, like the planets, had satellites revolving round them, for they are not only preceded, but accompanied and followed by diseases which appear to reflect back upon them some of their malignity" (iv. 113). Another distinguished writer of this country remarks on the same subject: "During the epidemic state of the atmosphere which existed in the first years of its occurrence (the yellow fever), it was remarked that the preceding diseases gradually assumed a more aggravated character, so that its appearance might be predicted."¹ In 1793, the fever was preceded by bilious remittents as well as catarrhal fever or influenza.² In 1805, it was ushered in by intermittents, with cholera infantum, and then diarrhœa.³ In 1819, the summer diseases which preceded the fever exhibited a near approach to their former exalted character, and more frequent occurrence than usual. In the succeeding summer and autumn, "cholera morbus and infantum were very prevalent; bilious fevers, from which our city had been for several years nearly exempted, were common diseases, and dysentery, which had become a rare disease in Philadelphia, was of frequent occurrence, and very difficult to manage. The country was also unhealthy." Dysentery in many places was extremely fatal, and bilious and remitting fevers, along the watercourses, assumed a character of peculiar and most unusual malignancy, and differed very little in their general symptoms from the yellow fever of our cities. In May, a fever of a bilious and remittent character, combined with typhoid symptoms, appeared among the blacks, and continued to prevail among them as an epidemic in September.⁴

Kindred observations have been made time after time in other cities of

¹ Chapman, on Epid. Dis., Med. and Phys. Journal, ix. 398.

² Rush, iii. 75; Barnwell, p. 367.

³ Caldwell, pp. 38, 43, 44.

⁴ S. Jackson, Fever of 1820, pp. 11, 13.

this country; so frequently and generally, indeed, that Dr. Irvine, of Charleston, lays it down as an axiom, that the country must be unhealthy before the town becomes so, and that a season generally sickly is especially necessary to its appearance (pp. 5, 6). Dr. Joseph M. Smith, of New York, than whom few in this country have paid greater attention to this and other subjects connected with epidemics, remarks that the observation of Dr. Irvine is corroborated by the most respectable American physicians (*Epidemics*, p. 168); and a learned philosophical writer does not hesitate to state, as the result of his extensive researches, that the yellow fever, which he denominates bilious plague, never shows itself in the temperate latitudes of America, except when the current epidemics of the country manifest a general constitution of air unfriendly to health (*Webster*, ii. 77). Examples of this will be found in the history of the several epidemics in New York,¹ Baltimore,² Boston,³ Alexandria,⁴ Norfolk,⁵ Natchez,⁶ Charleston,⁷ New Orleans,⁸ St. Augustine,⁹ Mobile,¹⁰ Bay of St. Louis,¹¹ and Savannah.¹² The same results have been obtained in Barcelona,¹³ Cadiz,¹⁴ Arcos,¹⁵ and Carthagen.¹⁶ Even in Gibraltar, where Sir. W. Pym has, in opposition to Dr. Hennen (p. 119) and others, denied the existence of remittent fevers, the yellow fever has usually been preceded by a run of such cases either in the garrison, in the neutral ground, or the adjacent country.¹⁷ "It is a notorious and acknowledged fact," says the late Dr. Chapman, to whom reference has already been made, speaking of the differences between the yellow and bilious fevers, "that while one ravages our cities, the other, at the same time, prevails in the circumjacent country. Examples to this purport are abundantly afforded. We have seen it on every occasion of the occurrence of our summer pestilence. While the wharves, and their immediate vicinage, shall be devastated by the yellow fever, the other districts of the city and the neighbouring country become the prey of bilious fever in its several modifications."¹⁸

¹ Bayley, p. 55; Med. Rep., vii. 177; Miller, Works, p. 118; C. Drake, xxi. 127; Watts, p. 219; Seaman, p. 3.

² Drysdale, i. 23, 370; Potter, p. 24; Letters, pp. 102, 176; Davidge, p. 67.

³ Rand, Repos., ii. 467.

⁴ Dick, Rep., vii. 191.

⁵ Selden and Whitehead, Rep., vi. 248.

⁶ Perlee, iii. 7; Cartwright, ix. 6-7; Merrill, ii. 219-221; ix. 241-3.

⁷ Simons, p. 10; Dickson, iii. 250-1; Repos., xxi. 59, 60; Dickson, Works, p. 337.

⁸ Barton, pp. 6, 8; Dupuy de Chambery, xxi. 17; Harrison, ii. 130; 1839, p. 156; N. O. 1819, p. 36.

⁹ Strobel, p. 140.

¹⁰ Rept. of Com. in 1819; Lewis, in N. O. J.

¹¹ Merrill, N. O. J., viii. 2.

¹² Waring, pp. 8, 11; Daniel, pp. 37-8.

¹³ O'Halloran, pp. 25, 27-29.

¹⁴ Berthe, p. 57; Blin, p. 5; Chervin, Gaz. des Hôpitaux, Sept. 1839.

¹⁵ Obregon, in Chervin, Examen., p. 77.

¹⁶ Proudfoot, xxvii. 240-247.

¹⁷ Rept. of Sickness, &c., of Brit. Army, p. 7; Amiel, Edinb. Journ., xxxv. 269; T. Smith, ib., xxxv. 38; Amiel, in Johnson, pp. 265-6; Frazer, Med.-Chir. Rev., xiii. 339, 340; O'Halloran, p. 167; Chervin, De l'origine locale, p. 10; Gray, Lond. Med. Repos., Nov. 1817, p. 417; Guyon, Ann. Marit., p. 748; Burnett, p. 203.

¹⁸ Philad. Med. and Phys. Journ., ix. 130.

Facts of this kind are familiar to tropical physicians, and have been specially recorded by Desportes (i. 52, 159), Dr. James Clark (p. 62), Dickinson (p. 17), Musgrave (ix. 108, 129, 130), Catel (p. 10), Rufz (pp. 8, 26, 58), Cornuel (in *Rufz*, p. 65), Amic and Fazeuille (*ib.*, p. 66), Imray (*Edinb. J.*, liii. 78-9), Roehoux (p. 546), Dutrouleau (in *Rufz*, p. 70), Vatable (*Ann. Marit.*, pp. 336, 338), Baneroft (*Sequel*, p. 10), Gilbert (p. 39), Chervin (*Rept. on Rufz*, pp. 74-5), as having occurred in Dominica, Antigua, Guadaloupe, and other West India Islands. On the African station, it has been found that diarrhœa is the frequent preeursor of serious epidemieal attacks of fever (*Bryson*, p. 167); and, before the breaking out of the epidemie of 1829 at Sierra Leone, the country on the Boullom shore, and to a great distance, was affected with fever, originating eastward and progressively advancing towards the coast (*Boyle*, pp. 257-8).

In fact, the experience of the profession in temperate regions has shown, that whenever the yellow fever makes its appearance in any locality liable, in a notable extent, to summer and autumnal diseases arising from domestic causes, it is, in a majority of instances, preceded by an increase of these, unless its outbreak takes place at a very early period of the season, and before malarial influences have had time to produce their accustomed effects.

The Yellow Fever, when epidemic, often absorbs or supersedes other diseases.—In circumscribed localities visited by the yellow fever, where the aforesaid diseases prevail to a limited extent—or in more extensive places, when the morbid poison spreads widely and is powerful in its effects, the fever appears to supersede all other febrile complaints, and to reign alone. Dr. Rush states that, in 1793, this took place three weeks after the outbreak of the epidemic. “I have before remarked,” he says, speaking of the epidemic of that year, “that the influenza, the scarlatina, and a mild bilious remittent prevailed in the city before the yellow fever made its appearance. In the course of a few weeks, they all disappeared, or appeared with symptoms of the yellow fever, so that, after the first week of September, it was the solitary epidemic of the city” (iii. 75). The same absorbing influence was found to occur in 1794 (iii. 213, 215), and in our subsequent epidemics, and is dwelt upon by almost every writer who has directed his attention to the subject, in this and other cities of the United States, in Europe, and in tropical climates.¹

Dr. Drake, in the second and posthumous volume of his invaluable work on the diseases of the Mississippi Valley (p. 199), furnishes us with a table, compiled from the registers of the Charity Hospital of New Orleans, which gives the average monthly number of patients with yellow fever and autumnal fever for six years. As it exhibits the absorbing power above alluded

¹ Desportes, i. 39, 40; R. Jackson, *Sketch*, p. 13; Bancroft, p. 6; Humboldt, p. 775; Bally, p. 254; Roehoux, p. 546; Smith, *Edinb. Journ.*, xxxv. 39; Amiel, *ib.*, p. 280; Wilson, *Fever of Gibraltar*, p. 72; Coulter, in Webster, i. 323; Monson, in *ib.*, p. 178; Drysdale, i. 366; Letters on Fever of Baltimore in 1819, p. 102.

to, I shall insert it here. In four of these years, both diseases were epidemic; in two of them, yellow fever was absent, or slightly sporadic:—

MEAN OF 1839, '41, '42, '43.

<i>Both fevers epidemic.</i>	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.
Yellow fever	0	11	223	404	131	58	6
Autumnal fever . . .	62	182	159	108	124	135	91

MEAN OF 1838—1840.

<i>Yellow fever nearly absent.</i>							
Yellow fever	0	0	2	4	6	1	0
Autumnal fever . . .	130	225	207	254	259	136	91

UNITED AVERAGES.

Yellow fever	0	11	225	408	139	59	6
Autumnal fever . . .	192	407	366	362	383	271	182

The same absorbing influence is strikingly illustrated by the occurrences of one of those years, 1839:—

Months.	Yellow fever.	Autumnal fever.
June	00	81
July	11	146
August	481	65
September	360	39
October	129	88
November	62	152
December	00	131 ¹

It is not to be denied that, in many instances, the effect in question is not so universally or so completely produced; for seasons occur in which the yellow fever coexists, in the same localities, with febrile affections of a lower grade of severity; while, in a far greater number of cases, the disease prevails in places more or less circumscribed in extent at the time that the others spread their effects in adjacent or more remote localities. Now, when I say, to use the language of Sydenham, “that just as one nail drives out another, so does epidemic drive out epidemic, I do not mean that the disease which is expelled, and which retreats, disappears altogether; I only mean that it decreases in frequency.”² The fact that in infected spots, the number of intercurrent complaints during an epidemical visitation is comparatively small, or that these do not appear at all, may be due to the circumstance that such complaints do not usually show themselves in a notable extent—as is the case in the eastern part of our city, and particularly along the wharves, where remittent bilious fever is of rare occurrence; and because, in such localities and elsewhere where diseases of the kind prevail, individuals who, under different circumstances, would have suffered from them, yield to the influence of the wider spreading and more violent epidemic. They swell the number of victims of the latter, and by so doing, diminish the evidence of the coexistence of the milder disease. Whatever then may be the overshadowing and

¹ Drake, ii. 200.

² Sydenham Soc. Ed., i. 160; Epid., Constitution of, 1670, p. 1-2.

monopolizing influence of the morbid poison of the yellow fever, the coexistence of the other disease, even in infected localities, is sometimes proved beyond the possibility of doubt, exercising its effects on the protected or acclimatized, at the same time that the other attacks strangers or the unprotected. The further we recede from such localities, the more the prevalence of such affections to a large extent, or in greater severity than usual, becomes manifest—the two forms prevailing separately in different individuals, or blending together in the same individual. Desportes, in his account of the diseases he observed in St. Domingo during the fourteen years of his residence there, mentions several instances in point in 1734, 1735, 1739, 1741, 1742, 1743, 1744, 1746.¹ Subsequent writers in the West Indies, Europe, and this country, have adduced confirmatory facts.²

These facts of the succession and coexistence of remittents and other febrile complaints arising from domestic causes with the yellow fever, have, from its frequent occurrence, been adduced as an argument in favour of the identical nature of those diseases;³ but though, for reasons elsewhere stated, there is good ground for dissenting from these conclusions, we are justified in the opinion, that the circumstances mentioned indicate at least a great analogy between the characters of the causes giving rise to the two forms of disease, or rather a family connection between these; that as the former is due to local and domestic morbid agencies, the latter must be admitted to arise from the influence of some other modification of morbid poison of kindred nature and origin, and that as the one form of disease is destitute of contagious properties, the other cannot be supposed to be endowed with them.

Yellow Fever, in this respect, differs from Contagious, and approximates to Non-contagious Febrile Diseases.—In opposition to Schnurrer, we may say with Hopfengartner, Smith, and others, that contagious diseases, requiring as they do for their introduction into any place and their propagation, the communication from individual to individual of a specific virus, and being circumscribed in the sphere of their action, cannot and do not so modify and poison the atmosphere, even when they prevail epidemically, as to impart, through its medium, their peculiar character to the ordinary diseases of the season, and make them wear their livery. Such will be found to be the case with the smallpox, and other diseases of kindred nature, which, though prevailing at times very extensively, and in a very malignant form, leave other complaints to follow their course unmodified in their character and symptoms. If they reign paramount, driving away or superseding, as it were, other diseases, they do so, in all probability, by affecting many who, had they not prevailed, would have been seized by

¹ Vol. i. 52–54, 66–7, 93–5, 110, 111, 114, 122–3, 128–9, 136, 159, 172.

² Musgrave, ix. 129–30; Proudfoot, Edinb. Journ., xxvii. 243; T. Smith, ib., xxxv. 38; Amiel, ib., xxxv. 269; Ib., in Johnson, p. 265; Dickinson, p. 17; Imray, Edinb. Journ., liii. 78; Savarésy, p. 293; Rochoux, p. 546; Chervin, Rept. on Rufz, pp. 74, 94; Pinkard, ii. 408, 490; Bancroft, p. 408 (note); Ib., Sequel, p. 13 (note); Spinosa, Mém. sur le F. J., &c., pp. 8, 28; Med. Reposit., iv. 207; Rufz, p. 40; Cornuel, in Rufz, p. 71.

³ Bancroft, Seq., p. 10; Merrill, ix. 243; Chervin, Rep. on Rufz, pp. 71, 94.

other diseases, and not by destroying, modifying, or mitigating the causes of these. They are more or less inflammatory, more or less malignant—as are also other complaints which prevail at the same time; but these complaints do not derive their existing character from the reigning contagious malady, but from the same atmospheric constitution which imparts it to the latter. All instances of a different kind in which a mixture of phenomena appears, are the results of real and evident complications, and are, besides, too seldom encountered to be regarded as evincing the existence of the modifying influence in question. They are the effects of modifications imparted by atmospheric complaints to the prevailing contagious disease itself, which, though unaltered in its nature, and presenting its main phenomena, varies somewhat in its outward appearance. Contagious diseases exercise, Dr. Smith remarks, a primary, not secondary influence, over other prevailing complaints, and if, during an epidemic of smallpox, or any other eruptive contagious disease, affections of the skin occur, they are due to the existing meteoration, and not to the contagion.

I am not unmindful of the fact that the oriental plague has often been cited in illustration of this modifying influence being possessed by a contagious disease, an effect manifested during the epidemics of London, Holland, and Germany, described by Sydenham, Diemerbroeck, Sennertus, and Hildanus. But, as already stated several times, the more recent and correct observations of Assalini,¹ Brayer,² Clot-Bey,³ C. Maclean⁴ and Aubert Roche,⁵ have so satisfactorily proved the non-contagiousness of that disease, or have limited that contagiousness within such narrow bounds—thus corroborating the opinion long ago expressed by Chicoyneau,⁶ Boyer⁷ and other writers—as to justify us in rejecting the exception that would be made in its favour, especially when we bear in mind the malarial or local origin of the complaint. Nor can we suppose that the peculiar condition of atmosphere which at some periods has prevailed over large tracts of country, imparting a typhoid character to most diseases, and under the influence of which typhus fever spreads among certain classes of persons, under particular circumstances of exposure, can be adduced in contradiction to the above statement. In this case, the modifying power is evidently the result of that atmospheric peculiarity, and not of the influence of the prevailing disease; inasmuch as it manifests its effects far beyond the reach of cases of the latter, the sphere of the contagion of which is acknowledged by all to be very limited.

Very different is the case with febrile diseases originating from causes of a local and domestic character, and becoming general through the extensive diffusion of these, aided by peculiar constitutions of atmosphere; as well as

¹ Obs. on the diseases called the Plague, pp. 16, 191.

² Neuf années à Constantinople, ii. 330.

³ De la Peste, p. 159.

⁴ On Epid. and Pestil. Diseases, i. 147, &c.

⁵ De la Peste ou Typhus d'Orient, p. 67, &c.

⁶ Traité des causes, des accidens et de la cure de la peste, 4to. p. 110, etc.; ib., Lettre à M. de la Monière, 12mo. p. 4. ets.

⁷ Réfutation des anciennes opinions touchant la peste, 12mo. p. 8.

with epidemic diseases due to peculiar meteorations. Such diseases have the power of impressing their character and physiognomy on other complaints, those of a contagious character included, and often, as just seen, of supplanting them more or less completely during the period of their prevalence; but are in no instance endowed with the well-ascertained power of being propagated by contagion, as some have supposed, but have not proved. This power, of which illustrations in great numbers will be found recorded in medical and historical writings from the days of Hippocrates and Thucydides to our own, has given rise to the well-known axiom laid down by Sydenham, that no two diseases of unequal force can coexist in the same place, the epidemic being a sort of monarch, exacting a species of homage from all inferior disorders, and which imposes upon them the degradation of a livery.¹ Being not powerful enough to expel entirely a disease, it forces upon it some of its symptoms, and gives to it, in a greater or less degree, its own peculiar character and aspect.

It has been seen, in a former chapter, that the yellow fever exhibits, in a striking manner, the modifying empire in question, and usually exercises it over other diseases within the circle of its range. For during a wide-spreading and violent epidemic, it either assumes, to a certain extent, the place of all others in the infected district, or imparts to them a portion of its physiognomy and character. The reality of the fact relative to ordinary fevers, and the analogy it thereby establishes between the yellow fever and diseases arising and spreading through the instrumentality of causes independent of personal contagion, and incapable of being carried from place to place, can scarcely need to occupy us long. Examples of it abound; a few will suffice in this place. It is remarked, by Dr. Rush, that the common bilious fever which prevailed in Philadelphia in 1780 chased away every other febrile disease (iii. 77). I have already, in the chapter on complications, alluded to instances of the kind.

In his account of the measles of 1801, Dr. Rush states that the disease wore the livery of the autumnal fever in the following particulars: "It was strongly marked by remissions and intermissions; the exacerbations came chiefly at night; there was in many cases a constant nausea and discharge of bile by puking" (iv. 73).

Dr. Balfour tells us that the intestinal remitting fever of Bengal, of which he has left us a graphic account, often appeared with symptoms of dysentery, rheumatism, and pleurisy.² The union of two poisons in this way was noticed by Pringle and others in Europe, in the case of typhus and marsh fevers. The fever of Banker Street, New York, in 1820, was evidently a complication of bilious remittent and typhus—the *idio koino miasmal* fever of Dr. J. M. Smith.³ In the epidemic of Naples, in 1764, it was rare, as we learn from Sarcone, for other diseases, originating from causes different from those of the epidemic which scourged the city, not to pass finally into the latter.⁴

Dr. Blake, in his account of the climate and diseases of California, remarks

¹ Chapman, *Law of Epidemics*, Philada. Med. and Phys. Journal, xii. 105.

² A Collection of Treatises on the Effects of Sol-lunar Influence in Fevers, pp. 124–5.

³ On Epidemics, p. 57.

⁴ *Maladies de Naples*, ii. 59.

that although the influence of malaria does not show itself in the Sacramento Valley, "by producing any of the more marked forms of disease by which its presence is usually manifested, yet we have constant indications of its existence, by the character it impresses on almost every form of disease occurring in this locality."¹ Experience, indeed, everywhere shows that remittent and intermittent fevers are sometimes engrafted upon or blended with smallpox, scarlatina, measles, dysentery, erysipelas, syphilis, seabies, whooping-cough, oriental plague, cholera, gonorrhœa, &c.

Dr. Nepple, in his description of an epidemic of periodie fever which prevailed in the canton of Dombes, in France, in 1823, states that almost every febrile disease was then attended with periodical paroxysms, which subsequently degenerated into true intermittent attacks. Bronchitis, which was very common, and of a highly acute character during the winter, became complicated with nervous irritations, spasmodic dyspnœa of a more or less intermittent type, and with remittent paroxysms. Intermittent neuralgias were more than usually common. In a word, every disease appeared to have assumed a nervous and periodie character.²

The same phenomenon was observed at Auch some years later, where, independently of intermittent pernicious fevers which prevailed extensively, periodicity entered as an element in, or as a complication of, the greater number of the intercurrent diseases, even in those which presented the inflammatory character, and in which physicians were more than once obliged to employ at the same time the lancet and the sulphate of quinia.³

The sweating fever prevailed epidemically in the department of Dordogne, in France, during the greater part of the year 1841, affecting 10,803 individuals in a population of 83,342, and causing a mortality of 797, or 1 in about 5.5 of the sick. For two years previous, the department (especially that part of it embracing the cantons of Nantron, Riberae, and Périgueux) was overrun with eruptive fevers—measles, scarlatina, smallpox, and varioloid. Generally, one or two of these occupied the ground three or four weeks, and then made way for the others. In several instances, measles and scarlatina, or smallpox and varioloid, marched together; while in some, all four existed at the same time. Under these circumstances, the sweating fever made its appearance. In the course of the year—from the 7th of May to November—the disease broke out five times in as many different places, and disappeared; so that the epidemic, considered in its *ensemble*, appeared, as it were, in five instalments. On four of these occasions it assumed suddenly its legitimate garb, and immediately drove away all other diseases.

On the fifth, however, things took a different turn. All at once, after a thunderstorm, on the 7th of May, and the days following, a disease heretofore unknown broke out. It was very different in its nature and results from the measles, which then prevailed; but yet difficult at first to recognize, owing to

¹ N. O. Journ., ix. 510, 511.

² Tr. des Fièvres Interm., p. 137.

³ Compardon, Aperçu sur les Maladies qui ont régné épidémiquement à Auch, Bulletin de l'Acad., viii. 634.

its being under the dependence of the complaint it so singularly and suddenly came to replace. The disease no longer presented exactly the same train of phenomena, the same kind of pulse, or eruption. Nevertheless, there was in that new physiognomy of symptoms a family air which greatly puzzled the physicians. The measles were modified. To the precursory febrile symptoms were added others which are strangers to the disease; while others which belong to it disappeared unexpectedly. Some days before, the patients had been seized with a chill, cough, coryza, and all the opening symptoms of measles. In some cases, one of these symptoms now gave way; in others, a different one disappeared; in all, the attack henceforward came on without the chill. Some, however, had a marked and well-defined rubeolic eruption, while in others the skin became covered with a miliary eruption.¹

It is not improper to remark here that, though admitting the modifying power exercised by yellow fever over concomitant diseases, and while rejecting as erroneous the conclusion of M. Rochoux, who denies its existence altogether as regards the fever of the tropics, and deduces from this an argument in favour of a difference between that fever and the fever of temperate climates, I am disposed to think that its influence has often been somewhat magnified, more particularly in the West Indies. From all the information we can collect on the subject, the conclusion is natural that the influence in the latter is manifested fully only in times of violent epidemics, and that in our latitudes it is seldom so complete as to cause the total disappearance of other diseases, and especially of those of kindred character. Nothing is more common, in hot climates, than the coexistence of two grades of fever essentially different in the degree of their violence and in the character of the subjects attacked. While a mild intermittent prevails among the natives and old inhabitants of a district, who greatly outnumber the unprotected, the yellow fever prevails among these. It happens, also, that remittents and intermittents prevail among all classes—strangers and natives—until the cause of the yellow fever becomes fully developed, when the latter attacks strangers, while the others continue their course among the acclimatized. Again, at the close of an epidemic the autumnal fevers continue to show themselves, and, the cause of the yellow fever having ceased more or less completely, strangers are no longer affected by it; but are now, like the acclimatized, open to attacks from the former. Of this, Desportes gives us several examples. But, I repeat, the occurrence is to be observed at those seasons when the disease prevails to a limited extent; for in times of unusually severe epidemics, when the fever is exceedingly malignant, and the cause so pervading and powerful as to attack large multitudes, and not to spare, as usual, the acclimatized—an instance of which it did not fall to the lot of Rochoux to witness—the reigning disease absorbs and supersedes all other febrile complaints, or modifies them to a greater or less extent.

In regard to temperate climates, and of our own country particularly, it is not certain that the superseding and modifying power in question is always as

¹ Parrot, Histoire de la Suette Miliare, Mém. de l'Acad. de Méd., x. 395-6.

great as is stated by some writers, though no one can deny that, under ordinary circumstances, it is more striking than in tropical regions. In the epidemics of those regions where the protective influence of acclimatization does not prevail, and where necessarily the protected, unlike what takes place in the West Indies, are comparatively few, the number of individuals stricken down with the fever in the infected district, during a wide-spreading epidemic, being ordinarily very large—ininitely more so, perhaps, than that of those labouring under other complaints—and the cases following on each other in rapid succession, attention is principally fixed on the reigning and deadly disease; and it must occur that, under such circumstances, a number are attacked by the latter who otherwise would have suffered in some other way. To this, too, it must be added that the advocates of the idea of the complete monopoly of the reigning epidemic are almost exclusively to be found among those who, believing in the unity of febrile diseases, regard the yellow as only a higher grade of the remitting and bilious fever of the season, and view the latter, when it coexists with the former, not as an evidence of the prevalence of two forms of fever, but of various degrees of one and the same complaint, all of which are due to the agency of the same cause.

Nevertheless, after making all possible allowances for exaggerations on this subject, there cannot be a shadow of doubt of the reality of the absorbing and modifying influence adverted to, and its manifestation on numerous occasions; and we may derive from the occurrence an additional reason for believing in the local origin and non-contagious character of the yellow fever, seeing that it shares that power with other fevers which, if not identical with it in the nature of its causes, phenomena, and pathological conditions, belong to the same family of diseases, and which are all devoid of contagious qualities.

CHAPTER XVI.

PROOFS OF NON-CONTAGION—CONTINUED.

The Disease one of Hot Climates and Hot Weather only, and is arrested by Cold.—Some of the contagious diseases of the eruptive class prevail, at times, to a very considerable extent in hot climates. The spread of smallpox in Africa, and among the black and the white population of the West India Islands, furnishes evidence of this fact. But though this may hold good occasionally in temperate regions, where, under certain circumstances, such diseases prevail during the summer months, it is more generally found that they reign there principally in the cooler seasons of the year, and even during the cold of winter, and that they disappear on the accession of hot weather; while some of them, as scarlatina, are unknown, and cannot be propagated in the Antilles, the natives of which are said to require a sojourn of eighteen months before becoming susceptible to the impression of the poison. Even as regards

smallpox itself, it may be doubted whether it spreads as widely, and proves as malignant in hot regions as it does in cold countries. Desportes, who notes the occasional introduction of the disease in St. Domingo, states that such events were not calculated to create alarm, inasmuch as he had remarked that the disease was not dangerous, that it was not readily communicated from individual to individual, and that if some negroes died of it, the circumstance was to be attributed more to want of care than to the malignancy of the disease and the violence of the symptoms (i. 89).

If such be the case in cold regions—at least with regard to some of the eruptive febrile diseases of a positive, and to some of a probable contagious character—if they are diseases of cold weather—or at most, if they appear in both cold and hot seasons—these facts are still more strikingly true as regards febrile diseases of a different character. Typhus fever, of the contagious nature of which there is no reason to doubt, has been well said to be the disease of cold climates. That cases of it appear occasionally in the tropics, and during the hot weather of northern latitudes, may be true; certain it is, also, that Desportes, Dazille, and other of the older writers on the diseases of the West Indies, mention the occurrence there of putrid and malignant fevers, which have often been regarded as identical with the typhus fever of Europe and this country. But we have unquestionable testimony to the effect that such cases are exceedingly rare within the tropics; and no American or European physician need be told that in temperate regions the disease prevails often in winter, and disappears with the accession of hot, or even warm weather. Dr. John Hunter, whose extensive experience was obtained in both hemispheres, states that he never saw the jail or hospital fever earlier than the month of November, and thinks it seldom appears so soon; that it becomes frequent about Christmas, and increases during the months of January and February. “If March and April are warm, it grows less frequent; but if they are cold, it continues nearly as common as in the preceding months. When the weather begins to grow warm, it gradually disappears.”¹ “I would observe,” he adds, “that for upwards of two years that I remained in Jamaica, I never saw one instance of the hospital fever, though the military hospitals were often as much crowded as they are in Europe.” “The heat proves a prevention of the disease, as much as cold forwards its production.”² “In the crowded transports at the beginning of the last war,” says Dr. Ferguson,³ “typhus fevers were frequent amongst newly embarked troops, and they, of course, for the doctrines of the day, carried the contagion to the West Indies, which afterwards became yellow fever under that exalted temperature.” “But, alas for the faith! Typhus fever will not stand carrying to warmer latitudes. If the ship be even ordinarily clean, it will vanish long before you can have entered the tropics; and you may as well attempt to transplant a willow tree, or a hazel, into a West India colony, as a fever of

¹ Transactions of Lond. College of Phys., iii. 350.

² Ibid., p. 355.

³ Recol., p. 143; Ib., Med.-Chir. Rev., Jan. 1840, p. 317; Ib., Med. Gaz., xxii. 369.

that class. I do not utterly deny, although I never saw it, the possibility of landing typhus fever there out of a foul crowded ship—but it will not stay; it will disappear infallibly as soon as ventilation, even in the most ordinary degree, is restored. It is not to be denied that crowded convict and emigrant ships have carried typhus fever through the tropics to the Cape of Good Hope and Australia; but no importation, however foul, could establish it in those countries, or cause it to spread—nor is it, I believe, known in them even now.”

Many facts in corroboration of the principle that contagious fever is in a great measure unknown in tropical climates, and frequently prevails in the cold season of temperate regions—in a word, that heat is in some measure inimical to the matter of contagion, which generally luxuriates during the continuance of a cold temperature, are recorded in the works of Bancroft¹ Trotter,² Blane,³ Lind,⁴ R. Jackson,⁵ Howard,⁶ Lempriere,⁷ Tullock,⁸ Chervin,⁹ &c. When we refer to the British, Irish, French, and other histories of continued fever, typhus, and typhoid, we find that on the point under discussion they obey the same law. Though at different times and places they have commenced, reigned, and ceased in every season of the year; though they have been known, in Europe and this country, occasionally to prevail in the warmest portions of the year—they more usually prevail during the cold weather of winter or the cool of spring; as a general rule we do not find their epidemic course impeded by the accession of low temperature.¹⁰ So far as the great valley of North America is concerned, Dr. Drake—whose ability and accuracy will not be questioned—has found: 1. That typhus and typhoid fevers make their appearance in every season, but begin more than twice as frequently in the fall and winter as in the spring and summer. 2. That they prevail much oftener in autumn and winter than in the other seasons; and most frequently of all in winter. 3. That they sometimes cease with the accession of summer, reviving in winter, again to cease when summer returns;

¹ Essay on Yellow Fever, p. 508.

² Med. Naut., i. 184, 197.

³ Diseases of Seamen, p. 233.

⁴ On Seamen, p. 319; *Ib.*, On Fev. and Infection, pp. 317, 318.

⁵ A Sketch, i. 258; *Ib.*, Outline, &c., of Fever, pp. 3, 35.

⁶ On Prisons, p. 467.

⁷ Dis. of Army in Jamaica, i. 25, 32–3.

⁸ Mortality, &c., of British Army, p. 7.

⁹ Examen Critique des Principes de l'administration, &c., p. 73.

¹⁰ Percival, Tr. of King and Queen's Col. of Phys. in Ireland, i. 255–264; Grattan, *Ib.*, i. 465–6; Grant, on the Pest. Fever of Sydenham, pp. 9, 33, 40; F. Barker, *Ib.*, ii. 522, 528; O'Brien, *Ib.*, iii. 494–5; Willan, Diseases of London, in Miscel. Works, pp. 163–176, 217, &c.; Bateman, Epid. of Lond., p. 15; Roux, Traité des Fièvres Adynamiques, p. 436; Montault, Mém. de l'Acad. de Méd., vii. 193, 369; Røederer and Wagler, p. 30, &c.; R. Jackson, on Contagious Fevers, pp. 58, 59, 60; Stoker, on Continued Fever, &c., pp. 37, 38; Forgét, Traité de l'Entérite Folliculaire, p. 409; Dublin Journ. of Med. Sci., x. 34; Rept. of Registrar General for 1843 and 1844; Cheyne, Dublin Hospital Reports, ii. 44, 52; Barker and Cheyne, An Account of the Rise, Progress, and Decline of the Fever lately epidemical in Ireland, i. 15, 49–59, 148, 193; ii. 172, 178; Genest, Clinique Médicale de Chomel, p. 349; Lombard and Faueonnet on Typhoid Fever; Valleix, Guide du Méd. Praticien, x. 766.

and that the same remark is conversely true of winter. "On the whole," Dr. Drake remarks, "they prevail more in cold than in warm weather, and this coincides with my own observations at Cincinnati, where the number of sporadic cases has generally been greater between the autumnal and vernal equinoxes than in the other half of the year" (ii. 444).

In addition, it may be remarked that if the contagiousness of the oriental plague be admitted, that disease may be adduced as a further illustration of the facts that cold weather promotes the development, and a hot temperature dissipates the matter of contagions, as it prevails during winter in Egypt, and disappears on the accession of summer.¹ But on this I will not insist, seeing that the contagiousness of that disease cannot, in the present state of our knowledge, be viewed as a fact placed beyond dispute; that the season of its prevalence differs in other countries; that the period of its origin and cessation in Egypt appears to depend on causes independent of the mere elevated temperature existing at the time, or on which the latter exercises an indirect agency; and that at Constantinople and other places the disease is under the control of other influences.

Again: Contagious diseases are not dependent for their origin and propagation on the hygrometrical conditions of the atmosphere, either prior to their appearance or during their continuance, and whether they occur sporadically or assume an epidemic character. Wet or dry weather does not appear to be essentially necessary to their development, and does not arrest their progress, while it will be found that, so far as regards typhus fever, if dampness exercises a controlling influence, that dampness is almost invariably associated with cold, not heat.

When, with these facts before us, we examine the period of origin and cessation as well as the causes and mode of propagation of diseases with which the yellow fever is more or less closely allied, we find that they are diseases of hot climates or hot seasons; that they commence in hot weather, continue, and are more malignant and wide spreading after the middle of summer, and during the autumn, and that they disappear in temperate climates on the accession of very cold weather or frost. Besides this, we find they are everywhere under the influence of certain hygrometrical conditions of atmosphere, are due to the action of domestic causes, and are universally admitted to be void of contagious properties; nay, more, that no facts are recorded of a nature calculated to afford satisfactory reasons for suspecting them to be contingently so—a fact admitted very generally even by contagionists in Europe, the West Indies, and this country.

On the connection of these fevers with high atmospheric heat, I have already touched. To what was then said, it may be added that remittent and intermittent fevers seldom appear before the middle or close of summer, and cease on the accession of winter. Their existence in an epidemic form in temperate regions, is never known to occur in the spring of the year; cases that occur in that season, or in winter, being accounted for without having re-

¹ Rapport sur la Peste, p. 330; Clot-Bey, p. 225.

course to the supposition of the development then of the efficient cause. They never show themselves in winter, and if they appear early in summer—which is seldom the case—the occurrence is only noticed under peculiar circumstances of atmosphere existing during the preceding months, and which invest these with the characteristic conditions appertaining to autumn. They are emphatically diseases of hot weather, requiring for their production a continuance for some time previous of high atmospheric heat. They appear, generally, some weeks after the hottest month; the period being retarded as we proceed north. For the same reasons they may readily be understood to be diseases of hot latitudes, prevailing, as they do, violently and almost perpetually within the tropics, and ceasing long before we reach the polar circle.

In the Mediterranean stations, the admissions into the hospital, and the deaths, average nearly twice as high between July and October as during any similar number of months in the year.¹ In Spain and Portugal, the sickly season, in malarial districts, is from July to September.² The fever of Rome, in general, is held to begin with the great heats about the end of June. The Roman people have fixed on St. Peter's day as the exact period of its outbreak, and it reigns from then till it is put a stop to by the equinoctial or autumnal rains of September. The readers of Horace will recollect that, in his time, July was notorious for its insalubrity. “*Addueit febres et testamenta resignat.*”

In this country and in Canada it commences in July or August, and ends in the south in November or December, and in the north about October. In England, much the same results are obtained.³ In Walcheren, the sickly months are June, July, August, September, when the heat is considerable.⁴

In hot latitudes, where the heat is almost constant, the fever, if not constant also, commences earlier than in our latitudes, and lasts later when not put a stop to by unusual agencies. On the African coast it exists to some extent at all seasons; but is formidable from April to November, and especially rife from July to the last-mentioned month. In Senegal, thunder-showers commence towards the close of April, or beginning of May, and continue to the middle of July, when the rains set in. This season (*hivernage*) lasts to the end of October or beginning of November. It is at this period (the hottest of the year) that remittent fevers usually make their appearance.⁵ In the West Indies, also, the period of the greatest liability is between July and December, when the hottest weather, combined with considerable moisture, prevails. In Bengal, fever commences in August, and continues till November.⁶ In Ceylon, the months of June, July, and August are, in general, the period of the year when the greatest sickness prevails.⁷ In a word, the epoch of appearance and disappearance may vary in different localities according to the situation of these, and their position relative to the equator, and the consequent modification in the period of the seasons; but everywhere

¹ Williams, on Morbid Poisons, ii. 460.

² Macgrigor, Med.-Chir. Tr., vi. 387.

³ Williams, op. cit.; Watson, 445.

⁴ Blanc, Dissertation, i. 225.

⁵ Levacher, p. 48.

⁶ Clark, on Long Voyages, i. 116.

⁷ Marshall, Topography and Diseases of Ceylon, p. 39.

endemic or autumnal fevers break out, or are most rife, during or shortly after the hottest weather.

While such is the connection in question between the production and diffusion of periodic fever of a non-contagious character and high temperature, it will be found that whenever in a miasmatic district, the thermometer descends to the freezing point, and there remains awhile, the progress of malarial fever is arrested. In this vicinity, among others, every one, from the learned physician to the gossiping granny, knows full well that the accession of severe cold weather, and particularly of black frost, is sure to be marked by a cessation of the fevers in question, and that, in the few cases that present themselves after that event, the attack is ascribable to that power of dormancy, by virtue of which the cause of some diseases remains, for a greater or less length of time, latent in the system after exposure, and which, in some forms of fever, may, as we have seen, be prolonged for weeks and even months. Had I space and leisure to enlarge much on this subject, and, indeed, were it necessary, I could demonstrate, by means of numerous quotations, that such is the case also in other parts of this country, and in foreign lands—in every place, indeed, where the cold of winter is well marked or the frost severe. A few references will be sufficient. Thus, in regard to periodical fevers, the fact of their cessation, through the effect of cold or frost, is recorded in the writings of the physicians of our northern and middle States, and of Canada: Rush,¹ Currie,² Potter,³ Caldwell,⁴ E. H. Smith,⁵ J. M. Smith,⁶ Wood,⁷ Coventry,⁸ Vaughan,⁹ Lee,¹⁰ Usher Parsons,¹¹ Agnew,¹² Lucas,¹³ Stratton.¹⁴ In the South, Southwest, and West, we find it mentioned—often insisted upon—by Taylor,¹⁵ Ramsay,¹⁶ Hansford,¹⁷ Thompson,¹⁸ Waring,¹⁹ Arnel,²⁰ Prior,²¹ Smelt,²² Dunbar,²³ Staley,²⁴ Dickson,²⁵ Hildreth,²⁶ Drake,²⁷ Bissell and Metcalf,²⁸ Simons,²⁹ Grant,³⁰ Cooke.³¹ Cross we the

¹ Non-Cont. of Yellow Fever, Med. Repos., vi. 162.

² On Bilious Fever, pp. 13, 21; Med. and Philos. Register, 181, 195.

³ On Contagion, p. 16.

⁴ On Miasma, p. 16; Med. and Phys. Mem. (1800), p. 202.

⁵ Webster's Collection, p. 107.

⁶ On Epidemics, p. 71.

⁷ Pract. of Med., i. 142, 267.

⁸ On Lake Fever, N. Y. Med. and Phys. Journ., iii. 15; Tr. of Med. Soc. of the State of N. Y., for 1825, p. 42.

⁹ Med. Repos., iv. 130.

¹⁰ Ibid., iii. 252.

¹¹ On Malaria (Essays), p. 200.

¹² Med. Recorder, vi. 138.

¹³ Ibid., v. 420.

¹⁴ Edin. Med. and Surg. J., lxiv. p. 10.

¹⁵ Webster's Collection, p. 148.

¹⁶ History of South Carolina, ii. 55–56.

¹⁷ Webster's Collection, p. 148.

¹⁸ Chapman's Journal, x. 106.

¹⁹ N. A. Med. and Surg. J., i. 5; ix. 375.

²⁰ Med. and Philos. Register, ii. 10.

²¹ In Potter, op. cit., p. 16.

²² Med. Repos., ix. 127.

²³ Ibid., viii. 258.

²⁴ Med. Recorder, v. 460.

²⁵ Am. Journ., ii. 64.

²⁶ Med. Repos., xi. 345; Chapman's Journ., ix. 109.

²⁷ Topog. and Dis. of the Valley of the Miss., pp. 608, 713.

²⁸ Drake, p. 395; Merrill, Memphis Med. Record, i. 90.

²⁹ Charleston Journ., iv. 543.

³⁰ Am. Journ., July, 1853, p. 112.

³¹ Med. Record, vii. p. 453.

Atlantic, we find the same thing taught by Sydenham,¹ Lancisi,² Cleghorn,³ Bartholin,⁴ Bancroft,⁵ Williams,⁶ Copland,⁷ Macculloch,⁸ Monfalcon,⁹ Rigaud de Lisle,¹⁰ R. Hamilton,¹¹ Sir J. Pringle,¹² F. Home,¹³ G. Brown,¹⁴ Tournon,¹⁵ Bailly,¹⁶ Sir Ch. Morgan,¹⁷ Sir James Clark,¹⁸ Booth,¹⁹ Carriere,²⁰ De Renzi,²¹ Jacquot.²² In fact, it is mentioned as a well-known and indisputable phenomenon, by all who have written on the periodic fevers of England, Holland, Flanders, France, Italy, Spain, etc. Nay, in the West Indies themselves, the healthy season is the cool season, and the continuance of periodic fevers during the winter months is in direct proportion to the continuance of heat. In warm winters fever continues; in cool ones it disappears in great measure, or completely.

According to the conclusions of Dr. Drake, "the fever will occur in winter, at all places where that season has a mean temperature of 60° or upwards, as at Vera Cruz, Tampico, Havana, Key West, Tampa Bay, and Fort King, and it is well known that cases do occur at those places in that season; but in other places, where the winter heat barely rises over 60°, they are few in number. At New Orleans, and generally under the 30th parallel, where the mean winter heat is as low as 50°, the fever is suspended."

Dr. Drake further says: "But the seasons are made up of months, and we are here brought to consider its connection with their respective temperatures. Up to Tampa Bay, every winter month rises above 60°; but at New Orleans, or the 30th parallel, only the nine months from March to November have that temperature; and as we advance to the north, the number of months having it constantly decreases. Thus, at St. Louis it is attained by five months only, from May to September, inclusive; at Fort Snelling, by four; at Fort Brady, by three; at Montreal, by four; at Quebec, by three. In advancing further north, June and September fall below it; and finally, in the distant north, July and August, or the entire year. Long before this reduction is reached by those two months, however, the fever ceases, and therefore it results that a continuance for more than two months of a heat equal to 60° is necessary to the development of the fever." "It appears, from all that has been said, that within the tropics, autumnal fevers occur throughout the year, and that as we move northerly the duration of its prevalence shortens, by its beginning later

¹ Works, ii. 191.

³ Dis. of Minorea, p. 133.

⁵ Tr. on Yellow Fever, pp. 292, 407; Sequel to do., p. 91.

⁶ On Morbid Poisons, ii. 460.

⁸ On Malaria, p. 155.

¹⁰ In Johnson on Trop. Cl., p. 313.

¹² Dis. of the Army, pp. 14, 116, 172.

¹⁴ Cyclop. of Pract. Med., ii. 235.

¹⁶ Traité des F. Int. Pernicieuses, p. 134.

¹⁸ Med. Notes on Italy, pp. 80-83.

¹⁹ Life of Dr. Armstrong, i. 258; ii. 295, 597.

²⁰ Du Climat de l'Italie, p. 371.

²¹ Oss. Sulla Topog. Med. del. Regno di Napoli, p. 68; Ib., Miasmi Paludosi, p. 21.

²² Des F. a Quinquina, p. 38.

² De Noxiis Paludum Effluviis, p. 46.

⁴ Hist. Anatomiearum, p. 72.

⁷ Vol. i. p. 759; ii. 1100.

⁹ Traité des Marais, p. 345.

¹¹ On Marsh Remit. Fever, pp. 28, 32.

¹³ Med. Facts and Observ., pp. 46, 76.

¹⁵ Etudes sur Rome, i. 203.

¹⁷ Lady Morgan's Italy, i. 439; ii. 106.

in spring and terminating earlier in autumn. Mareh and November first escape; then April and May on the one hand, and October on the other; lastly, June and September."¹

Now if, having settled those points respecting malarial non-contagious fevers which, as seen, though not identical with yellow fever, belong evidently to the same family of diseases, we extend our investigation, and inquire how matters stand relative to the latter fever, we shall find that, like the other disease, it is one of hot latitudes, and ceases under circumstances similar to those by which it is influenced, and diametrically opposed to those we have noticed in connection with contagious febrile complaints. As we have already seen, a high atmospheric heat is essential to its development and epidemic diffusion. It requires to be preceded, during a certain length of time, by particular thermometrical and hygrometrical conditions of the atmosphere, and closes its epidemic career under the influences of the same meteoric agencies as ordinary autumnal fevers. That in some sickly seasons, in this country as well as in Europe, cases are sometimes met with several days or (as in the south) some weeks after cold weather has set in, is true; but such results are rare, in the north particularly, and they are most generally the effect of the breaking out of the disease in persons who had imbibed the seeds of it prior to the fall of the thermometer. But here, as elsewhere—at the north and the south—the disease invariably ceases as an epidemic after a frost sufficiently severe to kill the leaves of trees and annual plants.

Dr. Rush,² speaking of our yellow fever, says: "It is completely destroyed by frost." Dr. Currie (p. 2) remarks, in reference to the epidemic of 1793: "As the weather became cold and frosty, its declension was so rapid that it appeared as if extinguished by a miracle." In 1797, the Academy of Medicine³ derived an argument in favour of the identity of yellow and bilious fevers from the fact that they were both uniformly checked and destroyed by the same causes—heavy rains and frost. And if the reader will examine the records of all the other epidemics by which this city has been visited—those of 1699, 1741, 1747, 1794, 1797, 1798, 1799, 1805, and 1820—he will find that they were all put a stop to, more or less suddenly, by the occurrence of similar changes of temperature.

Drs. Harrison,⁴ Thomas,⁵ Gros,⁶ etc., of New Orleans, inform us that the fever of that city ceases as an epidemic after the occurrence of frost. Drs. Moultrie,⁷ Lining,⁸ Campbell,⁹ Chalmers,¹⁰ Ramsay,¹¹ Harris,¹² of Charleston, tell us much the same thing relative to the fever of that city, which invariably

¹ Drake, *op. cit.*, p. 714.

² Facts intended to prove the Yellow Fever not to be contagious, Works, iv. 155. See also iii. 98, 99, 100, 201; iv. 8, 45, 95.

³ Letters to the Governor of Pennsylvania, etc., p. 3.

⁴ Remarks on the Yellow Fever, N. O. Journ., Sept. 1845, p. 130.

⁵ Essai sur la Fièvre Jaune d'Amérique, p. 110.

⁶ Rapport, etc., pp. 6, 61.

⁷ French translation, p. 5.

⁸ Essays and Observations of Edinburgh, ii. 409.

⁹ See Watts, p. 249.

¹⁰ Climate of South Carolina, ii. 60.

¹¹ Med. Reposit., iv. 219.

¹² Barton's Journal, ii. 29.

ceases on the accession of frost or severe cold. Nor do we find matters take a different turn in that respect at Natchez. Of the epidemic of 1817, we are told, by Dr. Perlee,¹ that "on the 9th of November there occurred a severe frost, which at once arrested its progress, and permitted the inhabitants to return in safety to their homes."

The same writer, in his account of the epidemic of 1819, informs us that the weather became cool in the middle of November, and the disease began to subside. About the first of December, there having been a moderate frost, the Board of Health quickly informed the inhabitants that they could return to their homes with a reasonable prospect of safety.² In reference to the epidemic of 1823, the most disastrous by which that city was visited, Dr. Merrill remarks: "During the night of the 31st of October, a very great change took place in the weather. The wind changed suddenly from the south to the northwest, and the thermometer fell from 78 to 28 degrees in about sixteen hours. On the 1st of November it was considered safe to return to the city."³ In testimony of the salutary effect of frost on that occasion, we have the further authority of Dr. Monette.⁴ In 1825, the disease continued, with little abatement, until the 26th of October, when, as Dr. Merrill⁵ states, "we had a heavy shower of rain. The next day the wind changed to northwest, and blew strong and cold, and during the succeeding night the thermometer fell to 34 degrees, which induced many families to return to the city."⁶ Dr. Monette also remarks that the disease continued its ravages until checked by frost and cold weather, about the 28th of October.⁷ From the same writer we learn that, in 1837, "the disease continued to spread gradually, and with occasional abatements, until checked by frost, about the 25th of November."⁸ It may not be improper to remark that Dr. Merrill says, in relation to Memphis, that if the grading there "has fallen short of creating an epidemic of quite as grave a character as similar causes have elsewhere, it may be owing to the modifying influences of a few timely showers of rain or an early frost."⁹ I have now before me a list of about ninety good and substantial authorities, who describe the yellow fever as it has appeared epidemically in Boston, New York, Providence, Wilmington, Baltimore, Norfolk, Franklin, Vicksburg, Alexandria, Gallipolis, Gibraltar, Barcelona, and Cadiz; who all, like the authorities already mentioned in connection with the disease as it has shown itself in this city, New Orleans, Charleston, and Natchez, impart to us information which I recommend to the particular notice of those who deny the destroying and purifying agency of frost, of severe cold, or blasts of northerly winds.¹⁰ I must also recommend

¹ Philad. Med. and Phys. Journ., iii. 6.

² Ibid., p. 10.

³ Ibid., ix. 255.

⁴ Observations, p. 65.

⁵ Ibid., Essay, p. 59.

⁶ N. A. Med. and Surg. Journ., ii. 220.

⁷ Essay, p. 62; Observ., p. 67.

⁸ Observations, p. 70; Essay, p. 75.

⁹ Public Address on the Health and Mortality of Memphis, Memphis Medical Recorder, i. 90.

¹⁰ Dalmas, p. 39; Caldwell, Fever of 1805, p. 51; Ibid., Memoirs (1800), p. 209; ditto of 1826, p. 124; Bally, pp. 313, 314; Campbell, in Watts's Med. and Surg. Reg., p. 249;

to their attention those instances, not difficult to be found, of vessels infected with yellow fever, and which have been rendered healthy, often rapidly, on reaching a cold climate, and passing through the ordeal of frosty weather.¹

“In the event of a ship’s company being attacked by fever,” says Dr. Bryson (p. 227), “whether from external or internal causes, which shows a disposition to become general, and to assume a malignant form, characterized by intensity of action, early yellowness of the skin, and black vomit, it will be of the greatest importance for the safety of all on board, that she should immediately quit the locality where the disease originated, and proceed, with all possible haste, to some colder region; if in the south, to the southward,

Moultrie, *Dissertatio*, &c., p. 3; Monette, 1st edit., pp. 50, 60, 63; *Ibid.*, 2d edit., pp. 7, 63, 65, 67, 70; Thomas, p. 110, 2d edit., p. 14; Potter on Contagion, p. 25; Osgood, p. 17; Hosack, *Febrile Contagion*, p. 10; Facts, etc., by the College of Physicians, p. 4; *Dict. des Sci. Méd.*, xv. 357; Davidge, p. 69; Caisergues, p. 194; Valentin, p. 88; Harrison, *N. O. Journ.*, ii. 130; Chalmers, *Climate of South Carolina*, ii. 60; Bancroft, pp. 292, 407, 425; *Ibid.*, *Sequel*, pp. 89, 91; Taylor and Hansford, *Webster Collect.*, p. 148; Selden and Whitehead, *Med. Reposit.*, iv. 129, 336; Chisholm, *Letter to Dr. Haygarth*, p. 177; Currie, *Med. Register*, i. 181; *Report of Philad. Acad. of Med.*, p. 7; *Med. Rep.*, i. 406; Hardie, *Fever of New York in 1798*, p. 13; *Ibid.*, 1822, pp. 67, 68; Condie and Folwell, *Fever of Philadelphia in 1798*, p. 81; Currie, *Fever of 1799*, p. 25; *Ibid.*, on Bilious Fever, p. 15; Halphen, p. 62; Brown (S.), pp. 26, 108; Pierquin, pp. 40, 60; Townsend on Black Vomit, p. 30; *Ibid.*, *Fever of New York in 1822*, pp. 257–8; Palloni, *Sulla Febbre Gialla*, etc., p. 20; Dariste, p. 31; Jackson (Sam.), *Fever of Philadelphia in 1820*, p. 24; *Letters on the Fever of Baltimore in 1819*, pp. 80, 113; Addom’s *Dissertation*, p. 7; Hosack’s *Essays*, i. 292; Shecut, pp. 94, 100; Copland, iii. 169; *Report on the Fever of New Orleans in 1820*, p. 6; *New York Report of Quarantine*, p. 44; Stone, *N. O. Journ.*, ii. 551; Pym, pp. 2, 8, 216; *Ibid.*, 2d edit., pp. 2, 65; Smith (J. M.), pp. 81, 90, 91; Burnett (Sir W.), p. 342; Blane (Sir G.), *Dissertations*, ii. 152, 155; Caillot, p. 107; Barton (Ed.), *Fever of New Orleans in 1833*, pp. 6, 9; Chervin, *Report on Mem. by Rufz*, p. 77; *Ibid.*, *Letter to Monfalcon*, pp. 17, 18; Forry, *Climate of the United States*, p. 290; Bayley, *Letters from Health Office*, p. 9; Amiel, in *Johnson on Tropical Climates*, p. 270; *Ibid.*, *Edinb. Med. and Surg. Journ.*, xxxv. 276; Robert, *Guide Sanitaire*, i. 43; *Physical Inquiry*, etc. (N. Y.), p. 25; Gillkrest, *Cyclop. of Pract. Med.*, ii. 279; Lining, *Edinb. Essays*, ii. 409; Smith, *Edinb. Med. and Surg. Journ.*, xxxv. 40; Wood, i. 296; Chapman, *Med. and Phys. Journ.*, ix. 135; Merrill, *ibid.*, ix. 255; *Ibid.*, *N. A. Med. and Surg. Journ.*, ii. 220; Townsend on Plague and Yellow Fever, *New York Journ.*, ii. 46; Emlen, *N. A. Med. and Surg. Journ.*, v. 328; Humboldt’s *New Spain*, ii. 765; *Fever of New York in 1805*, *Med. Reposit.*, ix. 213; Vaughan, *Fever of Wilmington*, pp. 12, 20; Wheaton, *Med. Reposit.*, x. 335; Harris (Tucker), *Barton’s Journ.*, ii. 29; Monro, *Med. Reposit.*, iii. 136; Ramsay, *ibid.*, iv. 219; Seaman, *ibid.*, iv. 249; *Opinion of the Medical Faculty of Baltimore on the Fever of 1800*, *Med. Reposit.*, iv. 353; Archer, *Med. Recorder*, v. 61; Bond (Thomas), *Lecture*, *N. A. Journ.*, iv. 271; Deveze, p. 197; Brent, *Med. Reposit.*, ii. 390; Rayer, *Fever of Barcelona*, pp. 43, 48; Audouard, p. 414; Pariset, *Relat. Hist.*, p. 93; Miller (Edw.), *Rep.*, pp. 88, 106; O’Halloran, p. 118; Drake, p. 608; Reese (Meredith), *Observations on the Epidemic of 1819, as it appeared in a part of the City of Baltimore*, p. 46.

¹ Trotter, i. 357; Caillot, p. 114; Doughty, p. 25; Ferguson’s *Recol.*, p. 143; Kéraudren, p. 18; Pym, 2d ed., pp. 65, 127; Bryson, *Report on the Climate and Principal Diseases of the African Station*, p. 53; Barrington, *Am. Journ.*, xii. 309; Waring, *Med. Reposit.*, iv. 1, 234; Allan, *Edinb. Monthly Journ.*, xi. 326. See also cases of the Gen. Green, *Med. Reposit.*, iv. 1, 234; of the U. S. ship *Hornet*, *Am. Journ.*, xii. 307.

and if in the north, to the northward, avoiding the neutral ground between the trade winds. The great utility of this measure was practically tested by the *Vestal*, in 1835, when her crew were assailed by fever at Port Royal, in Jamaica, which did not cease, although she was shifted from the inside to the keys on the outside of the harbour, nor until after she had gone far beyond the precincts of the island, and entered the twenty-seventh degree of north latitude on her way to Bermuda. The crew of the same vessel, although not the same men, after having been paid off and reeommissioned, were again violently attacked by fever whilst cruising among the Windward Islands of the West Indies in the latter part of 1839. Instead of running at once to the northward, she proceeded to Carlisle Bay, where she remained about a fortnight; during that time the disease evidently increased in malignancy, and carried off a considerable number of men. She was then directed to proceed to the northward, and again the disease disappeared a few days after she had crossed the tropics. The ship's company of the *Vesuvius* were promptly relieved of an invasion of fever by her being ordered from Saerifieios, where it was contracted, to Halifax."

These facts, observed in such divers places, and attested by so many respectable and even eminent authorities, and which ought to be, and I have little doubt are, familiar to the disbelievers in the efficacy of frost in arresting the progress of malarial fevers, taken in connection with the statements made by Humboldt, in reference to Vera Cruz, that the vomito or yellow fever seldom begins to prevail there before the average temperature of the early months reaches to 75.2 (Fahr.); that in December, January, and February, when the heat remains below that limit, and the cold is often very sharp, the disease usually disappears completely; that the latter declines sometimes very suddenly, through the influence of the north winds (*los nortes*), an observation also made in more southern climates;¹ that the later these continue to blow in the spring the later the fever makes its appearance; and the sooner they commence in the autumn, the less apprehension is felt respecting its continuance: all this, I say, should suffice to carry conviction to all unprejudiced minds.²

If the statement of the purifying agency in question is denied—if, while acknowledging, what no one can justifiably impugn, the truth of the events recorded, it is maintained that, in the association of the accession of frost with the cessation of fever, we are to recognize nothing but the occurrence of a fortuitous coincidence, we must admit that the frequency of that coincidence, and its manifestation in so many and diversified places, is to be viewed as a matter of the utmost astonishment, seeing that everywhere the one event is sure to follow closely on the heels of the other.

As an offset to this, it has been remarked that, in the Havana, the same thing happens without frost; and that hence, when the latter occurs in any place, at the close of the epidemic season, we have no right to affirm that it

¹ Catel, pp. 8, 9; Pym, pp. 2, 8, 216; Caillot, p. 106; Dariste, p. 31.

² Humboldt, 4to., ed., p. 765.

produced the effect assigned to it. This cessation of fever without the aid of frost may be true. Indeed, I know full well that it is annually observed in tropical climates—not only in the Havana and other parts of Cuba, but in the West Indies generally, and on the coast of tropical America and of Africa. Nor could it be otherwise. There, frost or intense cold is a thing never heard of, and yet fever epidemics come to an end. I know, also, that it has occasionally occurred in some parts of Europe, in the United States, and even in this very city. I know full well, besides, that in some epidemic visitations, observed in both tropical and extra-tropical regions, the disease has stopped, not only before the accession of frost, or cold or cool weather, but even before the cessation of great heat. None will be disposed to deny such occurrences, who have perused attentively the history of West India epidemics, and of those that appeared in this city in 1803, in Mobile and New Orleans in 1847, as described by Drs. Caldwell,¹ Nott,² and Fenner,³ and in Leghorn, in 1804, as related by Palloni. The same thing occurred in this city and in New Orleans in 1853,⁴ as also in Charleston in 1854. Indeed, the occurrence is common in New Orleans, for we are informed by Dr. Barton that in a long series of fifteen epidemics, no one has required that “the temperature during which it has ceased its ravages, should descend to the limit of frost, and that actually the minimum at which any epidemic ceased, was a small fraction over 60°.”⁵

But these facts, true as they doubtless are, do not in the least invalidate the reality of the power which I have, in common with so many others, ascribed to cold and frost; for, from the circumstance that yellow fever has sometimes stopped in this country, and usually ceases completely, or in great measure, in tropical regions, without the aid of frost, and sometimes before a cessation of high atmospheric heat, we cannot argue that frost, or severe cold, when occurring before the fever has been arrested in its epidemic course, will not produce that effect. This cessation often is too instantaneous, and is, besides, too constantly noticed, after its occurrence, to be ascribed to any other agency. The two results are not incompatible. While, therefore, we maintain that the effect arises most usually from the last-mentioned cause, we admit, what experience has sufficiently demonstrated, that the same beneficial change is also produced through other agencies—copious rains, violent storms, heavy winds—especially from the north, desiccating and long-continued heat and droughts, and not unfrequently from the want of subjects susceptible to the morbid impression of the poison; and that, at times, it is brought about by a change in the epidemic meteoration, the evolution of ozone in the atmosphere, or some other purifying influence, the nature of which has so far eluded our researches; a change which, as Dr. Smith⁶ remarks, is, in effect, equivalent to the reduction of the temperature to 32° of Fahrenheit. The same effects have been noticed in other forms of malarial fevers; and every

¹ Med. Reposit., vii. 149.

³ New Orleans Journ., v. 203.

⁵ Introd. to Rept. on Fever of 1853, p. xiii.

² Charleston Journ., iii. 5.

⁴ Fenner, Trans. of Assoc., vii. 461.

⁶ On Epidemics, p. 174.

one is familiar with the fact that, in Egypt, where, for reasons mentioned, the plague prevails during the winter months, it stops in June, under the empire of the same parching heat which, in that country, arrests the progress of animal putrefaction. But, I repeat, were these occurrences more frequent than we know them really to be, they could not serve to counterbalance and overthrow all that has been said in support of the agency ascribed to frost and severe cold, in arresting the progress of yellow and other malarial fevers. They could not, in any event, serve to denote an affinity between yellow fever and contagious diseases, and prove that the former does not arise in temperate regions from local causes, seeing that in tropical regions, where the fever is indisputably of local origin, it ceases, and must, necessarily, cease without frost.

From the difference noted between contagious fevers and the yellow fever relative to the subjects mentioned, and the similarity, on the contrary, existing on these same points between that disease and the ordinary fevers of summer and autumn, the conclusion is natural, that the yellow fever must approximate to the latter rather than to the first mentioned diseases as regards origin and mode of transmission—that being a disease of high temperature, which is inimical to, if not destructive of contagion, and arrested by cold, which never prevents, if it does not promote, the diffusion of that power, it must, like other febrile diseases of hot climates and seasons, arise from the operation of some of the several modifications of domestic causes, which require long-continued heat for their formation, and are arrested by cold; as well as be classed among those of a non-contagious character.

Contagionists, both here and abroad, while admitting (as they could not avoid doing), the sudden extinction of the yellow fever by frost, have endeavoured, but in vain, to reconcile the fact with their theory. Sir William Pym, unwilling to perceive the force of the objection that may be urged on that score in favour of domestic origin and non-contagion, contents himself with alleging that it is one of the peculiarities of the Bulam fever, which differs from all other diseases in having its contagious powers increased by heat and destroyed by cold, or even by a free circulation of moderately cool air. We find, also, that the late Dr. Currie, of this city, was of opinion that the effects in question only proved that the contagious principle which is the cause of the yellow fever requires the same portion of caloric to preserve its volatility, and keep it suspended in the atmosphere as the effluvia from domestic causes.¹ Chisholm, on his side, says that since “it is admitted that yellow fever is the product of infection (contagion), in combination with a high temperature”—“and of this there cannot be any reasonable doubt;” it is evident that “cold weather must have extinguished it.”²

In order that the peculiarity contended for by Pym should really appertain to the disease, it would be first necessary to prove, that the latter is truly possessed of the powers in question—which is exactly the point at issue; and to give some good reason to account for a fact so much at variance with

¹ Medical Register, i. 181.

² A Letter to Dr. Haygarth, &c., p. 177.

every other of analogous kind. But this task remains yet to be accomplished. Pym and the other writers mentioned have begged the question, and we are left to wonder how it comes that Dr. Currie did not perceive the improbability of two morbid principles so different from each other—exhalations from domestic causes and the supposed matter of contagion—being acted upon exactly in the same manner by any one agent, while that agent produces, a different result on the contagion of other fevers.

More natural is it to regard, with Dr. Bancroft and others, the invariable extinction of this and of autumnal fevers by frost, as an evidence of these proceeding from a cause existing in the external open atmosphere, or within, or near the surface of the earth, and not within the chambers of the sick, which are artificially warmed upon the occurrence of frost; and consequently, to admit that this fever is not the effect of personal contagion, which being generated by those who labour under the disease, cannot, within their beds or bedchambers, be ever exposed to the action of frost.¹ To the latter conclusion we must the more readily incline, inasmuch as were the disease contagious and not the product of a cause independent of the sick, individuals who approach the beds of the latter, where the morbid agent could be generated and accumulated, and where the frost does not penetrate, would, in consequence of being—owing to their confinement—unprotected from the low temperature of the external atmosphere, and exposed to a concentration of the contagious matter, be unquestionably more liable to take the fever, than those who remain aloof; whereas it is found, that those who nurse or visit the sick are never attacked by it subsequently to the occurrence of frost, to which they are not exposed;² and that this frost, while failing to promote the transmission of the disease, acts injuriously on the patient, and generally hastens his decease.

Influenced in its prevalence and severity by Atmospheric Vicissitudes.—We have seen that though contagious complaints may prevail at all seasons of the year and in all latitudes, as well as under every variety of hygrometrical states of the atmosphere, they do so more particularly in cold weather, and in northern countries; but that yellow fever, on the contrary, like diseases arising from domestic causes atmospherical and terrestrial, and devoid of contagious properties, requires for its development a long continuance of high temperature and a certain quantity of humidity, and is invariably and suddenly arrested by the accession of frost or heavy rains, &c. It remains now to be mentioned that, contrasting again with contagious diseases, which, when once established, follow their epidemic course little if at all disturbed by external influences, and assimilating itself to febrile diseases of domestic origin and of a non-contagious character, the yellow fever is modified in the extent of its prevalence, as also in the severity of its attack, by variations of temperature or weather. Thus, it is found that during the course of an epidemic of that disease, we can always notice from day to day some modification in the severity and character of the cases, as well as in the multiplication of

¹ Bancroft, Seq., p. 89; Rush, iv. 155; Miller, p. 101.

² Ibid., p. 91.

them; and at the same time generally discover that these modifications coincide with particular and evident changes in the sensible qualities of air. Lining remarked more than a century ago, that the disease was more or less violent according to the heat or coolness of the weather. "In hot days," he remarks, "the symptoms were not only more violent, but in those who seemed, in moderate weather, to be on the recovery, or at least in no danger, the symptoms were all so greatly heightened when the weather grew considerably warmer, as frequently to become fatal. In cool days, the symptoms were not only milder, but many who were apparently in great danger in hot days, were saved from the very jaws of death by the weather becoming happily cooler."¹ Dr. Vaughan, of Wilmington (Del.), speaks of the "manner in which the fluctuating malady corresponded with the varying states of the weather" (p. 20). In Barcelona, 1821, it became severe in hot weather, diminished when the weather became temperate, and disappeared with cold (*Rayer*, p. 48). At Cadiz, in 1804, "the state of the weather, and the temperature of the atmosphere, had a sensible influence; for, upon a hot and dry day, or during the Levant winds, a greater number of individuals were taken ill, and those already under a fever became worse."² At Gibraltar, in 1813, the fever commenced on the 8th of September, after a long continuance of early winds and sultry weather, with the thermometer at 80° in the shade. It made little progress till the 20th, when a very hazy close day augmented the number of attacks from 6 to 33. From this time the numbers appear to have daily increased, especially during the hazy weather which occurred in the beginning of October.³

Dr. Rush called attention to the fact, that though the fever is arrested by frost, heavy rain, intense heat, and high winds, it is rendered worse by cool, warm, damp weather, and light winds (iv. 156); and that a comparison of the daily variation of the mortality with that occurring in the weather will show the influence of the latter on the disease (iii. 96). This was the case in 1793, when it was found that every change that was less than that which produces frost, evidently increased the number of the sick (iii. 50). Of 1794, the same author says that, from the influence of occasional showers of rain in September and October, the disease was frequently checked so as to disappear for two or three days in his practice; while showers of rain lessened, moist and damp weather, without rain, increased it. The cold weather of October checked, but did not banish it; and, as the succeeding months were very moderate, the disease occasionally appeared (iii. 201-2).

Dr. Deveze, whose experience in yellow fever was chiefly obtained in this city, remarks (p. 191), that when an epidemic of this disease is well established, it seldom remains in the same state throughout. In some instances, on the contrary, it breaks out with great violence, and after spreading rapidly, and exhibiting great violence, it stops suddenly. In other instances, after creeping along very gradually, and remaining stationary for a long time, it

¹ Essays and Observations, ii. 426-7.

² Sir James Fellowes, p. 196.

³ Burnett on Mediter. Fever, p. 341.

suddenly spreads widely, and assumes a more violent character. Often its course is interrupted during several days; but it breaks out afresh with redoubled violence. In a word, the yellow fever presents all manner of variations, in point of intensity and diffusion, during the course of the same epidemic; and all these variations constantly correspond with like changes in the condition of physical agents. Winds and rain increase or diminish its intensity. Heat generally increases it, though, when excessive and long continued, it sometimes puts a stop to its epidemic progress. These phenomena are recorded in the history we possess of various epidemics; and if we reflect on the mode of operation of those agencies, and the circumstances which accompany the changes in question, we shall perceive how easily they may be accounted for agreeably to the laws of infection. We find that strong, cold, and continued winds generally act by diminishing the number of the sick, and the intensity of the disease; that warm, mild, and damp winds usually produce an opposite effect; that copious falls of rain are unfavourable, and light showers favourable to the extension of the disease. The same variations I have just cited in relation to epidemics of yellow fever, are observed in individual cases of the disease. According to the winds, the rain, and especially according to the degree of heat, it diminishes or increases in intensity.

While in contagious diseases, the condition of the sick is but little affected by such circumstances, whether the patient remain in a warm and close place, or is removed to one differently situated, the disease usually is but slightly modified by the change. These modifications of the yellow fever, arising from a difference in the character of the physical agents to the influence of which the sick are exposed, may be exemplified in the effects obtained by the removal of patients labouring under the disease from the place where they took it to another at a greater or less distance. "It is thus," says Dr. Deveze, "that in all the epidemics of Philadelphia, it was seen that individuals removed to the country—where the atmosphere is cooler, and the ventilation more perfect—were more easily cured than those that remained in town. It is from a knowledge of this phenomena that in St. Domingo, patients in a condition to bear removal were sent to the mountains—that at St. Lucia they are sent to Gros Ilet (Pigeon Island). If, on the other hand, the place where patients are sent is warmer, less properly ventilated than the one where they had sickened, a contrary effect is obtained, and the danger is increased" (p. 193).

In the West Indies it is often found, that individuals affected with the fever are much benefited by going to sea. In a few days a ship's crew, from being in a sickly condition, have been known to become healthy, while among those who remained ashore, and in the focus of the infection, the mortality was very great. "Those who were carried to sea in the Middleburgh ship of war," says Lind, "speedily recovered. Their fluxes and fevers lost the dangerous symptoms, and an immediate stop was put to the mortality which raged among them at Curaçoa. During the sickness at Cadiz and Pensacola, the removal of the sick into ships which lay at anchor in a pure air produced the same happy effects. A malignant sickness in the island of Grenada and the

Grenadines proved very fatal to the English who, upon the conclusion of the late peace, first went over to settle there; but it was observed that such of the sick as were put on board the ships to be sent to Barbadoes, generally recovered at sea before they could reach their intended port" (2d ed., pp. 189, 190). Dr. Lind elsewhere refers to the Greenwich Hospital of Jamaica, which was erected in a tainted atmosphere, and states that "it was observed that the yellow West India fever often reigned there, attended with the most profuse evacuations of blood by vomiting, stools, and even by every pore of the body, where no such symptoms distressed those patients whose cases had been similar, and who were permitted to remain in their ships" (*Ib.*, p. 188). Many instances of the kind are on record; while the advantageous effects of a change from a highly heated to a cool atmosphere are dwelt upon in a particular manner by Moultrie, who states that during a violent epidemic of the disease, the thermometer fell suddenly on the 21st of September to 58° (Fahr.), and that having visited his patients he found them all greatly improved.¹ During the epidemic of Martinique, in 1837, it was remarked that whenever the winds blew from the west, the number of the sick increased.² The same fact is mentioned by Catel (p. 9).

The results to which I have thus called attention being those obtained not in yellow fever only, but in all febrile complaints arising from causes of a general and external kind, and which are void of contagion, and contrasting with those observed in contagious complaints, we find therein an additional reason for regarding that disease as assimilating itself, in an etiological point of view, with the class of fevers first mentioned rather than with those last, and for denying its possessing or acquiring contagious properties.

Its Prevalence in tropical regions coinciding with its Occurrence in this country, no Proof of Contagion and Importation.—The prevalence of the yellow fever in the West India Islands, or other parts of tropical regions with which commercial intercourse is held—and where the disease is acknowledged to be endemic, and to originate from the agency of local or domestic causes—during the periods of its existence in this or other cities of the Union, or of Europe, cannot be urged as a valid reason in favour of the latter having derived the disease from the former. To be of avail, it should be shown not only that the disease existed in tropical regions *prior* to its appearance in the place where it is thought to have been introduced, but that it should have been prevailing in the very port whence vessels likely to have transported it sailed; and secondly, that there should have been a communication, previous to the outbreak of the disease in any part of temperate climates, between that locality and some infected place in tropical regions. In some instances, as will be seen when we come to inquire into the origin of each separate epidemic, this condition is far from having been fulfilled. It has happened that the fever has broken out in some localities of temperate regions before its appearance in the only place in the West Indies whence it could be supposed to have been brought, or that it has appeared in the latter

¹ *Oper. cit.*, p. 3.

² Chervin, Report on Rufz, pp. 26-32.

after the departure of the vessels which could, with any show of probability, be accused of having introduced it. Such will be found to have happened in Cadiz in 1800, in Leghorn in 1804, in Barcelona in 1821, and in other places which might easily be named.

In some cases, the fever has prevailed when, from the absence of any arrival from an infected port of tropical or other regions, prior to its manifestation, or at any time during the summer or autumnal season, it was impossible to attribute it to intercourse with such regions. This was observed in this city in 1805, 1819, and 1853, and at Cadiz in 1819, on the latter of which occasions the ultra contagionists, for want of a more probable source, referred it, as already stated, to an arrival some time previous from Calcutta! Now, in these instances, it matters not whether the fever prevailed or not, either in tropical regions or in this country and Europe; for in one instance it occurred too late in the former region to justify us in regarding it as the parent of the outbreak in the latter, and in the others, though it may have existed within the tropics prior to its appearance in this city and Cadiz, there were no means of communication by which its introduction could be explained. If, therefore, the disease could, under those circumstances, make its appearance in the places mentioned, its want of connection with the prior or contemporaneous prevalence of the same disease in tropical parts is fully established; and hence the conclusion is natural, that what has happened in the instances specified may have happened and continue to happen in every other instance, and that the occurrence of the fever in our city and in any other parts of temperate climates is in no way necessarily connected with its prevalence in the West Indies, on the coast of Africa, or in any part of the world where it is endemic and of local origin.

The same train of reasoning is applicable to those instances of outbreak of the disease in this city and other localities of temperate—or even of tropical—regions during its prevalence in places of the same regions or countries with which the former generally communicate; for although such outbreaks often follow on the arrival of vessels, steamboats, individuals, or merchandise from the infected spots, it not unfrequently occurs that no such arrivals had preceded the appearance of the disease. The occurrence of the fever at Memphis in 1828, at Brandywine Village in 1853, and at Gallipolis in 1795, may be cited in proof. I may also call attention to those instances of the sudden appearance of the fever in places, in this country and elsewhere, when the only cities whence the supposed contagion could have been received, and whence they are usually thought to receive it, remained uninfected. This occurred at Natchez in 1823, in which year New Orleans was healthy. Surely, if the disease can and does break out in such places, under circumstances of the sort mentioned, there can be no reason to conclude, when the occurrence is preceded by an arrival from an infected place, that the disease is due to that arrival, and must be necessarily traced to those places. The more zealous contagionists will doubtless insist upon the fact that the vessels which left tropical ports prior to the outbreak of the fever may still have introduced the disease in this country or Europe, by transporting the con-

tagious germs derived from some sporadic case, and which had laid dormant since some preceding epidemic season. They may say, also, that if the disease could not, from want of means of transportation, have been introduced in extra tropical ports, directly from an infected place within the tropics, it may have been so in an indirect manner; the disease having been brought, already formed, or in its germinal state, into some other place with which the one subsequently infected has communicated. But, in the former case, the explanation is founded on the admission of facts, the correctness of which is far from being proved—the transportation of contagious germs, their derivation from a few solitary cases (the very occurrence of which is doubtful), and their lengthened preservation from one epidemic season to another. In the second case, it would be necessary, before recognizing the validity of the explanation, to show that arrivals in neighbouring ports, from infected regions, had in every instance taken place. Now that it has not always been so, is shown by the history of the epidemic of Cadiz in 1819; for on that occasion, not only had no vessel capable of having introduced the disease arrived in that city, but none had reached any neighbouring port. It would be necessary, also, to explain how the disease or its contagious germs could infect a place which it reached in a roundabout way, and spare the place that had received it directly from a tropical region.

In addition, it may be mentioned that the disease exists in some one or other of the West India Islands every year, and that with these this city and most of the American seaports, as also those of Southern Europe, keep up constant commercial intercourse, and that, nevertheless, the greater number of such ports remain free from the disease season after season; and that, too, although on occasions of immunity the preventive means employed are not a whit more stringent or more carefully enforced than in seasons of sickness.

Exemption of other parts from the Disease when it prevails in some places, no Proof of its Exotic Origin and Contagious Character.—The exemption of other seaports or places of this country, when the fever has prevailed in Philadelphia, and the like limitation of the infection to one or a few cities, when the disease has appeared elsewhere, cannot be adduced as an argument in favour of contagion and importation. This circumscribed limitation is far from being incompatible with the doctrine of those who deny the contagious character and exotic origin of the disease. If, as they maintain, the disease is due to the agency of local causes, we at once perceive the impossibility of its being otherwise than circumscribed within comparatively narrow limits, and obtain an easy explanation of the fact of one or a few localities being affected, while all others, even at a short distance, remain exempted from the infection; for such sources of infection, whatever be the disease they produce, are always more or less limited in point of extent, and their sphere of operation is restricted to a small locality—a town or city, or a section of country. The limitation of the infection to one or a few cities, and the exemption of all others, does not therefore involve the necessity of admitting the contagious character and exotic origin of the fever, since they

can be as readily explained on principles of a very different nature. To say that if the yellow fever arose not from an imported contagion, but a distempered state of the air, it ought to spread far and wide, instead of limiting itself to a particular city, is to betray an imperfect knowledge of the characteristics of such distemperatures, and especially of that to which the disease in question may be ascribed. No one, at the present day, whatever may have been the case formerly, supposes the fever to be the product, exclusively, of a peculiar meteoration. The latter promotes its diffusion, but cannot be its efficient cause. All who deny its exotic origin and contagious character, and many of those who admit the contingent development of the latter, look to circumstances of locality for that efficient cause—circumstances to the like of which other forms of febrile complaints have been satisfactorily traced. If in this they are correct, we may perceive that the distempered state of the air contended for, though energetic enough to produce the disease in question in the locality where it is generated, will not and need not be sufficiently diffused to affect with equal certainty a wide extent of country.

The fallacy of the argument may be illustrated by the history of other febrile complaints. Remittent, bilious, and intermittent fevers are undeniably the products of local causes, and devoid of contagious properties. Now, although far less circumscribed in the sphere of their prevalence than the yellow fever, still that sphere is always subjected to certain, and at times to considerable, limitations. We find, on some occasions, the disease spreading over an immense space of country, while on others it prevails around or in the suburbs of one or a few of our cities, leaving the others free. The next year, or several years after, other localities suffer in their turn; and yet no one dreams of contending, on the ground of the circumscribed extent of their spread, and from the exemption of other places, sometimes very near to those infected, for the exotic origin and contagious character of those fevers.

Nor is this all. By those who advocate the belief in the necessity of an imported contagion into this city and other places of temperate climates, the disease is admitted to be a native of tropical regions—the West Indies, the coasts of South America and Africa. They admit that there it originates from the operation of causes appertaining to the localities; and when it breaks out in any of those, a large portion of the medical men—whether contagionists or otherwise—unite in rejecting the idea of its having been brought from some other place. Now, it does not require much research to discover, that the disease is very far from spreading at the same time to all the places liable to its visitation. While it attacks one or more of these at one period, it spares others; and perhaps the next year it visits the latter and leaves the former untouched. During the long periods of exemption—of ten or even twenty years—noted in some of the West India Islands, which are not as far from each other as are some of our own cities, the fever has existed sometimes in an unprecedented extent in other islands—intercourse between them remaining unshackled. The same has occurred in the several parts of the same island. The fever does not always prevail contemporaneously in an epidemical form in the Havana, Matanzas, and St. Jago. A like observa-

tion has been made in Jamaica, where Up Park, Kingston, Port Royal, Fort Augusta, and Spanish Town, do not all suffer in the same season.¹ Up Camp and Kingston suffered severely in 1819, 1822, 1825, 1832; not in 1821 and 1827. In the first year of the second series, Port Royal had a fatal epidemic; and in 1827, Fort Augusta, which had been free in 1819, 1822, and 1825, was devastated by it. Bermuda remained for many years free from the disease, while other islands were sorely afflicted, though the intercourse with these remained uninterrupted.² When the fever prevailed in Demerara in 1837-8, it was not communicated to Barbadoes and other islands.³ On the western coast of Africa the same thing occurs. In 1823, the fever prevailed at Sierra Leone, while Fernando Po was free. In 1830, the reverse occurred; Fernando Po suffered, and the disease spared Sierra Leone. (*Bryson.*)

Assuredly, if the argument under consideration is admitted to be valid as regards this city and other parts of temperate climates, it ought to be equally so in reference to tropical regions; and the impossibility of urging it as to these must be viewed as a strong reason in favour of rejecting its application to the former.

Malignancy of the Disease increased by continuance of residence in the Infected Localities.—When once produced, diseases arising from the operation of specific poisons transmissible from one individual to another, are no longer influenced, as to the malignancy of their character or the extent of their duration, by a continued exposure to the cause from which they originally sprung. In other words, individuals labouring under a contagious disease may remain in close approximation to, and surrounded by, other individuals similarly affected, without experiencing thereby an aggravation in the disease, which runs its usual course uninfluenced by the further exposure to the contagious poison (*Deveze*, p. 149). On the other hand, patients suffering from such complaints are little, if at all benefited by seclusion from others alike affected. The slightest exposure to the effluvium of the disease in its less malignant form, or the introduction into the system of the slightest quantity of the virus, when the latter is tangible—even when taken from persons labouring under a mild attack—is sufficient to produce the disease in its worst form; and every subsequent exposure, however prolonged or complete—every further inoculation proves innocuous, and adds in no way to the violence of the disease, which may be mild even when the case or cases from which it proceeds are of the most malignant character, and placed under the most unfavourable circumstances.

In saying this, I am not to be understood as doubting the deleterious effects of localizing agencies on zymotic diseases arising from specific contagious ferments or poisons. It is not to the effect of agencies of that sort I here allude, but only to the action of the poison or efficient cause of contagious disease. Such diseases, I repeat, are not aggravated by prolonged

¹ Tullock's Rept., and Arnold on Rem. Fev.

² Doughty, p. 199.

³ Blair, p. 55.

exposure to the cause which produced them, and if the malady is rendered more malignant in places crowded with individuals affected with it, the mischief is not ascribable to the fact of these being so diseased; for the accumulation will be found equally hurtful if the individuals so collected labour under other complaints, or no complaints at all. Every one knows that while the febrile exanthemata are never produced by the malarial exhalations evolved from foul localities or marshy surfaces; while typhus and typhoid fevers are, as it is said, seldom the offspring of the former, and certainly never of the latter; while none of these diseases are occasioned by the ingestion of putrescent food, by the use of foul water, by imperfect ventilation, by starvation, by excessive muscular exertions, by the intemperate use of alcoholic liquors, and the like, and while, with the exception, perhaps, of typhus, they do not arise from the effluvia proceeding from the human body—particularly the lungs and skin—and consisting of the effete and highly putrescent matter mingled with the air or perspiration—it is a notorious fact, that they are principally rife in situations where such influences operate, and strike with greater violence, malignancy, and fatality among individuals exposed to their baneful effects. This is true, whether the disease be the product of a zymotic poison floating in the atmosphere, and independent for its development of any organic process, as Asiatic cholera; or whether it arises from a poison formed in the system and transmissible from one individual to another through means of contact, or the medium of the atmosphere; or whether, again, it is due to a particular poison proceeding from external sources of animal or vegetable decomposition, or from the result of a morbid condition of the system, as is the case, perhaps, with puerperal complaints, erysipelas, and *surgical fever*. But I again remark that the tendencies of their localizing causes are not less injurious in the way mentioned, on individuals labouring under a truly contagious disease, than on continued exposure on the part of such individual, to the poison giving rise to the disease.

While such is the case with regard to contagious diseases generally, febrile complaints arising from the influence of domestic and external causes—causes independent of any effluvium emanating from the sick—present very different results; for in respect to them it is found, that though the localizing agencies referred to produce baneful effects; these effects are principally noticed in places where the prevailing morbid poison is evolved, and disinfects the atmosphere. In other words, a continued exposure to the morbid agency producing these complaints almost invariably aggravates the character and enhances the danger of the case. This has been long ago insisted upon by Lind, who remarks that fatal experience has taught that the most sovereign remedies, and the best methods of cure that can be proposed for the relief of such persons while they breathe an unhealthy air, are most frequently rendered inefficient; that diseases in such a situation generally become more anomalous, and are both attended and followed by such symptoms as, in all probability, could seldom appear in a purer air (*Tropical Climates*, p. 183). The same writer states that, of many thousand patients afflicted

with fevers when he visited Haslar Hospital, and who were removed from Spithead, from the ships in the harbour or from the warm infirmary at Portsmouth, where the air is foul, many hundred afflicted with the most dangerous and malignant symptoms of that disease, were greatly benefited by a change to the pure air of the hospital (p. 180).

The noted epidemic of Naples, in 1764, so ably described by Sarcone, furnishes an additional and striking evidence of this; for it was there found that the sick who were removed to hospitals which stood near the sea recovered much quicker than in other places, and that few of them died; while those who remained in the centre of the city were carried off in a frightful proportion. (*Lind*, p. 201.)

Witness the instances of individuals who reside in sections of country noted for the prevalence of bilious remittent and intermittent fevers. With such as remain within the focus of the infection, relapses are frequent, the disease tedious and of more severe character, while those who, after taking the disease in such districts, are removed beyond the reach of the morbid cause, are more slightly affected and more easily cured. As regards the remitting fever of our country, it is well known that the mortality, in proportion to the sick, is much greater among individuals who remain in the infected district than among those who remove to healthy situations. In a word, the danger increases in proportion to the length of time the person affected has been exposed to the cause of the disease.

Now, when we examine what occurs in the yellow fever, we readily find that it approximates, as regards the peculiarity in question, not to contagious diseases, but to complaints arising from the operation of causes of a local and external kind, and spreading in a manner totally different from the former. The experience obtained during the epidemics of this city is conclusive on the subject; for patients who were allowed to remain in the infected district where they had sickened, generally experienced the disease in its most malignant and fatal form, though sleeping in well ventilated and clean apartments, and effectually separated from other individuals labouring under the same disease. The effect was still more strikingly noticed when such patients were transferred to more unhealthy spots. On the other hand, those who were removed beyond the limits of the infected district were, as a general rule, found—whatever might be the hygienic condition of their new habitations, and however surrounded they might be by other patients—to suffer from a milder and less dangerous form of the fever.

Nor have these facts been noted only in Philadelphia. They have occurred in every other place where the yellow fever has prevailed to any extent. Some of the facts to which reference was made in the preceding section, and which were borrowed from Deveze, Lind, and others, show as well the benefit obtained by a removal from the source of infection as that derived from a change of temperature and a freer circulation of air; and others may easily be found to confirm what was said on the subject. This is true not only in reference to the condition of individuals labouring under the disease, but to that of persons who have recovered; and no one who has examined the sub-

ject will feel disposed to gainsay the statement of the late Dr. Vaughan, of Wilmington (Del.), who found that "a noxious state of the atmosphere was manifested by the lingering states of convalescents who remained in the contaminated region, while those who removed into the country were speedily restored to health."¹

CHAPTER XVII.

PROOFS OF NON-CONTAGION—CONTINUED.

Epidemic Influence felt by those who avoid Exposure to the Sick.—During the prevalence of an epidemic disease arising from the introduction from abroad of the specific cause on which the existence of that disease depends, and requiring for its diffusion the mediate or immediate communication of the well with the sick, persons placed beyond reach of the contagion experience no ill effect from the epidemic influence. Some may be affected so slightly as not to feel the necessity of confinement, or of making a change in their mode of living, or they may exhibit but one or few of the symptoms of the disease; but the latter is always recognizable, and most, if not all the cases may be traced to exposure, more or less direct, within the sphere of the contagion, which in most, if not all those diseases, does not extend beyond a few feet. If those not so exposed suffer from indisposition, their symptoms are usually of a nature different from those produced by the reigning disease, and due to causes of the ordinary complaints of the season, or perhaps to the general epidemic constitution of atmosphere existing at the time, and through the influence of which the disease in question is generally diffused and modified.

In diseases arising from endemic influences, and prevailing epidemically—which are due to local and external causes, which spread solely through the agency of the atmosphere, and independently of any morbid poison exhaled from the sick—the effect is different. Remitting and intermittent fevers, and other affections of similar kind, which take their origin in the way mentioned, may be cited in illustration of this fact, for, during the time of their prevalence, but more particularly at periods of extensive diffusion, the endemic or epidemic influence is almost universally felt whether or not persons so affected communicate with the sick; and if, in some cases, the symptoms presented appear due to the action of the general atmospheric constitution, in the large majority they are evidently—though often scarcely amounting to actual disease—the effect of the same local cause which gives rise to the prevailing complaint. This influence has been noted in epidemic malarial or autumnal fevers of all countries, whatever be the grade and type they may

¹ Vaughan on Fever of Wilmington, p. 20.

assume. It has been referred to by Schnurrer,¹ and other writers on epidemics. The effect on the blood has already been spoken of in the chapter on that fluid in its symptomatological relation, and need not again be reverted to here. Nor shall I do more than remind the reader of the power of a paludal atmosphere in enfeebling health and lessening the duration of life. On these topics I have elsewhere enlarged somewhat in detail, and must refer to what was said on that occasion.² It will be sufficient to remark that, during the continuance of autumnal fevers—though particularly in times of wide-spreading and long continued epidemics—the system experiences, besides the change in the blood above alluded to, modifications of greater or less magnitude, as exhibited in the physical condition of the spleen, liver, or other organs, or simply in their functional derangement.³ The presence of the poison in the economy reveals itself by modification in the colour of the skin and other tissues. Enlargement of the spleen and liver, even in those who have never been sick, but have been some time exposed to the action of malaria, is not uncommon. It retards the convalescence from other complaints. Our countryman, Dr. Holmes, whose experience on this subject is ample, remarks: “I believe there is scarcely a person, even in good health, in a miasmatic region, who is not subjected in some degree to the effect of the unseen agent around him. His rest will be broken at night; his appetite will not be so good as formerly; he will not enjoy that feeling of full health which he has been accustomed to; his system will be attacked from time to time by the offshoots of the diseases preying on others around him, though he will not be sick, and may escape with a like freedom from disease during all his sojourn in the country; but the whole constitution seems in some degree to labour under the influence of malaria; slight wounds, that would heal elsewhere in a few days’ time, must here undergo the slow process of suppuration and granulation; and even this does not come on easily, the wound remaining for days without any visible advances towards a cure.”⁴

In diseases arising from a peculiar atmospheric constitution or epidemic meteoration, the diffusion of these minor symptoms is also strikingly illustrated, as any one may know who has noted the progress of the influenza, of Asiatic cholera, of the wide-spreading pneumonia typhoides which prevailed in this country in 1812, '13; of the sweating sickness of 1483, '5, as well as of the black plague of the middle ages. From each of these, besides the many who were attacked with the disease in full force, thousands suffered from some one or a few of the symptoms in a mitigated form.

These effects, which are noticed in meteorologic disease, as also in febrile complaints due to endemic causes, but which are not attendant on epidemic contagions, are, as it were, characteristic of the yellow fever; for, during its epidemic existence, large numbers suffer in different ways, from slight indispositions, or exhibit in various forms the impress of the cause which

¹ *Materiaux pour servir à une doctrine des Épidémies*, p. 40.

² La Roche on Pneumonia, &c., pp. 287–295.

³ Boudin, *Fièvres Int.*, pp. 188–9; Holmes, *Am. J. (N. S.)*, pp. 307–8.

⁴ Holmes, *op. cit.*, p. 308.

gave rise to the disease—a result easily accounted for on the supposition of the latter being due to the action of an agent of a general and local kind, distinct from the effluvium emanating from the sick; and incompatible with the idea of the fever being diffused through the influence of contagion.

In speaking of the epidemic of 1793, Dr. Rush says: “After the 15th of September, the atmosphere in the city was charged with miasmata; and there were few citizens, in apparent good health, who did not exhibit one or more of the following marks of their presence in their bodies: A yellowness of the eyes, and a sallow color upon their skin; a preternatural quickness in the pulse; frequent and copious discharges by the skin of yellow sweats—sometimes of an offensive smell; a scanty discharge of high-coloured or turbid urine; a deficiency of appetite, or a greater degree of it than was natural; costiveness; wakefulness; headache; a preternatural dilatation of the pupils” (iii. 84, 85). The same writer, in his account of the slighter epidemic of the next year, speaks of a peculiar smell of wine of sulphur produced by the perspiration of his body, which he had noticed in 1793, and again showed itself that year, and which, he thought, indicated his being strongly impregnated with miasmata (iii. 217). But, as he was constantly in close approximation with the sick, and the effect may, by contagionists, be attributed to that exposure, I shall not dwell upon it. He mentions, as an additional proof of the presence of the seeds of the fever in the system in himself and others, that wine, even in the smallest quantity, imparted a burning sensation to the tongue and throat, such as is felt after it has been taken in excess, or in the beginning of a fever.

In Baltimore, according to the late Professor Potter, hundreds who were not confined, and who never took medicine, experienced the effects of the general cause under a variety of forms, such as nausea, giddiness, headache, constipation, a pale or yellow face, tinnitus aurium, pains in the extremities, and some other light shades of incipient indisposition. In some, these were premonitory of a formal attack; in others, they vanished, leaving the subject in his usual health. Many were listless, complained of universal languor, and were indisposed to muscular exertion, yet did not surrender to the disease.¹

During the sickly period at Natchez, in 1819, “a general depression of health appeared to be caused. Those who were not absolutely diseased, laboured under some indisposition.”² In 1820, at New Orleans, there were few individuals among those who had not the disease, even among the creoles and assimilated foreigners, who did not experience symptoms which to others were premonitory—loss of appetite, dull heavy pain in the head, and sometimes in the back, occasional rigors, night-sweats, slight catarrh, giddiness, anxiety, low spirits.³ Many persons, doubtful of their insusceptibility, from previous attack, or residence in the city, “remained the greater part of the season under the frightful apprehension that the next day, or perhaps the next hour, would

¹ Potter on Contagion, p. 55.

² Perlee, Med. and Phys. Journ., iii. 12, 13.

³ Report of the Physico-Med. Soc. of New Orleans on the Epid. of 1820, p. 7.

bring on other symptoms of a less equivocal character, and confine them to their beds. It was under the feelings thus excited that many fled the city for safety.”¹

It is acknowledged by Pariset, Bally, and François, in their report on the fever of Barcelona in 1821, that the action of the cause was not limited, but bore on all the inhabitants; that hence it happened that individuals who had sufficient strength to attend to business and walk the streets or public squares, presented a pale or sallow countenance, injected eyes, an air of languor, and an uncertain gait not unlike that of persons labouring under a slight degree of intoxication (p. 27).

All these effects are very often experienced by individuals who have not approached within a very considerable distance of one labouring under the yellow fever—who have not entered a house where the disease exists. If, therefore, their dissemination among them is due to exposure to the action of a contagious poison formed in, and exhaling from the body of the sick, or of fomites impregnated with that poison, the circle of the morbid influence of the contagion of yellow fever must be remarkably extensive—much more so by far than that of the specific poison of any disease relative to the contagious character of which there is no doubt. But effects, so wide-spreading, so general, cannot be due to the influence of a contagious poison, requiring for its formation and diffusion the presence of the sick, and the sphere of action of which is confined, as already stated, within very narrow bounds. More likely is it, they are due to a general cause pervading the atmosphere of the infected district or city, and originating in these from the operation of telluric or other agencies appertaining to, or existing for the time being, in the sickly localities, and depending in no way for their manifestation on the presence of the sick, or on an imported contagion. These phenomena are analogous or approximate to those produced by the cause of non-contagious malarial fevers. No rationale can be offered of them, consistent with the *modus operandi* of any known contagion. May we not infer from this, that the poison which gives rise to them must be classed with the former, and not with the latter?

The Yellow Fever is often preceded, accompanied, or succeeded by certain Phenomena in the Vegetable or Animal Kingdoms.—The connection of yellow fever with diseases arising from peculiar insalubrious conditions of the atmosphere, either of a local or general character, and its exclusion in an etiological point of view from the class of contagious diseases, may further be inferred from the circumstance that when it spreads extensively, it is almost invariably preceded, accompanied, or followed by certain phenomena in the vegetable or animal kingdom. As relates to vegetables, we may remark that the coincidence of blight with pestilence has been recorded from ancient times—and the wide-spread potato disease which has now extended to almost every region of the globe, concurrently with the presence of influenza and cholera poison, may possibly be a modern instance of it. At all events, it

¹ Report of the Physico-Med. Soc. of New Orleans on the Epid. of 1820, p. 7.

is certain that seasons which are unusually sickly to large classes of human beings, are often alike unfavourable to the health and fruitfulness of many classes of plants.¹

Foderé, in speaking of the marshes of Bresse, in France, remarks that plants are there of a small and feeble growth, stunted in appearance, and endowed with a small degree of vital force; and Boudin, who refers to this fact, states that he has seen plants which were transplanted in marshy localities deteriorate rapidly, and return to their normal state as soon as they were removed beyond reach of the paludal influence.²

Dr. Caldwell, who has noted these points in relation to the yellow fever particularly, remarks that "the growth of vegetables is excessively luxuriant or defective, they are unusually sickly, especially in their fruits and seeds, and certain diminutive and parasitical plants appear in crops inordinately abundant. Hence, epidemic complaints, which are often attributed to the diseased vegetable productions of the year, are nothing but *concomitants* of those productions, arising from a common cause."³ And this cause is certainly not the effluvium arising from the bodies of the sick, the sphere of activity of which is, even in diseases most decidedly contagious, very circumscribed. Sure it is, as the late Dr. Chapman remarks, contemporaneously with the occurrence of yellow fever in 1793, "much sickness was observed among the trees of the city and neighbouring country, and particularly the peach tree. It continued during the whole period, a series of years, of the epidemic influence, so much so that we were nearly deprived of that fruit. The foliage of the tree became yellow, and it gradually pined away till it finally perished. On the cessation of this morbid agency, we had no difficulty in raising the trees, which now abound with the fruit of the finest quality."⁴

The epidemic of 1853, in Louisiana and other parts of our southern States, furnished evidence to the same effect. Mr. Lawrence, who is engaged largely in horticulture in the lower part of the city of New Orleans, informed Dr. Barton that, during that memorable visitation, his garden-seed would often fail to germinate, but still oftener, when they would sprout up a few inches from the soil, a sudden blight would seize them, and in a few days they would wilt and die. This was eminently the case with the cauliflower, the celery, the cabbage, radish, and other vegetables. To keep up his stock, he in vain applied to his neighbours, to those on the opposite side of the river, and down the coast. The same influence had been extended to them. These effects only continued during the epidemic. "In other parts of the country, similar effects were produced in the destruction of orchard fruit, and a blighting influence of various forms of vegetable life." At Biloxi, the peaches rotted on the trees. At Bayou Sara, the China trees had a sickly appearance, and their leaves were covered with a crustaceous larvæ. At Baton Rouge, "fruit of the peach full of worms, and potatoes rotted in the ground."

¹ Report on Quarantine, p. 14.

² Fièvres Interm., p. 198.

³ Caldwell, p. 133; Rush, iv. 56; Condie and Folwell, p. 13.

⁴ Laws of Epidemics, Med. and Phys. Journ., ix. 256.

In Donaldsonville, "peaches, figs, blackberries, &c., were more abundant and finer than usual; garden vegetables were also finer in the early part of the year; but, later in the summer and fall, could not be grown; gardens were almost a barren waste. At Lake Providence, Judge Selby found that the fig trees did not produce so many figs as usual, nor were those which did grow, of so good a quality as is usual."¹

On this point, I once more say that yellow fever allies itself to diseases originating from a general distemperature of the atmosphere—the effects of malarial influences—while it recedes from those of a contagious character; for the phenomena referred to, which, like others that have been or are to be mentioned, testify to "its connection with the constitution of the atmosphere, and therefore proclaim it a genuine epidemic," or endemic, are not associated with the prevalence of diseases of a truly contagious nature, and spreading from the sick to the well.

Still more striking is the effect of the insalubrious condition of the atmosphere on animals, both domestic and wild, previous to and during the course of the disease. In Philadelphia—and the same remark will be found applicable to other cities of the United States—epidemics of yellow fever have often been ushered in and accompanied by sickness among dogs, cats, and hogs. Dr. Caldwell, who calls attention to this fact, states that by the slaughterers of hogs for the Philadelphia market, in 1793, when the disease prevailed in that place, it was observed that the livers of those animals were unusually diseased (p. 133). The same writer informs us that, for some weeks previous to the occurrence of the fever in 1805, many dogs in the neighbourhood of the source of infection sickened, and several of them died. Their complaint was intestinal, as appeared from their symptoms while living, as well as from an examination of their bodies after death (p. 169). Early in June of that year, the cats in the neighbourhood of the infected district began to droop, and, in the course of the month, died in considerable numbers.² The epidemic of 1797 was immediately preceded by a very unusual and extraordinary mortality among the latter animals, of which it was computed that four or five thousand died.³ Dogs, also, were severely and fatally affected the same year, and at about the same time.⁴ The next year (1798), cats were again affected, as well as rats, of both of which numbers were carried off. The same remark had been made in New York, a year or two previously. After the commencement of that memorable epidemic, dogs also were affected. Of the disease among cats we are told that, in general, the animal was without appetite, but thirsty, and, at first, much inclined to sleep; of a dull, melancholy turn, and soon began to look weak and thin. Many died in a numb and torpid state, while others, in the last stage of the disease, were seized with a delirium, puking, and an uncommon flow of frothy saliva from the mouth. A moaning or groaning noise was also very common.⁵ While

¹ Report of Sanitary Commission of New Orleans for 1853, pp. 31, 53, 265, 266.

² Fever of 1805, p. 61.

³ Condie and Folwell, p. 15.

⁴ Rush, iv. 8.

⁵ Condie and Folwell, p. 14.

such is the case with domestic animals, wild quadrupeds, undomesticated birds, and even fish and oysters, are known to participate at times in the calamity. In 1798, flies were found dead in great numbers in the unhealthy parts of the city.¹

In other cities and countries; the same remarks have been made relative to the affections of the lower animals during the prevalence of yellow fever. At Pensacola, in June, 1823, there was a fatal distemper among brute animals, the character of which was most observed among dogs, foxes, and panthers, particularly the last two, numbers being found everywhere dead in the woods—four, five, and six bodies being generally discovered collected about the same spot.² At and about New Orleans, in 1853, a similar destructive influence on the lower animals was found to be exercised. Birds and beasts, says Dr. Barton, have been driven from their usual haunts into the deepest recesses of the forests, showing by their instincts that they were sensible of some malign properties existing in the bosom of that atmosphere whence they derive their main vital influence. At Lake Providence, it was noticed that the feathered tribe almost entirely disappeared during the prevalence of the epidemic. Fowls, old and young, in some places, died often without previously appearing sick. At Gainesville, it was found that native cows died in great numbers, without obvious cause.³ Dr. Smith says that at Gibraltar, in 1828, in addition to the dogs and monkeys, a goatherd lost a great part of his flock, and almost the whole ceased to give milk. Another person lost several birds and monkeys. Three pointer dogs were also taken ill with shiverings and great debility, and they were constantly vomiting—at first a kind of bilious matter, afterwards a very black matter; the eyes and ears were quite yellow. Another lost eight game dogs within a few days of each other; all were yellow, and vomited. And, in the course of the epidemic, upwards of twenty parrots were sent to Mr. Frazer, of the civil hospital, from inhabitants, to be dissected.⁴ In New Orleans, in 1833, there was much sickness among horses, cattle, and swine, in the country; an abundance of flies and mosquitoes preceding the epidemic. The latter continued throughout the season.⁵ In the same city, in 1819, we are told that the cattle died: horses, oxen, and cows, with rotten tongue; sheep and hogs with their hoofs dropping off; and calves with rotten ears.⁶ Speaking of the epidemic of Barbadoes in 1816, Ralph remarks: "This season was noticed, by the oldest inhabitants, to be uncommonly hot and peculiar; and it is deserving of remark that a fatal distemper prevailed amongst the dogs, destroying them in great numbers."⁷

In a word, the observation has often been made, in this country, that epidemics of malignant fever were ushered in and accompanied by sickness

¹ Caldwell, p. 134; Rush, iv. 56.

² McMahon's Report, Sickness, &c., of U. S. Army, p. 36.

³ Report of Sanitary Commission (1853), pp. 38, 53, 265-6.

⁴ Edinb. Med. and Surg. Journ., xxxv. 36.

⁵ Barton, pp. 6, 7.

⁶ Cyclop., ii. 74.

⁷ Edinb. Med.-Chir. Trans., ii. 58.

among the above named and other animals, the symptoms being often analogous to those of the reigning disease.¹

The fever, on this point, again, approximates to malarial fevers, or fevers having an origin in a general morbid condition of the atmosphere. Dr. Rush refers to Cleghorn, who describes a diseased state of the liver in cattle, in an unhealthy part of the island of Minorca; and to Dr. Grainger, who takes notice of several morbid appearances in the livers of domestic animals in Holland, in the year 1743. "But the United States," he adds, "have furnished facts to illustrate the truth of this remark. Mr. James Wardrobe, near Richmond, in Virginia, informed me that in August, 1794, at a time when bilious fevers were prevalent in his neighbourhood, his cattle were seized with a disease which I said formerly is known by the name of yellow water, and which appears to be a true yellow fever. They were attacked with a staggering. Their eyes were muddy and ferocious. A costiveness attended in all cases. It killed in two days. Fifty-two of his cattle perished by it. Upon opening the bodies of several of them, he found the liver swollen and ulcerated. The blood was dissolved in the veins. Similar appearances were observed in the livers of sheep, in the neighbourhood of Cadiz, in the year 1799, during the prevalence of the yellow fever in that city" (iv. 161-2).

M. Dupuy has seen a number of oxen perish with symptoms perfectly analogous to those of intermittent fever, after having pastured in a highly marshy locality. In 1826, after the overflow of the river Manse, an intermittent epidemic broke out among horses, and occasioned a considerable mortality. The same able veterinary surgeon remarked, at a meeting of the Academy of Medicine, as a fact well known to all veterinary surgeons, that sheep imbibe water and humidity with the greatest facility; indeed, that they are in some sort hygrometric. "Well," he continued, "whatever be the quantity of humidity they may imbibe, they remain free from intermittent fever; but expose them to marsh effluvia, and they will soon be attacked."² Lancisi relates that, in 1713, during the prevalence of intermittent fevers, an epizooty carried off thirty thousand oxen.³ It should be added, also, that marshes occasion, among many animals, "a chronic endemy, perfectly analogous to that produced in the human species. The prolonged sojourn and pasturage of sheep in marshy localities produce in them the hydroæmia, a disease characterized by a diminution in the proportion of the blood-globules, as also by a notable one in the quantity of the serum, and which, conse-

¹ Med. Reposit., i. 250, 254-5, 351; Smith (E. H.), Fever of New York, p. 76; Condie and Folwell, p. 15; Shecut, p. 77; Vaughan, p. 18; Cartwright, Recorder, ix. 7; Baxter's Med. Reposit., xxi. 6, 7; Chapman, Med. and Phys. Journ., ix. 395-6; Kilpatrick, N. O. Journ., ii. 43; Seaman, Webster's Collection, p. 3; Thouvenel, iv. 200-2; Smith, Edinb. Journ., xxxv. 26; Maclean on Epid., i. 289; Sir J. Fellowes, pp. 45, 236; Cycl. of Pract. Med., ii. 74; Pariset, pp. 67-69; Osborn, p. 70; Pinckard, i. 138; Desportes, i. 17; Arejula, pp. 286, 331; O'Halloran, p. 26; Report on Quarantine (London), p. 13; Second Report, pp. 39, 365; Moreau de Jonnes, p. 112; King's Report on Fever of Boa Vista, p. 9; Poir, p. 63.

² Bulletin de l'Académie de Médecine, x. 1065.

³ Lancisi, De Boville Peste, p. 2, &c.

quently, is analogous to the paludal cachexia of men." M. Gasparin reproduced this disease in sheep by causing them to drink and to be rubbed with the condensed vapour obtained from marshy surfaces.¹

Mr. Chadwick informs us that in the course of the inquiries as to what has been the effect of drainage upon health, one frequent piece of information received has been that the rural population has not observed the effects on their own health, but they have marked the effects of drainage on the health and improvement of the stock. Thus, the less frequent losses of stock from epidemics are beginning to be perceived as accompanying the benefits of drainage, in addition to those of increased vegetable production.²

The General Board of Health of London, in their report on the practical application of sewer-water and town manures, already referred to, remark that the injurious effects upon health, of the prolonged retention of excessive moisture on a surface of vegetable mould, is established by the production of rot amongst sheep—an effect which sheep-feeders have produced by stocking closes just after they had been flooded, and whilst they were saturated with moisture.³

In his examination before the Metropolitan Sanitary Commissioners, already referred to in a former chapter, Mr. Smith, after speaking of the beneficial effects of draining on the healthiness of malarious localities, remarks that it is generally observed by the inhabitants that their cattle or stock are now less subject to diseases. In a farm in the west of Perthshire, the cattle were very subject to the disease called "red water;" since the draining, there has been no case of that disease. In other parts of Scotland and England, similar results are stated to have followed the introduction of thorough drainage.

Mr. Parker stated that the disease of footbalt in sheep and deer has been perfectly removed in many gentlemen's parks, and in extensive pasturage grounds, by deep under drainage.

"In the Highlands," Mr. Spooner remarked, "and more particularly on the west coast, there exists a well-known and fatal disease among sheep, incurable by any treatment, termed 'braxy,' which on undrained land and in wet seasons is a cause of very serious losses. This is, in a great measure, prevented by drainage, and the diminution of casualties alone is more than sufficient to cover its cost, independently of the increased quantity and better quality of the fodder produced. This system has been extensively practised for several years, and invariably with the same beneficial results. As to the health of cattle or stock," he added, "I have the strongest evidence of the beneficial effects of drainage in many instances. On the lands which I possess, and on several others in the district, a disease called 'red water' prevailed, in some years proving very fatal; but after drainage and cultivation of the marshy parts of the pasturage, the stock has been free of that disease. The surface drainage of sheepwalks in every district is well known to promote the healthiness of the stock, and I believe the thorough drainage of a single swamp in

¹ Becquerel, *Hygiène*, pp. 183, 194, 195.

² Second Rep. of Commissioners of Inq. into the State of Large Towns.

³ Minutes of Information on the Practical Application, &c., p. 9, London, 1852.

any locality will be an important means of improving the health, both of the population and stock connected with it."¹

It is stated, in the report of the Sanitary Commissioners, that, contemporaneously with the change noted in the character of fever in the London Fever Hospital, an analogous change has been observed, by the professors of veterinary medicine and surgery, in the diseases of dogs, horses, sheep, and cattle—namely, a change from an inflammatory type to one of debility—and that this has been observed to be particularly the case with regard to all the diseases properly considered epidemic to which these animals are subject.²

In further corroboration of this, it may be remarked that it has been found in this country that those animals that feed in marshes where periodic fevers prevail have diseased viscera. We are told, by Dr. Ludlow, that in the town of Walcott, Seneca County, N. Y., where marshes and lowlands abound, the hogs, when killed, are usually found to have eroded livers.³

Writers on the yellow fever have also mentioned the occurrence, during some epidemics, of a marked predominance of insect life.⁴ In the summer of 1798, one of the most calamitous epidemic years that Philadelphia experienced, the country around it was unusually infested by grasshoppers and caterpillars. In a lower degree, the same thing was true of the summer of 1802; and in 1797 the mosquitoes within the city were almost as annoying as the disease itself. Nor was there anything peculiar in the sensible qualities of the atmosphere to aid us in accounting for these phenomena. Yet, within the recollection of the oldest inhabitants of the place, such a superabundance of those insects had not previously occurred.⁵ During the great epidemic of 1853, in our southern States, a similar effect was observed. In Natchez and Clinton, mosquitoes were very numerous. At Biloxi, Centreville, and Donaldsonville, these insects, as well as flies, abounded more than usual. At Black River, Concordia Parish (La.), there was an unusual quantity of house flies early in the spring, which continued till summer ended, but mosquitoes were not as troublesome as usual.⁶

Many of these phenomena must doubtless be ascribed to a general modification of the atmosphere favourable to the spread of not only endemic, but of contagious disease; but some may with reason be ascribed to the local insalubrious agent giving rise to the fever; while in no instance can they be referred to the diffusion of a contagious effluvium. I am perfectly ready to admit that the lower animals are apt to be attacked by human diseases of a contagious character; but such cases are of rare occurrence, and may always be traced to direct exposure to the sick; while, in the instances cited, the effects, besides occurring often before the manifestation of the disease in the

¹ Drainage of the Land forming the Sites of Towns, pp. 69, 70. London, 1852.

² Report on Quarantine, p. 13.

³ New York Med. and Phys. Journ., ii. 88.

⁴ Rush, iv. 8; Caldwell, p. 134; Condie and Folwell, p. 13.

⁵ Medical Repository, x. 196; Seaman, Webster's Collection, p. 3; Desportes, i. 17; Vaughan, p. 18; Shecut, p. 99; O'Halloran, p. 18; Kilpatrick, N. O. Journ., ii. 43; Osborn, p. 70; E. H. Smith, Webster's Collection, p. 76.

⁶ Report of Sanitary Commission, pp. 39, 265-6.

human species, were too widely spread to be attributed to such exposure. Nor is it less true that epizootics are often noticed in places where the yellow fever never appears, and other localities, differently situated, are not always followed by or attended with the manifestation of the disease.¹ This only proves, however, that the efficient cause of the fever is not essential to the production of the effect in question, and that so long as that efficient cause is not evolved, those effects may exist alone. But whether this be so or not in many instances, it does not the less follow that in some the cause of the fever exercises a baneful influence over all living objects subjected to its influence, and that in other cases the frequent, if not invariable, occurrence of such effects in times of yellow fever epidemics, indicates a strong dependence of the latter on the insalubrious condition of the atmosphere giving rise to the former. It may, if combined with other facts, be used as an argument to corroborate the idea of the domestic origin of the disease, and speaks volumes against its importation from abroad, as well as against its contagiousness.

The system becomes inured to the Effect of the Efficient Cause.—It is certainly a privilege of the living system to become inured, to a greater or less extent, to the action of poisonous agents. In accordance with this, we find that individuals exposed continuously or habitually to the influence of most of the causes of zymotic diseases are less subject to suffer from them than those who suddenly come within the sphere of their operation. To a certain degree, this holds good with respect to a few contagious complaints. With others, and those the larger number, the susceptibility is never lost; while on the contagiousness of some in which the phenomenon is more evident, considerable doubt exists in the mind of many able and experienced observers. Hence, the power of acclimatization does not extend to the eruptive contagious febrile diseases. There is none for smallpox; none for scarlet fever; none for measles. The system must have gone through an attack of these diseases before it can be secure from the morbid influence of their respective poisons.

All this contrasts strongly with what takes place in the yellow fever; for in this, as we have seen, the power of acclimatization is exhibited in a remarkable manner. Let it not be said that the facts mentioned in a former chapter respecting the loss of susceptibility to the action of the typhus and typhoid fever poison by continued residence, with or without nativity, in localities where they exercise habitually their influence, militates against any argument founded on the contrast in question; in other words, that although the loss of susceptibility from acclimatization is fully exhibited in yellow fever, and not in many contagious diseases, the difference in that respect must not be adduced in evidence of the want of analogy—pathological and etiological—between the former and contagious complaints, inasmuch as that power extends to typhus and typhoid, and perhaps other febrile contagions; for the contagious qualities of some of these is still matter of doubt, and the loss of susceptibility in the most favoured among them, is after all

¹ Condie and Folwell, p. 15.

confined within very narrow bounds. Besides, the objection thus raised might be met by the fact, that the system is but feebly habituated to the action of the efficient causes of some forms of malarial fever, the non-contagious character of which is now admitted on all hands. But, however this may be—however true, that the argument works both ways, and can therefore help us but little in the settlement of the question under consideration; I cannot help thinking that we may adduce as an additional support of the opinion of the domestic origin of the disease, and of its independence of a contagious poison, the long known fact that localities where the fever appears so often that it may justly be regarded as endemic, and where it seldom or never attacks the natives or acclimatized, remain free from its inroads, unless by the arrival of strangers, in large numbers, materials are afforded for the action of the poison, though such strangers come from places where the fever has never existed, or does not exist at the time, and are not therefore in a condition to introduce it. This circumstance is often noticed in the West Indies and other tropical localities; and would, doubtless, be seen in our southern yellow fever cities, were the complete absence of unacclimatized strangers at any time within the reach of possibility—as well indeed as in our more northern, or in European climates, were their inhabitants, under any circumstances short of the protection afforded by an attack, exempt from the influence of the infection. If a few arrive at the time of the evolution of the poison, the disease will prevail sporadically among them—if many, it will assume an epidemic garb.

Such was the case at Dominica in 1793, when the streets were crowded by people who had escaped from Martinique to the number of some three or four thousand, and had been brought over in small vessels and in a perfect state of destitution.¹ Hence, violent epidemics have often occurred on the arrival of troops or fleets, or a large number of merchant vessels, by which an influx of unacclimatized persons was occasioned at an unpropitious moment. Writing early in the present century, Humboldt remarked that, for some fifty years before, the vomito scarcely manifested itself on any point of the coast of the Pacific, with the exception of the city of Panama. In that port, as at Callao (*Leblond*, p. 204), “the commencement of epidemics is most usually marked by the arrival of vessels from Chili; not that this last country—one of the finest and healthiest in the world—can transmit a disease which does not exist within its limits; but its inhabitants, when removed to the torrid zone, experience as powerfully as those of the North, the deleterious effects of an atmosphere which is not only excessively hot, but vitiated by putrid exhalations” (p. 761). The epidemic of 1794, at Vera Cruz, broke out soon after the arrival of three men-of-war, which contained a large number of unacclimatized young sailors (p. 783). That of Martinique of 1686, coincided with the arrival of the *Oriflamme* and other vessels from the coast of Siam.

From these facts we reach, as a matter of course, the conclusion, that the cause existed in those localities, and was not imported there; and that all it needed was fit subjects to be acted upon. I need scarcely remark that the arrival of crowds of susceptible strangers is not necessarily followed by the

¹ James Clark, *Fever of Dominica in 1793*, p. 1, &c.

breaking out of the fever in the epidemic form; and hence we may infer that the efficient cause is not always present; for, as already seen, several years often intervene between the manifestation of such epidemics, although unacclimatized strangers in large numbers may arrive during those periods of exemption. This was the case, as we learn from Humboldt (pp. 783, 784), at Vera Cruz during a period of eighteen years. The island of Guadaloupe was once more taken possession of by France on the 10th of December, 1814. A considerable amount of troops for that purpose arrived. No fever then existed in the island, and notwithstanding the large influx of strangers, none appeared till the middle part of 1816. The same thing occurred at the same time in Martinique.¹

From 1781 to 1783 the city of the Cape (St. Domingo), was crowded with French and Spanish troops and sailors, to the amount of 50,000. Few, if any of these had lost in any way, their susceptibility to the yellow fever. The disease, nevertheless, did not make its appearance.² During the above-mentioned periods of exemption, vessels from infected localities did not fail to arrive, without, of course, being prevented by quarantine regulations.³ Dr. Imray is very justly of opinion that the causes of the epidemic of Dominica in 1841 could not be said to have continued to exist in the island since the preceding visitation in 1838, and only ceased to act because there was no material on which the poison could exert its baneful influence; for the European troops were more than once changed, yet not a single instance of yellow fever occurred either in the garrison among the civilian inhabitants, or the shipping, from 1838 to June, 1841. "Moreover," as he remarks, "were this the case, Europeans, if susceptible, would invariably be attacked as they arrive in the West Indies. Now, we find that in some colonies, a long period of years passes on, and no case of the disease is met with, while the influx of strangers is going on. But, so soon as the causes become developed, then do the natives of cold climates suffer; and if others arrive while the epidemic influence is in operation, the risk of attack is very great."

Let it be remarked in addition that, in the West Indies and elsewhere, epidemics have not unfrequently ceased from the want of a further supply of susceptible subjects, as proved by the fact of the reappearance of the disease on any arrival of strangers, or a too early return of individuals who had sought refuge in some salubrious spot. Thus, Dr. James Clark remarks that the fever of Dominica, in 1793, became less violent in October, and ceased altogether in the beginning of November. This "was supposed to proceed from the comparative coolness of the weather, but the arrival of some American vessels, about six weeks after, convinced us that this short respite was more owing to the want of proper subjects for the vitiated atmosphere to act upon, than to the change of its temperature; for, in a short time all on board, who had not been in the West Indies before, were seized with it," and many

¹ Chervin, *Gaz. des Hôpitaux*, 1839.

² Vincent, *Diss. sur la Fièvre Jaune*, pp. 8, 9.

³ *Edinb. Med. and Surg. Journ.*, lxiv. 331.

died.¹ In like manner, Dr. Imray, already cited, states as a fact perfectly true, "that yellow fever occasionally disappears in consequence of the absence of predisposed individuals, while the causes linger in the country, and only manifest themselves on the arrival of Europeans, or of others who have become highly susceptible."²

Now, in all that precedes, we find nothing calculated to bear us out in the idea that the fever under consideration is endowed with contagious and transportable properties. If the disease often breaks out, or shows itself in yellow fever localities, on the arrival of unprotected or susceptible persons, it happens not unfrequently that, notwithstanding the influx of such individuals in very large numbers, no such results follow. From this we are to infer that something more than accumulation of susceptible strangers in a fever locality is required to produce the disease; and that the morbid agent to which the disease is due, is not an imported contagion, is demonstrated by the fact that at the very time that strangers thus accumulated remain free from the disease, vessels from infected places arrive without producing evil consequences, while in other instances, the disease breaks out among them without the instrumentality of such arrivals. Nor should it be forgotten, that in the list of contagious diseases, it is impossible to point out one that would comport itself in the same way under the circumstances mentioned. Where are we to find the contagious poison that could leave a whole community untouched and manifest itself by seizing strangers who arrive in the place in good health, and generally from other regions where it does not exist, or has never existed? Or, where is the disease which, if contagious, and capable of starting up in a place on the arrival of such strangers, would fail to do so when the latter are accumulated in large numbers—in a sickly state, too—especially when, at the same time, vessels from ports where that disease exists or have the latter on board, arrive daily, and are permitted to mix freely with others filled with susceptible individuals? Where are we to find the contagious disease which, after having altogether ceased its epidemic career and left no traces behind, attacks, six weeks after, strangers who happen to arrive in the place so visited, and leave thousands who have never had it before and are merely acclimatized to the place unattacked?

Yellow Fever local in its habitation.—Unlike contagious diseases generally, and similar to endemic fevers, though in a more marked degree, the yellow fever is local in its habitation. The first, when introduced into a place, extend in all directions, and with more or less rapidity diffuse themselves in any part of such places where the contagion may be carried, even at considerable distances from the original cases, each case which occurs there acting as a new source of contamination, and serving to widen the sphere of the epidemic. Endemic autumnal fevers, especially when assuming the epidemic form, while they retain their so universally recognized non-contagious character, pervade a wide extent of country—differing, however, from the former in this, that though spreading thus widely, they nevertheless do so within certain bounds and in certain directions only, and sometimes

¹ Fever of Dominica in 1793, pp. 4, 5.

² Ibid., p. 331.

spare, more or less completely, localities in even close proximity to others severely infected.

Now, if we examine what takes place in reference to the yellow fever, we discover that the sphere of its prevalence is always somewhat, and on some occasions very, circumscribed; the disease remaining confined within the limits of the localities where it originated, or extending at only a comparatively small distance from these. In Philadelphia, the disease has, during its various visitations, exhibited in a most striking manner this inability to diffuse itself afar. In 1699, the fever which commenced at a wharf between Market Street and the drawbridge, extended over the infant city, but does not appear to have gone much, if at all, beyond its precincts.¹ In 1747, it was mostly confined within the limits of the southern parts of the city below the drawbridge, and in the neighbourhood of the dock, which was then uncovered.² As we have already seen, the fever in 1762 was mostly circumscribed between Pine Street northerly, and three or four squares southerly, and extended from Front or Water Street to Third or Fourth in a westward direction. In the memorable epidemic of 1793, the disease, after commencing in Water Street, between Mulberry and Sassafras, extended northwardly up Water to Vine, and the streets running east and west, and several running in a contrary direction; and finally in most part of the city, which was then of limited extent, as also in the suburb of Southwark, and in Kensington, at that time a separate village. Beyond these localities it did not spread.

The next year its limits were more circumscribed, and were embraced within Market and Walnut in one direction, and Water and a few streets to the westward in the other. In 1797, the infection was chiefly confined to the district of Southwark, and to the village of Kensington. Some cases, it is true, appeared in the city proper; but most of these were easily traced to the above sources. The disease, in 1798, spread much more extensively than it had done in preceding sickly periods—even perhaps than in 1793—every part of the city, as well as Southwark and Kensington, becoming, before a long time had elapsed, to a greater or less extent the seat of the calamity. But, as in other seasons, the disease remained circumscribed within the above limits, to which all cases that occurred beyond could be traced. In 1799, the epidemic spread from Penn Street on the wharf, to almost every part of the city east of Seventh Street, beyond which very few cases occurred; and between Pine and Lombard Streets, near the southern boundaries of the city, and the district of Southwark, near the Swedes' Church. In 1802, it extended from the corner of Vine and Front to other parts of the city, particularly in Front and Water Streets, near the drawbridge. The next year it prevailed in Chestnut near Water Street, in Water near Sassafras, and in the lower part of the city near South; but, with few exceptions, its range did not extend to the westward of Second Street, and in many parts not even that far. In 1803 it prevailed in Water Street, scarcely reaching Second in a

¹ See ante, vol. i. p. 53, Additional Facts, by Col. of Phys., p. 5.

² See ante, vol. i. p. 60; Additional Facts, by the College of Physicians, p. 6.

form deserving the name of yellow fever. In Water Street, it occurred at various points embraced within the space of a mile.¹ Breaking out, in 1805, in the district of Southwark, between the New Market and the Swedes' Church, the disease extended in various directions. But the principal focus of the infection was circumscribed within the limits of Southwark, and a small section of the southern extremity of the city proper east of Second Street. In Southwark, the disease extended to the east side of Fourth Street. In 1819, it was strictly limited to two points—the north side of Market Street wharf, and Front Street above Walnut; no other cases occurring in any part of the city. In 1820, the fever, though appearing in several separate localities parallel with the river Delaware, but more or less distant from it—Water Street near Sassafras, on the wharf near Walnut, in Water Street between Market and Mulberry, in Front between Walnut and Chestnut, in Letitia Court, in Second Street near Shippen, and finally in the Northern Liberties—did not reach to the east side of Second Street. In 1853, it broke out at the corner of South Street and the wharf, and continued during the season to prevail in that neighbourhood, occupying an area of two or three squares in a northern and southern direction, and a third of that distance westwardly. The fever also broke out, but prevailed to a very limited extent, in the Northern Liberties, leaving the intermediate space, and all north of Spruce, free and healthy.

That cases occurred beyond the limits ascribed to the disease, during the several epidemics mentioned, is certainly true; but in the very large majority of these, if not in all, they were in persons who had imbibed the seeds of the infection in the sickly districts.

Nor is it in Philadelphia alone that the yellow fever is found to be thus circumscribed within a certain area. Turn we to New York, we find that in 1791 the disease was limited to Peck Slip and the neighbouring streets—principally Water Street—whence it spread over some portions of the then small city.² In 1795, it did not advance beyond the wharves and vicinity.³ In 1798, New Slip was the locality most affected.⁴ Two years after, it was limited to Pearl and Water Streets, and the adjoining slips.⁵ In 1803, the first alarm arose about the Coffee House Slip, and in that neighbourhood. The fever prevailed about the same time in some other parts of the city, but principally near the margins of the two rivers.⁶ In 1805, it was at first limited to the eastern side of the city, in Front, Water, and Pearl Streets—principally below Burling Slip. After a short time, however, it broke out near the North River, and prevailed mostly on the low ground situated on the margin of the two streams.⁷ But the area of the infected district did not

¹ Caldwell, *Med. Repos.*, vii. 149.

² Addoms, *Dissertat. Inaug.*, p. 7.

³ Bayley, pp. 60–90; Seaman, in Webster's Col., p. 5; E. H. Smith, *ib.*, p. 67.

⁴ McKnight, *Med. Reg.*, iii. 295; Hardie, *Fev. of 1798*, p. 28; *Rept. on Quarantine Laws*, p. 8.

⁵ Seaman, *Med. Repos.*, iv. 250.

⁶ Miller (E.), *Works*; Hardin on *Fev. of 1822*, p. 13; *Ib.*, *Doc. relative to the Board of Health*, p. 36.

⁷ Miller, *Report in Document relative to Board of Health*, p. 46, and *Works*, p. 89; Rodgers, *Letter to Board of Health, in same doc.*, p. 25.

exceed two hundred yards from one extremity to the other. The fever of 1819 was confined within narrow limits, the infected district being described by the Board of Health as beginning at the foot of pier No. 8, East River; thence running on the eastern side of the same pier, and the adjoining slip to the corner of Pearl Street; thence up Pearl to the west side of Wall Street; thence down Wall Street to the East River—thus including little more than the old slip and the parts adjacent.¹

The epidemic of 1822, the most severe New York has suffered for some years, was scarcely less limited as regards the extent of the infected district. Commencing in July, it was found that as late as October only six cases had occurred which could not be distinctly traced to the west and south of Fulton Street; and only two to that part of the city inclosed within the bounds of Catharine, Banker, and Pike Streets, and the river.² And at the close of the epidemic it was ascertained that from Rector Street, where the disease commenced, it gradually spread pretty equally in every direction; but did not extend beyond Fulton Street, which runs parallel with the other at a distance of eighteen hundred feet.³ In the language of Dr. Townsend, this was the *ultima thule*, or extreme boundary of the town's infected district.⁴ This writer states that there were in all but 55 cases, which occurred without these limits; all but twenty-two were traced to the infected district, and of these there are many reasons to think they had actually received the disease there (p. 113). The disease occurred also in the parallel streets—Cheapside, Lombardy, and Banker—situated at a distance of about three quarters of a mile from Fulton Street, and extending from Catharine Street in a northern direction to the distance of three streets, or about eight hundred or a thousand feet. But it made comparatively little progress in that region, and was, as shown, still more circumscribed in extent than in the lower district.

All the epidemics of Baltimore since 1793 have prevailed principally at Fell's Point—a kind of suburb on the margin of the water.⁵ In 1819,⁶ some cases occurred at Smith's Dock, but the main force of the infection was limited to the Point, while four-fifths of the city remained as healthy as usual. The fever which prevailed in Boston in 1798 did not extend beyond the town dock, State Street, Liberty Square, around Fort Hill, and the surrounding parts. In 1802, it broke out again in the vicinity of Fort Hill, but its sphere of action was even more circumscribed than it had been on the former occasion; and in 1819 it prevailed in the same district, but was confined to a still narrower area, the disease appearing mostly within one small square on the southeast side of the Hill. Nor did it at first extend over the whole

¹ Watts, pp. 302, 350; Med. Rep., N. S., v. 240.

² Proceedings of the Board of Health, p. 119.

³ Ibid., p. 196.

⁴ Fev. of 1822, p. 111.

⁵ Davidge, p. 66; Med. Rep., ii. 83-4; Report of Board of Health in Med. Repos., iv. 205.

⁶ Letters, &c., pp. 15, 34; Revere, Med. Recorder, iii. 237-8; Reese, Observations, &c., p. 24.

of this square, the longest side of which is stated to be about twenty-five rods in length; and, though it gradually spread over a larger district, it did not leave the circle of Fort Hill.¹

In Providence, during the three epidemics of 1797, 1800, and 1805, the fever was confined to the south part of Water Street, and the lanes and alleys immediately adjacent, and covered a surface of about one hundred rods in length.² In New London, in 1798, it was far from prevailing in all parts of the town, which, at that time, was of small extent.³ In Wilmington (Del.), during the visitation of 1802, it was confined to the district southeast of Market and Third Streets.⁴ That the area over which the yellow fever extends in New Orleans, Charleston, Natchez, and some other places, particularly in small towns, is comparatively larger than in those mentioned, may be true; for in these the disease spreads often over the whole surface. In New Orleans, it not unfrequently happens that the disease attacks not only the several municipalities into which the city is or was divided, but extends to the adjoining village of Lafayette; and in Charleston, Natchez, &c., during violent epidemics, few parts can present entire security from infection. But however this may be in some cases, in others the fever is circumscribed within narrower bounds. At any rate, in no instance has it been known to extend beyond the outskirts. Such is the case in New Orleans, where the efficient cause of the fever is invariably confined to the city.⁵ From the days of Lining, we know that in Charleston strangers run no risk unless they reside within the limits of the city. Country people, he says, had it when they came to town, while those who remained in the country escaped it (ii. 407). So well is this known, that Dr. Ramsay⁶ calls the fever a disease of cities; and a more modern writer has hazarded the opinion—perhaps incautiously—that a city atmosphere is necessary to generate the poison.⁷ In 1803, at Alexandria, the limits of the infection were so well defined that Dr. Dick could, when called to a patient in the other parts of the city, venture a decision from the symptoms alone, whether the subject had been occasionally exposed to the infected atmosphere in the sickly district.⁸ Equally restricted were the boundaries of the fever in Norfolk in 1821.⁹

The disease, in Barcelona, remained confined within the city and the adjoining village or port of Barcelonette.¹⁰ It did not extend beyond the ditch which surrounds the former, nor indeed approach it; for the greater part of the town remained entirely exempt.¹¹ Cadiz, Seville, Malaga, and other cities of Spain,¹² presented a like limitation. The same observation was made in

¹ New Eng. Journ., viii. 380; N. Am. Rev., x. 396.

² Med. Rep., x. xi.

³ Holt, Rep., iii. 292.

⁴ Vaughan, p. 15.

⁵ Chabert, p. 19; Harrison, p. 131.

⁶ Medical Repos., iv. 100.

⁷ Simons, Report to Board of Health, p. 22.

⁸ Med. Repos., vii. 191.

⁹ Archer, Med. Recorder, v. 63.

¹⁰ O'Halloran, p. 35; Rayer, p. 42; Chervin, Rapport de l'Acad., p. 37; Ib., Examen, &c., en Mat. Sanit., pp. 26-7; Ib., Fièvre Jaune d'Espagne, p. 173; Maclean, Manifest, &c., in Evils of Quarantine, pp. 133, 156.

¹¹ Chervin, Réponse à M. Guyon, p. 237; Bancroft, pp. 305, 309, 319.

¹² Edinb. Med. and Surg. Journ., xxxv. 272.

Gibraltar, as also in Leghorn, in 1804.¹ And those who have perused the many accounts we have of the yellow fever of tropical climates, need not be told that there, as elsewhere, the disease is usually confined to the cities or villages; that in many instances the infected districts are as circumscribed there as they are found to be in our latitudes;² and that at other times the disease occupies small localities, which demonstrate clearly the limited extent of its diffusion, and, as a consequence, its tendency to affect certain localities to the exclusion of all others. Many other facts and references of same import might be easily collected, but the preceding will suffice, and will justify the remark of Dr. Baneroff relative to marsh fever and its applicability to the disease under consideration, that the limitation of the epidemic influence to a city, or part of it, and the exemption of every other part of it, is an important fact which often occurs, “and well deserves to be remembered, not only as a distinguishing mark of these fevers, but also as an incontrovertible evidence of their total want of any contagious property; for contagious fevers are not thus narrowly confined and limited in their progress” (p. 294).

CHAPTER XVIII.

PROOFS OF NON-CONTAGION—CONTINUED.

The area of the sickly locality is sometimes very circumscribed.—It has often been found that the disease affects in preference, or even exclusively, certain very circumscribed localities—a particular building, a particular side of the street, or a particular part of a house—although the effect cannot be ascribed to the communication of the disease from the sick to the healthy, or its introduction from without. During the early part of the epidemic of Martinique, in 1838, the military hospital—the position of which is insalubrious—became suddenly a focus of infection. The wounded, the syphilitic, and individuals labouring under various maladies, though having no communication with yellow fever cases, were attacked while confined in their beds with that disease. This continued six weeks, during which the neighbouring houses remained perfectly free. The same manifestation of infection was noticed in other buildings, and especially in the convent of the Ladies of St. Joseph; all of which were situated at a considerable distance from the hospital. The same circumscribed localization was observed among the shipping in the port. In these the disease, when once appearing, attacked in rapid succession several individuals; but, instead of affecting the vessel moored in the immediate vicinity of the one first visited, it jumped over several, and committed its ravages in

¹ Palloni, p. 33.

² Rufz, p. 62; Vatable, pp. 344, 357; Dutroulan, pp. 18, 62; Ralph, p. 57.

others distantly situated (*Rufz*, p. 28). In Dominica, in 1841, the Government House afforded an instance of the same kind; scarcely an European slept there that escaped an attack.¹ It is recorded that during the epidemic which prevailed in Barbadoes in 1838, of thirty-six individuals residing in a building appropriated as quarters for the officers, twenty-eight were attacked, of whom ten died; while of the whole regiment residing in the soldiers' barracks, at so short a distance as fifty paces from this fatal spot, only thirty cases occurred, and none died.² On other occasions, it was observed that, in certain barracks and hospitals, the very diagonal of particular apartments afforded an accurate demarcation of the safe and unsafe portion of bed. It has been remarked by Dr. John Hunter and others, that houses raised on pillars or arches, or in any other manner, are freer from the fever than those upon the ground. How much a circumstance of the kind may, he says, be productive of the effect, was seen in the barracks of Spanish Town. "They consisted of two floors, the first upon the ground, and the second over the first. The difference in the health of the men on the two floors was so striking as to engage the attention of the Assembly of the island, and, upon investigation, it appeared that three were taken ill on the ground floor for one on the other."³ This greater preference manifested by the disease for lower floors, especially at the commencement of an epidemic, and sometimes throughout its whole course, has been pointed out by Dr. Gillkrest and other writers.⁴

Dr. Ferguson says that, so limited often is the influence of this fever, that one story of a house will be strongly affected by it, while all other parts of the same tenement remain healthy.⁵ Mr. Callow, surgeon of the 84th regiment at Fort Augusta, Jamaica, in his official report for 1827, referred to by Dr. Gillkrest, refers to the disease being confined to a particular extremity of a building (p. 280). It is observed also by M. Ralph, that a remarkable difference in the respective salubrity of the two floors was discovered in the barracks of Barbadoes. The soldiers who slept upon the basement floor were seized in proportionally greater numbers than those who occupied the upper. "Indeed," adds Mr. R., "in situations which proved to be the most unhealthy, a degree of security was always found to be afforded by sleeping in apartments raised a few feet from the surface; a fact of common observation, but one too little attended to by new comers to the West Indies."⁶ In another part of this sensible and interesting communication, the author states a fact connected with this subject which is too apposite to be omitted, namely, that in the military hospital, it was found that the attendants on the fever patients who were accommodated in the upper room were less frequently attacked than those who kept below with the surgical patients, without holding communication with the fever wards.⁷

¹ Imray, *Edinb. Med. and Surg. Journal*, lxiv. 335.

² Second Quarantine Report of Board of Health, p. 21.

³ *Diseases of the Army*, p. 306.

⁴ *Cyclopædia of Practical Med.*, p. 280.

⁵ *Med.-Chir. Trans.*, viii. 143.

⁶ *Edinb. Med.-Chir. Tr.*, ii. 57.

⁷ *Ibid.*, p. 59; Bancroft, *Sequel*, pp. 448, 449.

Dr. T. Smith, in his account of the epidemic of Gibraltar, in 1828, observes, that the examples of people enjoying immunity from situation alone, though in constant communication with the sick, might be multiplied without number. "In the same house, even, families occupying the upper stories were known to escape, while those on the ground floor suffered severely. In Bos-sana's house, at Rosia, for example, many individuals in the upper story escaped the disease, while every person in Belasco's family, living directly under them, and who had not passed through the fever, were attacked. In another house in the south, sixteen individuals died on the ground floor, and not one was taken ill in the upper story."¹

Berthe remarks that at Cadiz, in 1800, it was distinctly observed that the malady affected, with scarcely any interruption, all the houses situated on the same side of the street, and that it rarely passed over to the other side, where the streets were wide and well aired (p. 74). This, and other facts of analogous kind, Berthe conceives to be clear proofs of contagion; but, as Bancroft has well remarked, the next-door neighbours, on the same side of the street, are not likely to have been the only persons in all Cadiz who visited and approached each other;² and if these were exclusively affected, and the residents of opposite houses escaped, the attack of the former and exemption of the latter must be due to some other cause than that assigned. Nor is it less unlikely, considering the prevailing opinion as to the mode of propagation of the disease, that the immediate neighbours were more disposed than others to visit the sick.

But it is not alone in cities and towns that such circumstances occur, and that the explanation may be sought in the absence of intercourse between the sick and the healthy. They have presented themselves in ships at sea, where, from the nature of the accommodations or arrangements, the separation cannot be obtained. In these it is usually found that the fever, at the beginning, is confined to a small space—continuing for a while in one berth, whence it sometimes crosses to the opposite berth; sometimes travelling along one side, and returning pretty regularly by the other; traversing, at times, the ship from the rear to the fore-part, or in a contrary direction. In most instances, if not in all, the source of infection is below; the disease continues local and circumscribed in its sphere of action, and is extended only as the cause beneath is generated more slowly or rapidly in different instances. "Thus," says Dr. Wilson, "a man is removed from his berth to the main deck, labouring under fever, one day; another is removed the day after; one, two, or three the next day, and so on; the disease beginning then to extend itself in one or more directions, goes on till it pervades the ship generally. Yet all the while it cannot be traced from those on the main deck to any other part of the ship."³ In some instances, it is limited to a particular section or side of the vessel, the rest remaining comparatively free.⁴ In the case of the Rattlesnake, mentioned by Dr. Wilson, "the fever was first manifested near the main

¹ Edinb. Med. and Surg. Journ., xxxv. 35.

² Essay on the disease called Yellow Fever, p. 459.

³ West Indian Fever, p. 158.

⁴ Ferguson, Med.-Chir. Trans., viii. 142.

hatchway: the marines and the midshipmen of one berth suffered its earliest and most severe effects. Afterwards it proceeded forward rapidly, but pretty regularly, till it had affected almost the entire ship's company; but it did not go beyond the steerage in the opposite direction, no one being attacked in the gun-room except the purser, and I have good reason to conclude that in his case it was derived from the shore." "But its local origin and limited range of action were most strikingly exemplified in the berths of the midshipmen and other officers of that class. They were placed exactly opposite to each other, with the pumps at equal distances between them. One gentleman was affected in the starboard berths, while every member of the larboard berths was laid up nearly at the same time."¹

It may not be amiss to remark, on the other hand, that, in widely-extended epidemics—spreading their ravages over a large surface—it not unfrequently happens that small localities and single houses, situated in the midst of the infected district, and communicating freely with the latter, remain free from the disease. Dr. Vatables informs us that at Guadaloupe, in 1826, the disease spared completely some companies of infantry occupying Fort St. Charles, while other companies in the same fort suffered severely, though only at a very short distance from the former.² For the following fact we are indebted to Dr. Ferguson.

"The year 1816 was one of the sickly seasons of the West Indies. Towards the close of the year, the endemic fever pervaded the whole island of Antigua, and even the coloured population began to suffer, like those described by Dr. Frazer in Guiana, from intermittent and remittent fevers. To a white regiment in Monk's Hill Barracks were assigned the dock-yard guards of English Harbour, the most pestiferous in the West Indies, where the man, after standing sentry at night, would be seized shortly after with the fever, and expire in all the horrors of black vomit when carried back to Monk's Hill (a small insulated hill, 600 feet high, immediately overhanging the marshes of English Harbour). This occurred to numbers; but in all that time not a single case of that disease originated in, or was communicated to, the quarters—that is to say, the families of the garrison, the staff, and non-combatants of every kind; all, in fact, who never slept out of it, remained free and untouched."³

The Fever is not communicated beyond the infected locality.—It has been seen, in the two preceding sections, that the yellow fever is localized within bounds more or less restricted, spreading in some cases over a whole city, or town, or village, and in others limiting its ravages to only a small portion of these. Differing from contagious diseases, which, from the place where they are first introduced, manifest a tendency to extend in whatever direction they are carried by the sick, the fever in question is strictly limited to the locality—large or small, as the case may be—where it breaks out. Between the district infected and those which remain free, the line of demarcation is,

¹ Op. cit., pp. 159, 160.

² Op. cit., p. 334.

³ London Medical Gazette, xxi. 1033.

though more particularly perhaps in our own than in tropical latitudes, well defined. At a short distance from infected districts the fever does not appear; all who abstain from visiting them remain secure from an attack. On the other hand, those who venture in them are liable to be infected. All cases, in whatever part they occur, may be traced to exposure within the boundaries of such districts; while individuals who sicken there, but go through the disease beyond those boundaries, are not a source of danger to those by whom they are surrounded. As illustrative of this, it may be mentioned that in Philadelphia, as indeed in most other places of similar extent, the fever never or seldom occurs in the upper parts of the city. While it rages with violence in the infected spot—which, on some occasions, is of very restricted extent—no case is to be found in other sections of the city; and if they are so found, the individuals affected are sure to have contracted the disease in the infected area. Our population have lived to learn that, by avoiding the latter, no danger need be apprehended; and experience has taught them, besides, that individuals who, having taken the disease in the sickly district, go through it in the upper sections of the city—however malignant the case may be and whether ending in recovery or death—may be housed and nursed with perfect impunity, the fever being in no instance communicated from the sick to the healthy.

Dr. Jackson remarks, in allusion to the fever of 1820, that as the disease prevailed only within certain bounds, it afforded the most favourable opportunity to test the question of its being communicated by the sick to those in health. What was the result? “Nearly one-half of the cases of the disease were scattered in different parts of the city, evidently contracted in some one of the original seats of the infection.” “Many of them were under circumstances in the highest degree calculated to aid its propagation by means of contagion, did it exist. They occurred in the persons of the poor, in confined and ill-ventilated apartments, in houses crowded with inhabitants, in some of the filthiest and narrowest lanes, alleys, and courts of the city, in which the negro epidemic had been or was still prevailing; yet, in conditions thus propitious to its propagation, not a single instance is known of any person attending on, or who had communicated with, the sick, or their apartments, having taken the disease” (p. 83). “The friends and relatives of the sick became their kind and faithful attendants, and the constant companions of their bedside. I have seen wives, with devoted affection, supporting in their arms and soothing with caresses the last moments of their dying husbands; children consoling with their attentions their expiring parents, and parents, overwhelmed with grief, administering to the last wants of their departing offspring.” “The performance of these sacred offices of humanity, and examples of the best and kindest feelings of the heart, it is gratifying to know, were on no occasion productive of ill consequences. The indulgence of them brought none into danger, by the disease being contracted” (p. 85). This immunity did not occur in that epidemic only. From the commencement of the disease in 1805, a constant intercourse was kept up between the healthy parts of the city and the sickly parts of Southwark. “In consequence of this, many cases of the fever were introduced into central and healthy parts

of the city. These cases excited, at first, no small degree of alarm in the minds of the neighbouring inhabitants. But they were found to terminate sometimes favourably and sometimes unfavourably, without communicating the disease either to the families in which the sick lay, or to any individual who, from motives of friendship or otherwise, had had free access to their chambers. This was observed to be not an accidental, but a uniform occurrence, to which there was no exception; for in no instance did the disease spread from the sick to the well, in consequence of cases being thus introduced into healthy neighbourhoods.”¹ Such also was certainly the case in 1853; and no one acquainted with the occurrences of that year will refuse his assent to the statement of Dr. Jewell, that “in private practice, although numerous cases were attended away from the infected portions of the city, we have yet to learn that the disease, in a single instance, was propagated from the sick to the well, although there was an unrestrained intercourse between the patients and their immediate friends.”² Dr. Jewell, let it be remarked, is, or was, friendly to the doctrine of importation. Dr. Hill says, in reference to the fever of Wilmington (N. C.) in 1821: “I could not observe without conviction, too, that my patients who sickened in the infected districts did not disseminate the disease. In several cases, however, five or six weeks afterwards, other members of the same family have sickened—but not referable, surely, to contagion dormant for so long a time. Rather, in conformity with the well-known cases of epidemic diseases, the epidemic cause had spread itself more diffusely, and vitiated or poisoned our whole atmosphere.”³ The disease of New York, in 1819, was actually confined within the bounds of the forbidden district; “and however many sick were taken from it, and wherever removed to, still was the number of its victims not increased by a single case occurring beyond the barriers erected by the Board of Health.”⁴ In Boston, in 1802, it was, as it had been before, very limited in its extent, being wholly confined to houses promiscuously situated at the heads of wharves at the south part of the town; and it was remarkable that if a patient under the disease was carried out of the range of the morbid atmosphere, into a healthy part of the town, and attended by persons there resident, the disease was not communicated in a single instance.⁵ In New London, in 1798, the same thing occurred. When persons who sickened in the infected district were carried into other parts of the town, and there died, the disease did not spread from them to their attendants.⁶ It is admitted, by Townsend, that in New York, in 1822, 104 cases occurred in the uninfected districts; 65 of them had taken the fever in the focus of infection, by being employed in or merely passing through the same; the other 39 had been removed there, and it is thought may have carried their bedding and clothing with them. Yet in no one case did they communicate the fever, a fact which puzzles Dr.

¹ Caldwell, *Fever of 1805*, p. 52. See also *Fever of 1803*, *Reposit.*, vii. 153.

² *Trans. of Coll. of Phys.*, N. S., ii. 119.

³ *Med. Rec.*, v. 88, 89.

⁴ Watts, pp. 307–8. See also C. Drake, *Med. Reposit.*, xxi. 127.

⁵ *Med. Reposit.*, vi. 339.

⁶ Holt, *A Short Account of the Yellow Fever*, p. 17.

Townsend, and which he tries to explain away.¹ In Baltimore, in 1819, the disease, as already stated, existed at Fell's Point. "It was as easy to separate the healthy from the diseased portion of the Point, as it would be to stretch a line across a room, so circumscribed was the extent of the noxious poison. And although a continual intercourse was kept open between the city and Point, and diseased persons continually carried up to the centre of the city, and dying there, yet in no instance was the disease thus communicated."² The same thing was noticed at Wilmington (Del.) in 1802.³

From this we are justified in drawing a conclusion favourable to the doctrine which ascribes the fever to some specific cause existing in the locality where it is thus limited, and adverse to its contagious property. That cases are recorded of the disease having occurred at a distance from the strictly infected district, I shall not attempt to conceal; nor is it less true that in some instances the disease may have occurred in persons who had been in attendance on the sick; but these cases are rarely, if at all, met with, and of those cited some are far from being well authenticated, being possibly cases of aggravated ordinary remittent fever, the writers by whom they are adduced generally regarding the latter and yellow fever as identical in kind; while the others have apparently been the result of exciting causes sufficiently strong to excite into action the specific poison which, though too feebly diffused in such localities to occasion a general effect, is yet powerful enough to produce the disease under the circumstances described, in persons more than usually predisposed to its morbid influence.

The Fever not communicated in the country.—Complete as is the immunity in this and most other cities, at even a short distance from the infected district, it is, as might be presumed, even more so in the purer air of the country. There the disease never occurs, however constant and intimate the intercourse may be with the infected place. None are there affected, but those who have taken the disease in the latter, and neither they nor such patients as are brought there from the city, communicate the infection to any one around them—physicians, nurses, relatives, or visitors—the whole of the surrounding population remaining as secure from the fever as if placed at a thousand miles' distance from the individual infected. On this subject, the testimony of the profession is almost unanimous, that of the public nearly so; and, on a knowledge of the immunity enjoyed beyond the infected district are founded some of the most useful sanitary regulations adopted in recent times in this and other cities. This immunity was noticed by Dr. Rush during the epidemic of 1793, in relation to which, he says: "It has been remarked, that the fever did not spread in the country when carried there by persons who were infected, and afterwards died with it" (vol. iii. p. 110). In an-

¹ An Account of the Yellow Fever, &c., pp. 64, 65. See also Beck, New York Med. and Phys. Journ., ii. 473-480.

² Reese, p. 71; Revere, Med. Rec., iii. 231; Chatard, Marseilles Journ., v. 335; Letters on the Fever of 1819, p. 32; Chervin, Rep. of Acad., p. 26.

³ Vaughan, p. 20.

other place he says, in reference to the fever of 1797, that out of two thousand persons who carried the disease into the country from our cities, there were not more than three or four instances to be met with of its having been propagated by contagion; and, at a still later period, when he had retracted his former views relative to contagion, and, as we may infer, when he had become convinced of the unsatisfactory nature of those alleged cases of communication, he adduces as a strong reason for abandoning the views he had originally entertained, the fact that the disease does not spread in the country when carried thither from the cities of the United States.¹

Deveze (p. 220), Caldwell,² Dalmas (pp. 57, 61), Chapman,³ Coxe,⁴ Monges (ii. 62), in like manner, bear testimony as to the fact of this non-communication of the disease in the pure air of the country around Philadelphia, and of the immunity from danger enjoyed beyond the limits of the infected district. Similar observations have been repeatedly made in other cities and towns of the United States. Writing in 1798, the late Dr. Davidge, of Baltimore, says: "It is an unquestionable truth, a verity of the most public notoriety, that not one of the very great numbers who have left the cities and towns, some of whom have died and some have recovered, has communicated the yellow fever to those who have attended. Not one solitary fact has ever reached me; and my scepticism is such as to lead me into a persuasion, that there has not existed one unequivocal, well-analyzed fact, of a patient going to the country, and there multiplying this fever" (p. 88).

We learn, as regards the epidemic of that city in 1819, that many of the poor left the seat of infection, and were admitted into the neighbouring rope-walks and tents prepared for the purpose. More than 1,000 were furnished with subsistence, and hence were of that description of people among whom disease is very apt to be engendered and where contagion, if it existed, would spread with the utmost rapidity. Yet, in this instance, no case of the kind occurred. The occupants of these places of refuge remained healthy, though some died.⁵

The several epidemics that have occurred in New York, furnish us further evidence of the immunity contended for. In his description of the fever of 1795 (p. 49), Dr. Bayley, whose accuracy of observation has never been questioned, says: "Many people were confirmed in the opinion that the fever was contagious from the vague reports of examples having occurred where persons were infected with fever in the country, in consequence of having communication with those who had carried the disease from the city. Now, I believe, there are not any well-authenticated cases of the sort." Ten years later, Dr. Miller, in his Report on the fever of 1805, states that it is admitted the disease does not spread when the sick are removed from the impure air in which it was contracted. "By breathing this impure air," he adds, "without exposure to the effluvia of the sick, persons are every day attacked;

¹ Letter to Miller, in *Reposit.*, vi. 156.

² *Mem.*, 1800, 1826, &c.

³ Chervin's Report to the Acad., p. 26.

⁴ *Ibid.*

⁵ Reese, *Obs. on the Epid. of 1819*, &c., p. 52, &c. See also Chatard, *Marseilles Journ.*, v. 336; *Letters on Fever of Baltimore*, p. 55.

while, on the contrary, without breathing, however exposed to such effluvia, no person is attacked," and, from these circumstances, he draws the conclusion that the impure air of the infected district is the cause, and not the effluvia from the sick.¹ At a much later period, Dr. Watts, speaking of the fever of 1819, states that the poor of the infected district, to the number of one hundred and fifty, were removed to Fort Richmond, on Staten Island. As might naturally be expected, some of them carried the disease with them, and several, if not all of those who sickened, died of yellow fever; yet the disease did not spread, although these poor people had been thus hastily crowded together, under circumstances calculated to favour the extension of febrile poison. Nor did a single instance of the disease, spreading by contagion, occur at Fort Stevens on Long Island, where the sick poor were conveyed whenever it was practicable.²

In Boston, during the fever of the same year, no case subsequent to those that took place about Fort Hill, appeared to be derived from individuals who had sickened. Cases of the disease occurred in the persons of those who had visited that section of the town; but in no instance did they communicate it to others.³ The same immunity was observed in that city during former visitations in 1798 and 1802.⁴

The like incommunicability of the disease out of infected districts has been noticed in New London,⁵ Providence,⁶ Newburyport,⁷ Alexandria,⁸ Wilmington (Del.),⁹ and Augusta.¹⁰ Lining, more than one hundred years ago, remarked, in describing the fever as it prevailed in Charleston in 1732, &c., that although the infection spread with great celerity through the town, yet if any from the country received it there, and sickened on their return home, the infection spread no further—"not even so much as to one in the same house" (p. 394). Other writers on the fever of that city have fully corroborated the statements of that able physician.¹¹ Sir Nathaniel Johnson, one of the early governors of South Carolina, appears to have been apprised of the security thus afforded by the country, and of the impossibility of carrying the disease from the infected city to a distance, however short, anterior to the time of Lining. Hewatt, in his history of that State, informs us, that while the fever raged in Charleston (in the year 1702), the worthy governor held his head-quarters about half a mile distant from the town,

¹ Works, p. 102.

² The Med. and Surg. Register, i. 307-8; see also Drake, Med. Rep., xxi. p. 129.

³ N. Engl. J., viii. 381; N. Am. Rev., x. 395; D. Dexter and Shurtliff; Chervin's Rep., p. 26.

⁴ Rand, Med. Repos., ii. 470-1.

⁵ Sam. Lee; Chervin's Rep., p. 26; Holt, An Account, &c., Med. Repos., iii. 292.

⁶ Wheaton, Med. Repos., x. 333, 335, 337; William Bowen; Chervin's Report, p. 26.

⁷ Dr. Noyez, Chervin's Report, p. 26.

⁸ Dr. Semmes, Chervin's Report, p. 26.

⁹ Didier, &c., Ib., p. 26; Vaughan, p. 20.

¹⁰ Robertson, Report on Fever of 1839, p. 9.

¹¹ Ramsay, History of South Carolina, ii. 87; Ib., Rev. of Improvements, Progress, and State of Med. in the 18th Century, p. 39; Ib., Fev. of 1807, Med. Repos., ii. 234; Tucker, Barton's Journ., ii. 22-3; Shecut, pp. 96-7; Irvine, p. 9.

not wishing to expose his men to the dangerous infection, unless from necessity. We are not told—which we would doubtless have been had such been true—that any of the troops gathered around him, received the infection either from the air or from contact, by their communication with the garrison, in their necessary intercourse with each other.¹

Dr. Waring states, that it was remarked at Savannah during the epidemic of 1820, that although many went into the country, and died with black vomit, there was no further propagation of any similar affection.² The same non-communication in the country around that city was noticed during the memorable epidemic of 1854; not one of the many individuals who sickened at a distance from the infected place, spread the disease to their attendants. At Natchez, the fever, in 1823, extended to some distance in the country, on the same side of the river—originating at each point; but the inhabitants of the opposite side were not more sickly than usual. “It afforded a safe retreat to the citizens of Natchez who fled to avoid the yellow fever. Many who were ill with the disease were carried across the river, which is but a mile wide, and died opposite to the city in this low district; yet in no instance was the disease communicated.”³ In Mobile, in 1819, it was observed that the suburbs of the town, at no greater distance than one mile from the river, were as healthy during the prevalence of the fever as more distant parts of the country, and it is not known that the disease was communicated, in any instance, to persons out of the town, by the removal and attendance of the sick.⁴

In New Orleans, also, the fever, when carried beyond the precincts of the city into the purer atmosphere of the country, never proves the source of infection. Dr. Chabert tells us that, however frequently the disease may be carried from the afflicted place, no case, *certified by a physician*, is on record, of its having been communicated in the country (pp. 96–7, &c.).

In 1819, '20, '22, Dr. Thomas informs us (p. 203), that persons from the country near New Orleans took the fever in town, and were ill in the country without communicating it to others, though sleeping in the same room or bed with the Europeans and non-acclimatized individuals. Gros, and others, testify to similar facts; and we find their statements fully and officially corroborated in the report prepared by a committee of the Medical Society of that city on the epidemic of 1819: “A number of individuals at New Orleans, it is there stated, were attacked with the yellow fever at various periods, and retired to the country; several there died without communicating the disease to the inhabitants. The transportation of merchandise, of provisions, and articles of every description has continued without interruption during times of epidemics, through means of boats plying on the river and by land conveyances. Nevertheless, the disease has not, on that account, shown itself in the country. Several

¹ Hewatt, Hist. Acct., i. 182.

² Report, &c., p. 36.

³ Cartwright, Med. Rec., ix. 5.

⁴ Report of the Committee appointed to investigate the cause and extent of the late extraordinary sickness and mortality in the town of Mobile, in N. Y. Med. Repos., June, 1820, p. 343; see also Letters on Baltimore Fever of 1819, p. 207.

planters, compelled, on account of business, to visit the city during the ravages of the fever, contracted the latter, returned home, and there died, without communicating the disease to any of those who approached or nursed them" (p. 40).

As may be perceived, it is not alone among the opponents of contagion that this exemption has been insisted upon, for several of the authorities cited—Lining, Dexter, Shurtliff—while recognizing and recording the facts, were advocates of the doctrine in question, and of the importation of the disease. In a word, every succeeding epidemic among us has furnished reason to admit the correctness of the statement that, when the yellow fever reigns in a city of the United States, the inhabitants of the infected district—sick and well—remove in great numbers into the healthy parts, and there never communicate the disease to those by whom they are received and nursed; though the sick are sometimes received in filthy, small, and imperfectly ventilated apartments. And from the absence of the power of transmissibility under the circumstances referred to, combined with other facts already stated, or which are soon to be mentioned, it is difficult to withhold the conclusion, that the yellow fever of the United States is void of the property contended for by contagionists. Such being the case here, we may easily conceive that matters will not take a different turn in other parts of temperate climates.

Hence, in Cadiz in 1764, it was found, as we learn from Lind (pp. 125–6), that people of fashion, whom the dread of the distemper forced to retire into the country, remained there in perfect safety from it; and seamen who took the disease in the city and sickened on board of their vessels, did not communicate the infection to those around them (*Ibid.*, p. 125). In Seville and Alicant, also, it was found that the fever was not communicated at even a small distance from the infected districts.¹ Similar observations were made at Barcelona, both during the fever of 1803 and the memorable epidemic of 1821.² In regard to the latter, it is said that not a single positive fact can be cited to prove that healthy persons contracted the disease beyond the sphere of the action of local causes, however freely they communicated with the sick and their effects—that all the individuals who fell ill in various parts around Barcelona had received the infection in the port, or within the walls of the city, and that "whether the aforesaid sick died or recovered, there is no proved fact to show that any of their nearest attendants were affected if they had not been in Barcelona."³ Dr. T. Smith, in his account of the epidemic which prevailed in Gibraltar in 1828, adduces as one of the reasons which led to the almost universal disbelief in the contagion of the fever, "its never having been carried from one locality to another by persons or other means," and "its not having spread either at Europa Flats, the Neutral Ground, Windmill Hill, or in the bay amongst the shipping,

¹ Chervin's Rept., pp. 36, 46.

² Pariset, p. 61; Chervin, *Mat. San.*, p. 29; *Ib.*, *Fever of Spain*, p. 40; Rayer, p. 43.

³ Manifesto, Maclean, p. 133; *Ann. de la Méd. Physiologique*, i. 425.

although free communication between the town and those places was permitted."¹

The non-transmissibility of the disease in the localities mentioned is now so generally admitted that it may be regarded as placed beyond a possibility of doubt and controversy. It has been noticed not in one epidemic only, but in all—in 1804, 1810, 1813, 1814, and 1828—and is illustrated in the most ample and satisfactory manner by Amiel,² Burnett,³ Martindale,⁴ Playfair,⁵ Short,⁶ Bancroft,⁷ Donnet,⁸ Chervin,⁹ Guyon,¹⁰ Donnot,¹¹ Whitemarsh,¹² and other writers on the yellow fever of that city. The first of these, in his answer to the queries put to him by Mr. Frazier, relative to the fever of 1814, remarks: "An individual, labouring under our epidemical fever, on being removed to a pure and ventilated place, such as the Neutral Ground, or Europa Point, did not communicate the disease to those in the closest contact with him. This observation has been confirmed in many instances during the epidemic of last year among the foreign recruits quartered at the Brewery Barracks." And in his answer to the queries from the Army Medical Board on the epidemic of 1828 (*Edin. Med. Journ.*, xxxv. 264), he again reverts to the subject, remarking that "the fever did not spread at the Europa Flats, on board of the vessels in the bay, or on the Neutral Ground, when carried thither from the town; and that numbers of individuals who had the seeds of the disease about them, even in the case of their falling sick or dying there, did not communicate the malady to their neighbours or attendants."

Ninety-two women of the 12th regiment (which had been removed to the Neutral Ground) and 190 children who were never allowed to repass Bayside barrier, continued perfectly healthy. One woman only, who, during the period, obtained leave to enter and stay a few days in the garrison, caught the fever and died of it. Several of these women passed the night in the same beds with their husbands, attacked with, and labouring under the epidemic fever, and, besides, continued, as well as their numerous children, to use the same bedding after the men had been removed to the hospital; but in no instance was the disease contracted by the wife or the children even after that full exposure. "It has been said," adds Mr. A., "that the pure air of the Neutral Ground checks the contagious property of the fever; but when the wife, in the same bed, came in contact with the patient scorched with febrile heat, or bedewed with copious perspiration, when she inhaled under the same tent

¹ *Edin. Journ.*, xxxv. 13, 18, 46, 47, 49, 50. Dr. S. enters in detail on the subject, and refutes the statements made by Sir W. Pym in proof of the absence of the immunity in question.

² Answer to queries by Mr. Frazer; Bancroft's Seq., p. 395; Burnett, p. 324; Answer to queries, *Edin. Journ.*, xxxv. 264.

³ Page 453.

⁴ Cited by Burnett, pp. 436-7; Bancroft's Seq., p. 393.

⁵ Burnett, p. 437; Bancroft's Seq., pp. 398-9.

⁶ Burnett, pp. 437-8.

⁷ Essay, p. 393, &c.

⁸ Bancroft's Seq., p. 166.

⁹ Answer to Guyon, pp. 220, 221, 238; *Ib.*, Fever of Spain, p. 87; *Ib.*, Lettre à M. Monfalcon, p. 12.

¹⁰ *An. Marit.*, 1830, i. 751.

¹¹ Bancroft's Seq., p. 166.

¹² *Ibid.*

the effluvia of his breath, how could the air, however pure, sufficiently interpose to arrest the process of contagion, and its fatal consequences" (p. 275).

A similar circumstance was remarked at Arcos (Spain) in 1800 and 1804, as also at Barcelonette during the epidemic of 1821.¹ On that occasion, "the fever did not pass the ditch which surrounds the city. And if this undeniable fact does not prove that the cause has been purely local, let the cause be indicated by which it has been thus circumscribed and limited. Not a single positive fact can be cited to prove that any healthy person has contracted the disease beyond the sphere of the action of local causes, even having communicated with the sick and their effects. Thus, as during the whole month of August, those who fell ill, in front of the Casa Lonja, in the streets de los Encentes and de los Molas, it is certain, contracted their maladies in the port; so the few who sickened in Gracia, Sans and other parts of the plain of Barcelona, contracted theirs within the walls of that city. And whether the aforesaid sick died or recovered, there is no proved fact to show that any of their nearest attendants were affected, if they had not been in Barcelona.

"A great number of persons, who, after passing the whole day in the capital, retired at night to their families, either in country houses, or in the nearest villages, communicated the disease to no one, whatever were the situations of those houses; not even those who had quitted the town on the very day in which they had lost a member of their family, and notwithstanding their having taken no precaution."²

At Leghorn, the disease did not extend beyond the city, and from Palloni we learn that with the exception of some cases in which it was limited to the individuals who carried the germ in them, and afterwards sickened; the fever did not prevail at the smallest distance from the source of infection, though merchandise was daily transported thence to different parts of the country, and a large number of individuals left the city and took shelter in the vicinity.³ The same results obtained at Port du Passage in 1823.⁴

Even in the West Indies, where the disease, though to a certain extent localized, diffuses itself over a larger surface than it does in many parts of temperate latitudes, the absence of the transportability in question beyond the infected districts has long been recognized. Dr. John Hunter remarks that "what may be considered as the *experimentum crucis* to prove the non-contagion, is when the sick leave their usual residence and go to other places which are healthy without spreading the disease."⁵ We are informed by Dr. James Clark, in his account of the fever of Dominica in 1793-'4, that when patients labouring under this fever were removed to a high situation, for the

¹ Chervin, Rapport, p. 40, on the authority of Drs. Obregor and Calderon; Chervin, Fever of Spain, p. 51.

² Manifesto, Maclean, p. 133; An. de la Méd. Physiol., i. p. 424.

³ Osservazioni Mediche, p. 30; see also Edin. Journ., ii. 20; Valentin, Voyage, p. 169; Fabbroni's Report in Devezze, p. 231.

⁴ Chervin, Mat. Sanit., p. 35; Arruti, Trattato de la Fiebre Amarilla, and Jourdain, An. de la Méd. Physiol., iv. 51, &c.

⁵ Dis. of Army in Jamaica, p. 322.

sake of breathing a cooler and purer air, many fell victims to it; but the people about them were never infected, nor did the disease ever prevail in such places (p. 64). Bancroft remarks, and in this he is confirmed by Stewart,¹ who was an advocate of contagion, that similar observations were made even at Grenada; for persons who took the disease from the atmosphere of St. George, and sickened in the country, did not communicate it to any one near them (p. 417). At St. Lucia, while the fever rages with the utmost violence in the port and on the main land, it has never been known to extend to Pigeon Island, a small spot situated at a very short distance from the latter.² At St. Domingo, while prevailing with violence in the towns, and to some extent in the neighbouring plains, the disease is never communicated by the sick in the interior and in elevated regions.³ Dr. Imray was informed, by the surgeons of the Hospital of Basseterre (Guadaloupe), "that some instances came under their notice, in 1838, of planters, who had come to town for the transaction of business, and who had remained for a very short time, being attacked on their return to the country, and dying with black vomit; but from these points the disease never extended."⁴ The following statement, recorded in a report made to the French government by Dr. Cornuel relative to the epidemic of Guadaloupe in 1837, illustrates the non-transmissibility of the fever in a positive manner. As soon as the fever began to spread in the garrison of Basseterre, the government ordered off a large portion of the troops, and encamped them on the heights surrounding the town, where they were lodged on the various plantations. By these means the fever was put a stop to among them, unless they again exposed themselves by going to town, to the source of infection; and those who, having done so, sickened in the country, did not transmit the disease to others.⁵ Dariste remarks that, whatever be the extent to which the disease prevails in the cities of the Antilles, it is always circumscribed within their precincts, notwithstanding the perfect freedom of communication which is allowed. This circumstance, he adds, proves, in the most convincing manner, its non-transmissibility. The country is always preserved (p. 34).

Facts of similar import are mentioned in a report drawn up by the members of the Council of Health of Guadaloupe, Drs. Davers, Vanauld, and Cornuel, as also by Drs. Rouvier,⁶ Vatable,⁷ Lefort,⁸ and Kéraudren,⁹ Blane,¹⁰ Ralph,¹¹ R. Jackson,¹² Doughty,¹³ Ferguson,¹⁴ and Arnold.¹⁵ At Antigua, in 1817, it prevailed to a considerable extent in the town of St. John, but the country remained generally free. A case now and then oc-

¹ Account of the fever, in a letter to Dr. Hosack, Reg., iii. 188.

² Pugnet, p. 343.

³ Dalmas, pp. 40, 64; Bally, pp. 335-7; François, pp. 7, 8; Bonneau, p. 335.

⁴ Imray, loc. cit., liii. 93.

⁵ Rufz, p. 101.

⁶ Dissertation, p. 39.

⁷ Ann. Marit., 1st series, xi. 776.

⁸ De la Non-Cont., &c., pp. 25, 27.

⁹ De la Fièvre Jaune aux Antilles, p. 55.

¹⁰ On Seamen, p. 277.

¹¹ Edinb. Med.-Ch. Trans., ii. 64.

¹² Sketch of Fev., 1st ed., p. 393.

¹³ On Bulam, or Yellow Fever, pp. 50, 70, 71.

¹⁴ Recollections, p. 152.

¹⁵ On Bil. Remit. Fever, p. 30.

curred, not, however, till the progress of the epidemic was far advanced; but in no instance was it the effect of communication with the sick.¹ In Havana, the fever is never propagated in the country;² and in the island of Jamaica similar observations are daily made. For example, the plain of Liguana contains abundantly the cause of yellow fever, and the disease every now and then makes its appearance there; but, as Dr. Wilson remarks, it never extends six miles further in a northerly direction. "I run no hazard of contradiction," says that intelligent writer, "when I state positively that the disease was never seen six miles up the mountain side, except when carried there from the plain, and that it was never known to extend itself to a single person in the neighbourhood, or to affect any one who had not been recently in the plain." "The daily intercourse between the plain of Liguana and the mountain is considerable; in the plain the disease is often epidemic; persons affected with it there sometimes remove to the mountain, where they recover or die, as the case may be; yet, during a period of two centuries, it was never known that the disease was propagated in one instance only six miles from Liguana over the mountain."³

We are told, in no less a document than the report of the Medical Society of Guiana, that persons arriving from the country, and visiting Water (the infected) Street, were liable to be attacked; and some were attacked and died on their return home, without the disease spreading by contagion.⁴

Judging from all the facts I have thus collected in this chapter, the inference may fairly be drawn that the yellow fever is not a contagious disease; for, were it so, its sphere of agency would not be so circumscribed. It would be communicated out of the place where it first appeared, and would extend more or less readily to other parts of the city or to the country—wherever, indeed, the poison would be conveyed. But admitting, for the sake of argument, that the disease may become, from peculiar circumstances of locality, endowed with contagious properties, these facts would still tend to show that it is not to be viewed as possessing the property of transmissibility when carried beyond the infected district to other parts of the city or place where it prevails, or to the purer air of the country. From the preceding statements, too, we perceive that this absence of contagion is observed not only in the city of Philadelphia, but in every other place—in this country, in Europe, or in tropical climates—wherever indeed the fever prevails habitually, or has done so occasionally. It is not therefore a peculiarity or accidental circumstance attached to our fever alone, but constitutes an important characteristic of the disease everywhere. We are justified, I repeat, in drawing these deductions, for when with this before us, we inquire into the results, under like circumstances, of diseases of a truly contagious character, no unprejudiced inquirer will doubt the impropriety of classing the yellow fever among these. In no case do we find them so circumscribed in their localization. They do not limit their ravages to, and exhibit their contagiousness only within, a small area. Contagious in one

¹ Musgrave, ix. 108.

³ Wilson, p. 83, note.

² Osgood, p. 19; Madrid, pt. 1, p. 58.

⁴ Blair, p. 153.

place, they are so everywhere else; and all diseases that manifest such properties in one part of a city, town, or district, will not fail to do the same, and to be propagated from individual to individual, wherever it is carried by the sick or their effects. So true is this, that whenever a disease does not manifest a power of contagion in some places, we are justified in concluding that it will not do so elsewhere, and must take rank, in our nosological arrangements, among complaints of a different category. There is no fact to show that the pure air of the country is a neutralizer or destroyer of contagion—not even that the effect is produced by mountainous atmosphere or that of the open sea. Every day presents instances of the reverse kind—contagious diseases spreading in a pure atmosphere with nearly as much certainty as they do under different circumstances; and if, when diffused through an extensive or open space, where the ventilation is free, and in clean and comfortable apartments, a contagious poison appears to be rendered harmless, the result is obtained not by the decomposition or destruction of the poison, but by its dilution and dissipation, and by its meeting with individuals less predisposed to receive its impression. Prevent these effects, and the poison will manifest, *de novo*, its deleterious properties; a result very different from that obtained in the yellow fever, which, as we have seen, is never transmitted out of the infected district, even when the diluting influence in question is impeded by the nature of the circumstances under which the person affected is placed.

That the non-propagation of the fever out of an infected spot is not due to any power enjoyed by the atmosphere of the place where the sick have been carried, by which it is enabled to destroy the contagious virus, may be inferred from the circumstance that, were such the case, yellow fever could not originate and prevail there at all; whereas we know that in many instances the disease has shown itself and spread with the utmost violence in elevated situations, under circumstances when it was impossible to refer it to the instrumentality of the sick. On this subject, it will be sufficient to refer to the epidemics of Brimstone Hill, St. Kitts, in 1812 and 1825, and of Stony Hill in 1818, 1825, and 1827, when the disease spread with as much fatality as it ever has done in situations of a different character.¹ The fever has even appeared, without the least possibility of attributing it to importation, in localities—both in cities or rural districts—where on other occasions it had often been carried by individuals who had taken it elsewhere, and had failed to be communicated from the sick to the well. If the air of those localities had been pure enough to destroy or neutralize the contagion in the latter instances, there is no reason why it should not have done so always, especially as it remained apparently as pure—so, at least, would say the contagionists. More natural is it to suppose that contagion has had no influence in the matter; and that the disease arose in such places from the development of a morbid poison, the constituent elements of which did not exist there before. Even while I am writing these lines, I learn from a highly-valued correspond-

¹ Jackson, Sketch, pp. 14, 15; Wilson, p. 99; Report on Sickness in the British Army, pp. 38, 58.

ent that the fever this year has invaded portions of the city of Charleston in which it was never known to have appeared before. "The jail and marine hospital stand adjoining each other. For forty-five years, yellow fever patients have been treated in the hospital, and no case has ever been known to exist in the jail until this year. This year there have been 18 cases in that establishment. Further, the quarter of the city in which these institutions are situated has always been exempt from yellow fever till this season. It has suffered severely this year. What is the reason? Contagionists are citing these facts on their side; and it must be admitted that the bare announcement that yellow fever existed in the hospital, and then broke out in the jail adjoining, looks plausible. But when we add that these buildings have held their relative positions for forty-five years, and that fever has never before spread from one to the other, it becomes necessary to look for another." What is true in one case, is true in all of the same kind.

The facts to which attention has been called in the present chapter, are very numerous; the testimony on which they are offered is not to be impugned. Many others of the same kind might be added to the long array already presented, from the records of the fever in every place where it has manifested itself during the last two centuries. Instances of the exemption in question have been observed time after time, and without fail, in places where exceptions, if there could be such, might be expected to occur. They have been acknowledged to present themselves even by zealous contagionists. And surely they may very naturally be regarded as of sufficient force to neutralize or counterbalance—indeed, completely to overturn the half dozen instances in which a contrary result is supposed to have been obtained. In regard to some of these exceptional cases, doubts may be entertained relative to the ability of the reporters to decide as to the true yellow fever nature of the disease stated to have been communicated. Who can vouch for the accuracy of diagnosis in regard to the cases recorded by Drs. Coventry, McBride, Moore, the *Rev.* M. Channing, and even of the late Dr. Banks, of Clinton (Miss.), mentioned in another chapter? Experienced city physicians are occasionally deceived, can country practitioners like those mentioned, unused as they are to grapple with the yellow fever, be certain they are not deceived too when they meet with such cases? Will any one assume the responsibility for the *soi disant* FACTS collected by partisan contagionists of the Carpenter, Monnet, Pariset, Audouard, and Pym school? Who can say positively that, if searched for, some other cause will not be found to account for the occurrence of the genuine fever in unusual localities? Who can be sure that in some instances the result has not been the effect of coincidence, or of the gradual or sudden extension or evolution of the poison giving rise to the disease? For my part, I cannot but believe that, if a disease has been found by almost everybody, in every place and at all times, to be incommunicable out of infected districts, and a very few instances of a contrary kind are adduced, some error must have crept into the explanation of the origin of the latter. It is not possible that that disease could universally exhibit a complete freedom from the property of being propagated under the circumstances mentioned, and in

a few instances—as if with the sole view of gratifying the wishes of the contagionists—manifest the possession of that same property, sometimes too in situations least calculated to give it effect. Be this however as it may, the remark of a recent writer is applicable to the case before us: “In any alleged instance of this, such instance being an exception to ordinary experience, it is but reasonable to demand that it should be substantiated by such clearness and weight of evidence as is required in other cases to establish a fact antecedently improbable.”¹

CHAPTER XIX.

PROOFS OF NON-CONTAGION—CONTINUED.

Effects of Emigration.—The local habitation of the disease, its non-contagiousness—and, with these, its non-transportability—are further illustrated by the effects of emigration from, and of the clearing of, an infected locality, and of the means employed to prevent its being reoccupied or visited during the continuance of the sickly season. The results obtained in this, and some other countries by sanitary regulations intended to effect these objects, are entitled to the regard of every medical inquirer. These regulations are based on the well-known facts: 1. That individuals whose susceptibility to the impress of the yellow fever poison is not impaired by acclimatization or otherwise, run great risk of taking the disease, and very generally take it when they visit infected localities, and on the contrary are preserved by abstaining from venturing there so long as the disease prevails. 2. That no harm accrues to those residing beyond those localities by the presence of persons—sick or well—who remove from the latter. In former times, it was the custom to place a mark on the door, or a flag at the window, of houses containing the sick, in order to deter people from entering them, lest they might themselves approach too near the source of contagion, and add to the number of the diseased. Efforts were sometimes made to remove the sick to some public establishment prepared for that purpose. Whether this was effected or not, the inhabitants generally who could not or would not leave the localities, were allowed to remain. In Spain, and other parts, the sick and the well were compelled to do so. Emigration was prohibited, and the laws enforced in the most stringent manner, with the view of guarding against the contamination of the rest of the community. What were the consequences? The disease continued to prevail—its ravages extended—its victims were greatly multiplied, until the epidemic influence was cut short by frost or some other disturbing agency.

¹ Second Report on Quarantine, p. 68.

Examine now the effects of an opposite course. In 1797,¹ the clearing of that portion of this city where the fever prevailed was partially effected. In some places, a large number of the houses were emptied and closed. Other streets were completely cleared, and barricades erected to prevent ingress. The same plan was pursued at Providence (R. I.), in 1805. The fever broke out on the 19th of July, and from that date to the 25th, about twenty were attacked—a goodly number for so small a place. “At this time the Town Council ordered all that part of the town (where it prevailed) to be evacuated, and the vessels removed; and the fever immediately ceased.”² Of course, the contagionists attributed this salutary result to the removal of the vessels. But the effect is the same in places where no vessels exist.

The same plan was adopted in New York, in 1819 and 1822, with the most satisfactory results. Of the epidemic of the former of these years, we learn that it made its appearance about the beginning of September, that several cases occurred in the vicinity of Old Slip, and that the fever began to extend to Pearl, Water, and Front Streets. Immediate measures were adopted to clear the sickly districts of its inhabitants, and to fence up the avenues which led to the seat of the infection. These measures, together with the lateness of the season in which the disorder made its appearance, were, as the committee of the Board of Health remark, the probable cause that fewer cases occurred than at any former period, when the city had been visited by a similar malady.³

Dr. Watts, in his account of that epidemic, remarks in relation to this measure:—

“Had the inhabitants been allowed to remain, and no measures of cleanliness been resorted to, the district, it is probable, would have been a scene of the most dreadful pestilence, and, in addition to the number of its immediate residents, who probably would have sunk under it, the growing influence of its destructive atmosphere could have been felt at a much greater distance, and the atmospheric poison would have been confounded in the public mind, with the extension of the disease by *contagion*” (pp. 357–8).

In 1822, the fever having broken out at the foot of Rector Street, that part of the city where it prevailed was immediately and totally cleared of its inhabitants, and those who would not remove of their own accord, were turned out of their houses by force—the streets and lanes which ran into this section of the town were stopped, and the whole neighbourhood was completely deserted. What was the result? Instead of a mortality of some thousands, as occurred in 1798, when the mass of the population remained at their homes, there occurred that season some 400 cases and about 240 deaths.⁴

As already mentioned, Philadelphia once more saw a return of the fever in 1820. On that occasion, the plan referred to was more effectually put into operation. The infected district at the foot of Race Street was cleared

¹ Folwell, p. 17.

² Bowen, Med. and Philos. Reg., iv. 333–4.

³ Hist. of the Proceedings of the Board of Health of New York, in 1832, p. 5.

⁴ Proceedings of the Board of Health, pp. 37, 77, &c.; Townsend, pp. 220–21.

of its inhabitants—sick and well—the former being sent to the City Hospital, or to any place they or their friends pleased. The disease was put a stop to, when it again appeared at the foot of Walnut Street: the same course was pursued there. Barricades were erected across the streets and ingress to the district was as carefully prevented as possible. Here, again, the success was gratifying. In 1793, when a different plan was pursued, and in 1798, when the evacuation of the district was resisted by the people and never fully enforced, the number of cases was enormous and the mortality frightful; while in 1820, the disease was instantaneously put a stop to the number of cases reported during the whole season being only 125, and that of deaths 83.

The same means, and with the same results, have been resorted to during some of the later epidemics of Europe. Emigration everywhere has been found the true preventive to the extension of the fever.

Dr. Chervin has called attention particularly to the beneficial effect obtained from this measure in various parts of Spain, Cadiz, Malaga, Tortosa, Asco, Méquinanza, Palma, Valdemosa, Barcelona, Port du Passage, &c., in all of which, while the fugitives were exempt from the fever, the remaining portion of the population were carried off in great numbers.¹ On the outbreak of the fever at Gibraltar, in 1828, removal of the troops from the place was decided upon. As soon as it appeared in the 12th regiment, the latter was immediately encamped on the Neutral Ground, and not a single fresh case occurred until the regiment again resumed night duties in the town; that is to say, “it was from the 25th of September, when the soldiers came to breathe the pestilential exhalations in their stationary sources, that the admissions became numerous, and the increase of the disease most alarming.”² The disease having broken out early in the 23d regiment quartered in Rosia Barracks, this corps was encamped at Europa Flats, after which not a single case occurred among the men who never left the camp. Dr. Amiel, just cited, remarks in another place (p. 266), that the most signal success followed, in preceding epidemics, the removal from the impure atmosphere of the Rock of those who appeared most susceptible of the fever. This was done in 1813 with many thousand inhabitants, in 1814 with the sickly regiments, and in 1828 with the whole population of the 13th and 24th districts. “In 1814, the regiment of Dillon, which had arrived in the month of May, was quartered, at the breaking out of the epidemic, in the Blue Barrack, near the Moorish Castle. A great number of men took the fever, and soon died; in consequence of which the regiment was encamped on the Neutral Ground, and immediately the fever stopped. The 8th battalion, 60th regiment, arrived from Cadiz in August of the same year, and encamped on the governor’s meadow in a healthy state. Shortly afterwards, they went into the Cooperage Barracks in town; the fever soon broke out amongst them, and both the officers and men suf-

¹ Lettre à M. Lassis, p. 13. Paris, 1829.

² Amiel, Edinb. Med. and Surg. Journ., xxxv. 275; Smith, ib., p. 18.

fered severely. They were sent back to the encampment, and the disease instantly ceased as if by magic" (*Ib.*, note).

In tropical climates, the advice to all the unacclimatized is to fly from the infected towns or locality, and to seek shelter in the country; and if sickness breaks out among the troops, they are ordered off to some salubrious spot. Dr. John Davy, in his notes to Dr. Blair's work on the fever of Demerara, remarks on this subject: "When yellow fever makes its appearance in a particular spot, removal from that spot is the true measure of security; sometimes removal to a short distance; or a very few hundred yards, would appear to be sufficient; but, if it be practicable, removal to a greater distance is desirable, and to ground in its character different from that where the disease originated. Let experience in Barbadoes prove the propriety of this. When yellow fever broke out in the 88th regiment, occupying the lower barracks in the garrison of St. Ann, the encamping of them on ground close to the higher barracks, in the same garrison occupied by the 7th Royal Fusileers, who were then free from fever, and altogether escaped it, was successful. The same result occurred for a time, when the disease appeared shortly after in the 66th regiment, who followed the 88th in the lower barracks; their removal to the higher ground at first appeared to be successful, but only for awhile; ere long the fever made its appearance in the upper barracks then occupied by the 72d regiment, and, indeed, appeared more or less throughout the garrison. With the experience I now have," Dr. Davy continues, "and the strong conviction in consequence, I am satisfied that in the instance of this outbreak of fever, whether in the 88th, 66th, or 72d, the recommendation on the part of the medical officer, when consulted by the general officer commanding, would have been best given in conformity with the above, to remove the regiment attacked, not from one part of the garrison to another, but to a greater distance, and to a situation altogether different from that where the disease had made its appearance."¹

Now, I would ask, what do these facts teach, unless it be that the disease, in infected cities, or in the infected portion of a city, arises from a cause located therein?—but certainly that cause cannot be of a contagious character, and consist of an effluvium formed in and exhaling from the bodies of the sick, for that cause continues to exercise its morbid influence in the deserted locality long after the removal of its inhabitants, sick or well—much longer indeed than could be accounted for on the supposition of the disease being contagious. Besides, were the disease of that sort, and were the stop put to the progress of an epidemic due simply to the avoidance, on the part of those who are removed from an infected locality, of a contagious effluvium, it would be difficult to account for the circumstance, that while the sick who are sent to some hospital, or received in private houses, and the well who are turned adrift, or provided with quarters in the suburbs or in the open country, leave a sufficient amount of contagion behind to communicate the disease to those who visit the deserted streets or buildings, they invariably

¹ Blair, p. 59, note.

fail to carry along with them enough of the poison to enable them in a single instance to extend its baneful effects in their new habitations, whatever may be the hygienic condition of these. If it be most probable that the well who escape to avoid the effects of a contagious virus issuing from the sick, would have been seized had they continued to mix with these in the infected localities, how comes it they do not become affected in their intercourse with them after they leave those localities, during their transient visit to, or their residence in, their new habitations? Let it be remarked, that in order that the atmosphere of a locality should be contaminated by the effluvia from the sick, to a sufficient extent to affect a large portion of those who visit it, and to remain in that impure state long after that locality has been cleared of all living things, it would be necessary, that the number of cases should have been very large, and the virulence of the contagion very great; and yet we know that the cases at the commencement and at the usual period of such evacuations are few in number, and occur in succession. If, from a limited number of cases, there could exhale a poison sufficiently virulent and concentrated to produce so great and lasting an effect, we might naturally expect to find those same cases, or others proceeding from the same spot, communicating the disease in other parts of the city, or wherever they may be removed.

Those who visit an infected locality take the disease, though the sick have been removed.—More likely is it that the cause, from the effect of which the inhabitants of infected localities escape by emigration, consists in a poison existing there, but in no way dependent on anything evolved from the bodies of the sick—incapable of being carried by them, or in any other way, from place to place, and remaining behind after the locality so affected has been cleared and fenced up. That the cause is not removed together with the people thus ejected from their residences—that the disease must be due to an agent of a non-contagious character—will be admitted by those who bear in mind that long after the clearing and closing up of infected spots, the disease is taken by individuals who venture in them; though no sick remain to convey the contagion; though the place has been converted into a perfect solitude, and though the intruders enter no house or place capable of retaining the contagious poison. Often has the effect been produced in those who simply walked along the deserted streets. This has been noticed in all epidemics, during the continuance of which a transient visit to the place has been sufficient to occasion an attack in persons who neither look into a house nor hold communication with any one, sick or well. Musgrave says on this subject: “Many Europeans who arrived here towards the decline of the epidemic, when not one case was to be found in town, and who never entered a house in which one had previously been ill, were almost immediately attacked. Was this to be ascribed to contagion, or to some other latent cause to be sought for in the general atmosphere they inhaled?”¹ I could, if necessary, relate several instances of individuals who were seized with yellow fever after

¹ Med.-Chir. Trans., ix. 116.

remaining but an hour or two in the Havana, or merely passing through to embark.

The facts on the subject mentioned by Humboldt respecting persons taking the disease in consequence of passing through Vera Cruz in a litter, are familiar to most readers. "It may perhaps be supposed," remarks the great traveller, "that in such cases the disease has been taken on board of the vessel which, having sojourned some time in the port of Vera Cruz, contains deleterious miasmata; but the celerity of the infection is more incontestably proved by the frequent examples of rich Europeans, who have died of the *romito*, although on arriving at the mole of Vera Cruz they found litters prepared to take them at once to Perote. These facts," he continues, "appear at first sight to militate in favour of the doctrine of contagion. But how can we conceive of a disease being communicated at considerable distances—which, in Vera Cruz, is decidedly not contagious by immediate contact. Is it not easier to admit that the atmosphere of Vera Cruz contains putrid exhalations, which, by being inhaled during the shortest space of time, disorder the vital functions?" (p. 774).

At Gibraltar, in 1828, "the mass of those attacked merely marched to their guard-houses, to which, certainly, the inhabitants were not in the habit of resorting, and where the soldiers, as certainly, came in contact with no persons suffering from yellow fever. Arrived at his post, a soldier was placed as sentry, not in the midst of a dense population—not with people about him from whom disease might be transmitted; nothing of this occurred. The guard in charge of the ruins of Pompeii is not in the midst of a deeper solitude than were often the soldiers at Gibraltar, as they inhaled the 'death blast' in districts abandoned by the inhabitants. The men were far removed from contact with sick people—far out of hearing—far out of sight."¹ Mr. Amiel confirms this statement: "I may add," he says, "that when so many new cases occurred in the regiment after it had resumed the town duties, the men composing the guards were marched directly from the Neutral Ground to the guard-houses, mostly by the line-wall, and avoiding the streets as far as possible; those in guard-houses had no communication whatever with any description of inhabitants, and still less with the sick, or articles belonging to them; they were marched to and from the posts of the guards without being allowed any improper intercourse, and ultimately they went back to the Neutral Ground with the same precautions, and in the same orderly manner as they had come from it; thus rendering it impossible to trace, to any contagious sources, the malady which unexpectedly manifested itself in many of them but a few days after they had been employed on that duty."²

The history of our epidemics furnishes us with examples of the sort. During the fever of this city, in 1820, a young man, residing in a healthy district, jumped over the barricades, walked through the deserted streets for a few moments, and soon after paid the penalty of his imprudence. Watchmen sta-

¹ Gillkrest, Hist. of Epid. of Gibraltar in 1828; Second Report on Quarantine, p. 172.

² Edinb. Med. and Surg. Journ., xxxv. 276.

tioned in the cleared district to guard the houses and stores, have been known to be attacked with the disease, having necessarily seen no one likely to communicate it. In New York, in 1822, similar facts were noticed (*Townsend*, p. 221). At Franklin (La.), in 1839, "Dr. Horsby affirms that he knew of persons who came in from the country, and only walked the streets without entering a house, who were seized after their return home."¹

Speaking of Fort Hudson, on the Mississippi, Dr. Drake says that Dr. Beaumont (Ths.), who resided near the village, informed him there were more than twenty well-marked cases there in 1841. On the 12th of October, before any case had occurred, a gentleman from the neighbourhood rode through the town, and, without dismounting, lingered in it about three quarters of an hour; on the night of the 13th he was seized with the fever" (*Ib.*, p. 252).

Here, surely, we have a series of facts presenting but a very distant relationship to those which might be expected to occur were the disease endowed with contagious properties, and thereby rendered communicable from place to place. We find the fever arrested in its epidemic career, not by enclosing the inhabitants in an infected city or locality, with a view of preventing them from spreading the contagion elsewhere, but by evacuating such localities, preventing ingress to them, and enforcing the interdiction till the accession of frost or of some other purifying agency. Those facts teach us that the inhabitants of those infected localities, when thus turned out, do not communicate the disease to each other, or to any one around, in their new abodes—be these near or far, high or low, clean or dirty. Again, those facts demonstrate that, during the continuance of an epidemic, persons who visit the infected place imbibe the seeds of the disease, although they may not have seen, touched, or in any way communicated with the sick, or entered a house where any one affected was or had been confined. Finally, they prove that the same effect is produced even in instances when the localities infected have been completely cleared of their inhabitants, and when, therefore, there is no one from whom the contagion can be evolved and communicated. Whatever may be the views entertained with regard to the former of those facts—admitting for a moment that the non-communication of the disease out of the infected place, by the emigrating population, is explainable on some other principle—we must be suspicious of the existence of contagion when we find those who visit such localities attacked shortly after, although not having entered a house or communicated in any way—direct or indirect—with the sick. Contagious diseases do not comport themselves in that manner. Twenty or thirty or fifty cases of smallpox, or measles, or scarlet fever, or typhus fever, would not contaminate the atmosphere of the streets in such a way as to affect individuals who pass through them without entering the houses or rooms where the sick lay. And if such is the result in respect to those disorders, is it not likely that a contrary effect will be observed when the prevailing disease happens to be the yellow fever? Still less can we

¹ Drake, ii. 241; see p. 252.

subscribe to the opinion of the existence of contagion when we find that the disease is communicated to individuals who visit infected localities after these have been cleared completely of their inmates. The annals of no contagious disease furnish facts affording reasons to admit the possibility of the occurrence. The belief does not rest on even a plausible analogy. Granting that the street atmosphere could by any means be contaminated by effluvia issuing from the bodies of the sick, and remain so contaminated a few days after the removal of these, certainly the same thing could not hold during weeks and months. The contagious effluvia not being renewed, the atmosphere would soon be rendered pure and innocuous.

The impossibility of the occurrence, and the absence of danger from contagious effluvia to those who did not visit the sick, had not escaped the attention of some of our early contagionists. The second mode of receiving the infection, we are told by Dr. Cathrall, "was from the matter of contagion arising from the morbid body impregnating the atmosphere of the chamber, and being applied to susceptible constitutions. This, I believe, can only take place within a certain small distance of the person affected, for in proportion as it becomes diffused in the atmosphere, it must lose its activity, and be so far exhausted as not to be able to produce the disease. Therefore people in walking the streets are secure from being contaminated by the atmosphere which surrounds them; for although a number of articles, charged, as it were, with the matter of contagion, be exposed in the open air, yet the effluvia issuing from them would be so much diffused, corrected, and modified, through that immense space, as scarcely to be able to have any effect, except within the contagious atmosphere, extending but a small distance from the articles themselves. The danger of going abroad in the city, during the prevalence of a contagious disease, is from coming in contact with the clothes of those who had recently left the chambers of the sick" (pp. 11, 12).

With an object different from mine, and long before he had learned that the danger incurred by those who entered the infected localities was as great after as before the clearing of these, another writer of this city, Dr. Currie, remarked: "To suppose that the air of the streets can be contaminated by the contagion so as to communicate the disease through that medium, is contrary to, and contradicted by, the observation and experience of the learned of the faculty of every age or country; and any apprehension of an infectious contagious disease from that source can only be excused in those who have not had suitable opportunities of information." "People in walking the streets are by no means in any danger of infection (contagion), as the miasmata or contagious exhalations from the bodies of the diseased have never been known to be conveyed by the air many feet beyond the chamber of the sick, except by means of clothes or other porous substances which have come in contact with, or very near to, the body of one labouring under the disease."¹ If such is the case before the localities have been cleared, how much more impossible must be the occurrence after the inhabitants have been removed. The com-

¹ See also Fever of 1798, pp. 67, 68; Folwell, p. 72.

munication of the fever, in such instances, indicates, therefore, the existence of a cause completely independent of any poison emanating from the sick, unless we admit, with some more modern advocates of the doctrine of contagion, that the disease possesses a virulence and degree of diffusibility far exceeding that of smallpox or any other known contagion; that the arrival in any place of one solitary individual labouring under or convalescing from the disease, or whose clothes have been contaminated by exposure to an infected spot, or of a trunk of clothes, or a box of merchandise, is sufficient to affect a whole neighbourhood; that the poison spreads to the distance of several hundred feet; and that it may pass through stone walls, penetrate the various cells of a jail, the distant wards of a hospital, or the several stories of a large house.

If the poison of yellow fever is so virulent and diffusive that a few patients can contaminate the atmosphere of a large locality to such a degree that individuals who visit that locality, without approaching the sick, or entering a house, are almost sure to be attacked, how does it happen that such a poison does not produce its deleterious effects on the entire families in which cases occur? And yet, so far from this occurring, it is found that the number of individuals attacked in each house is often very circumscribed. The remarks of Dr. Miller, in allusion to the epidemic of New York in 1805, will apply to many others: "The proportion of single cases," says that judicious writer, "in the midst of families, is always great, and the instances of any large proportion of families being attacked were comparatively rare in our late epidemic. It appears, from the records of this epidemic, that there were thirty-one streets of the city, most of which continued to be filled with inhabitants through the whole season, in which only a single case in each occurred; and in the mass of six hundred cases reported by the Board of Health, there were only thirty-five houses in which more than a single case was found. If the number of deaths should be supposed to afford better ground of calculation, it will be found that there were forty streets, and these generally crowded throughout the season, in which only one death in each took place; not more than three died in any one house, of which there were only two instances; and, during the whole epidemic, there were only twelve instances of two persons dying in one house." Dr. Miller adds, in a note, that, from those reported to the Board of Health, it results that upwards of five hundred out of six hundred cases of malignant fever which occurred were single in the respective families, and that more than three-fourths of the deaths which took place in the city were likewise single in the respective families in which they occurred.

The same fact has been remarked elsewhere. Dr. W. Humboldt, speaking of the yellow fever of Mexico, says that if the disease breaks out in the midst of a family, it attacks only two or three members; the rest escape it, even those who give constant attention to the sick. "When," Dr. H. continues, "it happens—as is sometimes the case—that many individuals of the same family are attacked, it is found, on examination, either that the disease is general in the locality which the family inhabits, or that the individuals attacked had

been in a locality in which the disease prevailed. The numerous examples of two or three persons or more being attacked in the same house, at the same time, and in the same hour, and that general susceptibility which pervades all ranks, make it appear *not* that the disease has the property of spreading from one person to another, but rather that it is the product of a general cause, to the influence of which they have been simultaneously exposed."¹

It must be confessed, that a contagious febrile disease, the majority of whose victims had never approached the sick, or entered an infected house, and which, at the same time, spares the greater number of those exposed to the direct action of its poison by nursing and visiting individuals labouring under it, or living under the same roof, is of a somewhat anomalous nature.

The Local Origin of the Disease proved by Sporadic Cases.—The occurrence of sporadic cases of yellow fever in any place may with propriety be regarded as a valid reason in favour of the doctrine of local origin and of the non-contagious character of the disease. Need it be stated that instances of the kind in various cities and towns subject to the disease have been noted time immemorial, that in some places they occur almost annually between from periods of epidemic, and that in certain localities, where the fever has never or seldom assumed the epidemic form, sporadic cases are not unknown? Such cases, have, on various occasions, been observed among us at the period of the year when the fever usually prevails, and under the hygrometrical and thermometrical conditions required for its production and prevalence elsewhere. That the occurrence of sporadic attacks has been denied, that the disease reported on those occasions has been viewed by some as of a nature distinct from that of true yellow fever; and that those instances, as to the character of which there could be no doubt, have been attributed to a foreign source, is well known. But whatever may be the degree of regard to which those who entertain such sentiments are entitled, I cannot for a moment doubt that on those points they err; that sporadic cases of true yellow fever do occasionally show themselves; and that they have done so under circumstances which forbid the possibility of attributing them to any but home causes. Dr. Rush, writing in 1802, remarks in his letter to Dr. Miller, that he had seen one or more of them almost every year since he settled in Philadelphia, and particularly when his business was confined chiefly to that class of people "who live near the wharves and in the suburbs, and who are still the first, and frequently the only victims of the disease."² There may be, and probably is, some exaggeration in this statement relative to the frequency of cases of the kind, arising from the particular views entertained by the illustrious physician who, like some others, refused to recognize a difference between genuine yellow fever and the aggravated and malignant forms of remittent bilious fever. But after making due allowance for any error of diagnosis on the subject, facts of undoubted authenticity will bear us out in the statement of the occasional

¹ Rep. of Sanit. Commiss. of New Orleans on Epid. of 1853, p. 130.

² Yellow Fever not Contagious, Med. Repos., vi. 164; Works, iv. 162.

occurrence of sporadic yellow fever in this city. Such was the case in 1795, in 1796,¹ in 1800, 1801, 1806, and 1809.

In 1826, a few well marked cases terminating fatally with black vomit and other characteristic symptoms of the disease, were reported by competent physicians, and were witnessed by myself, and others conversant with the disease. In the same category must doubtless be placed the cases which occurred here in the summer and autumn of 1854. Those reported amounted to about thirty. As regards their local origin and their non-contagiousness, there could be no difference of opinion. The genuineness of the cases recorded in this city is not solely established on the authority of those who have witnessed them, on the phenomena they presented, and the circumstances under which they appeared. It may be inferred from the fact of the frequent occurrence of similar instances in other places, of both temperate and tropical regions, where the fever often prevails epidemically and is well understood.

Professor Dickson, of Charleston, remarks, that the cause of yellow fever, speaking of it as a unit, although it may consist of many combined influences, is in perpetual existence in the summer atmosphere of Charleston. "It is common," he says, "to talk of intervals between the several incursions of yellow fever. For myself I am fully satisfied that there are no such intervals, and believe that no single summer passes over the inhabitants of that city without offering cases of greater or less intensity."² Other authorities, besides Dr. D., speak of its frequent occurrence sporadically in Charleston.³ In Mobile, as we learn from Dr. Nott, not a year passes without the occurrence of sporadic cases.⁴ From Dr. Lewis we learn that, in 1821, only seven cases of yellow fever occurred in that town. "In 1822, Mobile was very healthy; only four or five cases of yellow fever, and very little disease of any kind," though it was epidemic in New Orleans. During the first two weeks of September, 1824, there were six fatal cases of yellow fever.⁵ In 1838, there were a few cases (*Drake*, ii. 221). So also in 1841 (p. 222).

In New Orleans, instances of sporadic development of the fever are not less frequent. Cases of the kind occurred in 1821 (*Thomas*, p. 63), as also in 1826, when the number sent to the Charity Hospital amounted to only 23. The year before, the number did not exceed 92; consequently the disease was scarcely epidemic. In 1836, the records of the hospital show 7; and Dr. Harrison says there were a very few cases in the city. The same writer states that in 1838 there were likewise a few, and 24 cases were received in the hospital. In 1845, the books of the hospital present but one case, which proved fatal. The bills of mortality gave also but one death; but a few occurred in private practice.⁶ Dr. Dowler states that he could offer a list of

¹ Works, iii. 242-3; iv. 63, 69, 102.

² Dickson, *Bell's Eclectic Journ.*, ii. 111.

³ Ramsay, ii. 87; Tucker, *Barton's Journ.*, ii. 24; Shecut, p. 102; Simon's Address, p. 14.

⁴ N. O. J., March, 1848, i. 565.

⁵ Lewis, *Sketch of the Yellow Fever of Mobile*, N. O. J., i. 286-8.

⁶ Harrison, N. O. J., Sept. 1845, p. 131; *Drake*, ii. 207, 211.

yellow fever cases admitted into the Charity Hospital for every year since its records began, taken after a most careful examination of the same, not excepting that most salubrious year 1821 (p. 22). Dr. Girardin regards sporadic cases as of annual occurrence, and describes minutely the form which the disease assumes generally under those circumstances (p. 28). "Until within three years past," says Dr. Hort—writing in 1845—"during which time the fever has been rather sporadic than epidemic, the yellow fever prevailed with great regularity every other year; but there were always sporadic cases in the intermediate years."¹

Writers on the disease as it has appeared in Spain, recognize the existence of such sporadic cases in several cities of that country. Arejula, Moreno, and Pariset, among the contagionists, admit them freely; Piguilem, Salva, and other men of note, make the same statement. Speaking of Gibraltar, Dr. Hennen remarks that cases of genuine yellow fever (as described by writers on diseases of the West Indies), accompanied by the true black vomit, occur in the City Hospital and private practice every season (p. 423).

Dr. Burnett, also, while stating that the occurrence of black vomiting in the epidemic of Gibraltar has not been so frequent a symptom as Sir William Pym would wish it to be believed, affirms that it has repeatedly taken place in individual cases of fever when the garrison was otherwise considered healthy. On the authority of Dr. T. Smith (*Edinb. Journ.*, xxxv. 38), it may be stated, that several cases of well marked yellow fever occurred in 1829 and 1830—in the latter of which years he saw three in the garrison—two of them terminating with black vomit. Of 39 cases taken from the records of the Civil Hospital, and examined carefully in 1829 by a commission of nine physicians competent to the task, all but one were decided to have been identical with cases of the epidemic fever which had existed in the garrison during the latter part of the year preceding.²

Louis informs us, that of 45 cases represented to the French medical commission from the register of the hospital, as instances of sporadic yellow fever—and 19 of whom ended fatally—some were evidently of that nature, while others were of a different character. But as Louis's denial of the yellow fever nature of several of the cases that ended fatally is based, in great measure, on the fact that the liver is not described as presenting the appearance he regards as the true criterion of the disease, a fact to which there may be and are exceptions, and as the symptoms which these cases, as well as those that recovered, presented, induced Mr. Amiel, Mr. Frazer, and others, who had more experience in the disease, and were entitled to more confidence in the matter in question than Louis, to adopt a different sentiment, we may presume that the number of cases was larger than he represents them to have been. But however this may be, we have from his own showing the proof

¹ See Rept. on Quarantine by Com. of N. York Legislature, p. 197.

² Dr. Gillice, Dow, G. Brown, A. Brown, Miller, Gillkrest, Amiel, H. Frazer, and Chervin; see certificate on the subject under date of 13th April, 1839, in Chervin's *Réponse à M. Guyon*, p. 226; see also Second Quarantine Report, pp. 18, 19.

that the yellow fever occasionally shows itself sporadically in Gibraltar. Dr. Gillkrest says he is in possession of such a body of evidence, drawn from registers and other authentic sources at Gibraltar, as would of itself place the matter beyond all doubt (p. 270). After speaking of the results obtained by the above-mentioned commission, he appeals to the further testimony of Dr. Gray, formerly physician to the Gibraltar Naval Hospital,¹ of staff-surgeon Glasse (Burnett), of Dr. Hennen, and the medical officers of the Ordnance, the 12th, 23d, 43d, 64th, and 94th regiments.²

We find likewise in Roberts's *Guide Sanitaire des Gouvernemens Européans* (p. 105), an account of the eleven sporadic cases of the disease which occurred at Marseilles in 1811, during a condition of atmosphere and an elevation of temperature analogous to those accompanying or preceding the development of the disease elsewhere.

The annual appearance of sporadic cases in most, if not all the West India Islands, is perfectly well known to every medical reader—too well, indeed, to need being dwelt upon in this place in any detail. Suffice it to remark that Pouppé Desportes, in his account of the diseases of St. Domingo, points out their occurrence in all the seasons, during his residence in that island, in which the disease did not assume the epidemic form. Such was the case in the years 1739, 1742, 1743, 1745, 1746.³ Since his time, instances of the kind have been noted by all writers on the fevers of southern latitudes.⁴

From a review of the circumstances under which these sporadic attacks of yellow fever occur, it will be seen that those who attribute them to the operation of local or domestic causes on systems peculiarly predisposed, cannot be far from the truth. They present themselves at the seasons of the year when the disease usually makes its appearance and prevails epidemically in countries subject to its incursions, and in some where many contagionists themselves acknowledge that it is due to local causes. They arise during and after hygrometrical, electrical, and thermometrical conditions of atmosphere which are well known to favour the evolution of febrile poison from local causes, and to give rise, by themselves, to the disease. In several instances, they have been evidently traced to such causes, and in none can they be referred to importation from distant places. This was certainly the case in this city, where, in 1801, 1826, and 1854, the most persevering contagionists could not find the smallest chance of deriving the disease from abroad. Such also was the case in Gibraltar, in Marseilles, and such is the case almost annually in Charleston, in others of our southern cities and in the West Indies, where the local origin of sporadic attacks is universally held as placed be-

¹ Lond. Med. Reposit., Nov. 1817, p. 417.

² Cycl. of Pract. Med., ii. 270.

³ Vol. i. pp. 92, 121, 131, 140, 161, 164, 174.

⁴ Ferguson, Med.-Chir. Tr., viii. 144; Musgrave, Med.-Chir. J. and R., iv. 981; Moreau de Jonnes, p. 117; Bancroft, p. 7; Dariste, p. 31; Lefort, p. 11; Savarésy, pp. 57, 65, 77, &c.; Wilson, p. 116; Humboldt, p. 761; Chervin, Rept. on Rufz, p. 6; Arbuckle, Gavin, Watson in Second Quarantine Report, p. 20; W. Humboldt, Rept. of Sanit. Com. of N. O., 1853, p. 130; Lacombe, Ib., p. 147; Pennel, Ib., p. 151; Sigault, Clim. et Mal. du Brésil, p. 257; Boyle, Med. Hist. Account of Western Coast of Africa, 204.

yond the possibility of doubt. Now, if sporadic attacks of the yellow fever are admitted to be the result of domestic causes, and to arise independently of importation and contagion, it is difficult to understand on what grounds the disease is denied to be due to the same causes, and why we should attribute it to a foreign source, in those instances where, instead of appearing to a limited extent, it spreads over a broader surface, and, assuming the garb of an epidemic, attacks a large number of individuals. It cannot be hazarding too much to say that in both instances the cause must be the same. If the fever arise, when sporadic, from the impress of a poison evolved from local sources of infection, it must be due to the same poison obtained from similar sources, when epidemic; and if in the one instance it is limited in its range, and in the other it attacks a great number in rapid succession, the difference must depend on the greater energy and diffusion of the aforesaid poison, and on its development during one of those peculiar constitutions of atmosphere which favour the spread of all diseases arising from agents of kindred nature.

By some writers, the occurrence of sporadic cases, as indeed of some epidemics, has been ascribed to the revivification under the influence of peculiar conditions of weather, of a contagious poison which had remained latent or dormant, either in the system, or attached to surrounding objects, from some preceding epidemic. In this way it was that Dr. Hosack accounted for the cases which occurred in New York in the autumn of 1799, prior to the arrival of the ship *General Wayne*, to which, however, the disease when it spread was subsequently ascribed. So far did he carry his belief in this hypothesis that he thought it not improbable that the poison might remain dormant not *one* year only but *two*, and break out after that period, notwithstanding the most rigid quarantine that might have been instituted to guard against its introduction.¹ The same doctrine, which met favour among some of the physicians of that city in regard to the cases of fever that appeared there in 1800, has proved particularly acceptable to several of the European writers who have described the epidemics of the Spanish peninsula.

Sir W. Pym adduces the opinion of Mr. Vance to the effect that the contagion of the epidemic of 1820 at Carthage had remained dormant over the winter to revive and produce the one which he observed there the next summer (p. 64). "The physicians," says Mr. Vance, in a report to Sir William, "cannot account for the disease making its appearance at Carthage; but I have good reason to suppose it might have remained dormant during the winter, till roused into action again by the summer's heat; and I am the more disposed to favour this opinion from the circumstance of their using so little precaution last year, neither destroying the bedding, clothes, and other furniture, of any of the people who died of the disease; as they had done in 1804, &c., (*Pym on Bulam Fever*, p. 64). Sir J. Fellowes has ascribed to a similar dormancy and revivification the epidemic which occurred with such violence at Malaga in 1804, and for the production of which he could not, even after

¹ Letter to Dr. Currie, in the latter's *Account of Fever of 1799*, p. 95-6.

personal inquiries, find any other cause.¹ "This," says F., "is confirmed in Dr. Mendozas's work on the subject" (p. 177). Other physicians—Spanish and French—have adopted similar views, and if we rely on their statements, we shall have no difficulty in accounting for the occurrence of the disease, on some occasions, at Seville, Medina Sidonia, Malaga, Mureia, Xeres, Cadiz, Puerto, &c.²

This doctrine is certainly convenient; for when contagionists and importationists are at a loss, from want of proof, to account for the appearance of the disease in any place where it has existed before, they may have recourse to the facilities afforded by it, and insist on the revivification of dormant contagious germs. But even were we disposed to admit the possibility of the poison remaining dormant for so long a time in the system—a subject upon which enough has been said in a former chapter—or in surrounding objects, the theory cannot be applicable to some of the cases to which I have referred. The sporadic cases recorded in Marseilles in 1811, had not been preceded, for years before, by any disease of the kind, and could not, therefore, have been due to the development of a latent contagion. A similar objection is applicable to some of the cases which occurred in this city. In other instances, the intervals are too long to suppose the germs could have thus lain dormant, and resumed their virulence when again called into play. From the inadmissibility of such a theory in such instances, we find reason to doubt the propriety of resorting to it in others in which the intervals are not so protracted.

The theory is objectionable on other grounds, even when the disease appears sporadically, or otherwise, the summer following an epidemic. The fifty or more deaths which occurred in New York in 1800, were, as just seen, accounted for by some, by this awakening of the residual fomites of the last year's epidemic. "If this had been the case," says an intelligent writer, "we should expect to find the disease reappearing in the same houses and families where it had raged last year. But, on a comparison of the reports of the present year with those of the last, as far as they go, only a single instance of such reappearance in the same house is found, and this attended with circumstances which prohibit every suspicion of dormant contagion."³ Again: if the disease can be reproduced in this way, why is it not so more frequently. In many places, such revivifications would not seem capable of being effected, at least as a general rule; for sporadic cases do not manifest themselves during seasons following a wide-spreading epidemic.

Whether the occurrence of sporadic cases is to be ascribed to the development of a poison laying dormant from some antecedent period of sickness, or

¹ Fellowes, p. 177; Bancroft, Seq., p. 115.

² Pariset, Fever of Cadiz in 1819, pt. iv. pp. 81, 95, 106; *Ib.*, Rept. on Fever of Barcelona, pp. 95, 96, 97, 559; Arejula, p. 448; Mellano, Hist. de la Epid. padecida en Cadiz, &c., p. 65; *ib.*, Periodico de la Soc. Med. Chirurg. de Cadiz, i. 287. See same Work, p. 23. Sola, cited by O'Halloran, p. 163; see Chervin, Mat. Sanit., p. 23; Bally, p. 82.

³ Medical Reposit., iv. 208.

whether, as is more likely, it is the result of the recent evolution out of local and domestic materials of the yellow fever cause—whether, I repeat, we adopt the one or other of these opinions, it results, from all the facts I have collected, that the disease, when sporadic, is not propagated by contagion; while its limited diffusion on those occasions may safely be referred simply to the circumstance that the cause does not exist in sufficient force, or to a sufficient amount to spread widely, and is besides unassisted by an epidemic constitution of atmosphere necessary to enable it to affect more those individuals most susceptible to its morbid impression. Add to this, that in tropical climates, the sporadic character of the disease is due not merely to the causes specified, but often to the small number of unacclimatized individuals present in the place.

I am aware that by Louis and others, it has been said that the admission of the occurrence of such sporadic cases will not settle the question of the mode of communication of the disease, and justify a belief in its non-contagiousness; “for we see every day the most incontestably contagious diseases—as, for example, the smallpox—under sporadic forms, and appearing only at distant intervals of time.” But, whatever may be the doubts of those writers, facts will, I think, justify a different conclusion. Authors have everywhere acknowledged the non-communicability of the disease when it appears sporadically. Robert himself, though a most zealous and uncompromising contagionist, admits that none of the cases which appeared at Marseilles communicated the disease to individuals around them (p. 101). Were it otherwise—were the disease really contagious—there is no reason why it should limit itself to a small number of individuals, not one of whom can trace their attack to exposure to the pretended source of contagion. It would necessarily spread around, particularly as no precaution is usually taken against such a result, and a large majority of those exposed to the effluvia from the sick are, except in the West Indies and our southern States, unprotected against infection. And yet we find the disease attacking a few individuals who have not been exposed to any source of contagion, and sparing all who approach the sick.

Indeed, if the yellow fever were really contagious, the occurrence of sporadic cases one year and an epidemic the next would not take place, because each case of the former category would become a focus of contagion (infection), and the disease would spread more or less extensively. The admission of the contrary result would imply that the disease might be contagious one year and non-contagious on another occasion. The idea cannot be entertained. The existence of the disease in the sporadic form necessarily calls for the admission that in those cases it was not contagious; and if such is true with respect to them, it must be equally so with regard to those which occur in times of epidemics. That truly contagious diseases—the smallpox among them—sometimes appear sporadically, is certainly true; but it is equally so that the cases that occur do not usually do so independent of each other. Most usually they originate from an ascertained intercourse with an individual affected with the disease; every succeeding case can be distinctly traced to a direct or indirect exposure with the sick; and if it does not spread

in such a way as to assume the epidemic form, the effect is due to the fact that the larger number of those who approach the sick are protected by a previous attack, or, in the case of smallpox, by vaccination, as well as to the care taken to separate the unprotected from the source of the contagion.

If they do occur without exposure, their origin may be a mystery difficult to solve. But, whatever explanation we may prefer, such cases do not arise from any ascertained morbid agency—certainly not from a poison evolved from domestic causes of any kind. In other words, such diseases arise from their own specific contagious poison; and we may in strict logic conclude that, when they appear sporadically, the few cases thus noticed are offsprings of the same *materies morbi* from which they are ordinarily produced, although we may not be able to ascertain the way in which the poison has reached the sufferers. But, admitting that such communicable diseases may arise spontaneously, without anterior contagion obviously or mysteriously applied, it must be borne in mind that cases so produced become foci of contagion, and that from them other cases may be traced. Very different is it in regard to the yellow fever. In all instances of the occurrence of sporadic cases, the disease springs up without the possibility of its being referred to a foreign source, the several cases occur independently of each other, few of them can be traced to a direct or indirect communication with the sick, and though in many of our cities and in Europe the large majority of those exposed are unprotected, care is seldom taken to prevent intercourse between those in health and such as labour under the disease.

The Fever is more prevalent and of worse Character in some parts of an Infected Place.—It has usually been found, during the epidemic prevalence of the yellow fever in Philadelphia, that the disease not only prevailed more extensively, but assumed a more malignant and fatal character in some parts of the city than in others. This localization and aggravation of the fever is in general observed in localities deficient in free ventilation and cleanliness, and more particularly in the original focus of the infection. To this, however, there are exceptions; for the disease has been known to occur under circumstances of a different kind, and where it would be impossible to attribute the effect to the mere concentration of the contagious poison exhaled from the sick. Be this as it may, for the present; it did not escape observation, in 1793, that in the vicinity of the river (Delaware), along the wharves, and the filthy streets and courts adjoining, the disease assumed its most aggravated form and proved more frequently fatal, and that this severity lessened as the infection progressed westwardly and towards the outward districts.¹ The same phenomenon was particularly noticed in the epidemics of 1802² and 1803, when it was most malignant in Water Street, and became less so as it travelled westward.³ In 1805, we are told that the malignancy and danger of the disease was graduated by its distance from the river, persons residing about Third and Fourth Streets, although attacked when the epi-

¹ Minutes of Proceedings, pp. 127–8; Barnwell, p. 374.

² Ffirth, p. 23.

³ Rush, iv. 84; Caldwell, Med. Repository, vii. 148.

demic was at its height, being in less danger than those who lived in Front and Water Streets.¹ "It is a circumstance well worthy of remark," says Dr. Jackson, in speaking of the epidemic of 1820, "that, in the different quarters where the disease prevailed, one class would be more common than the other. Thus, at Hodge's wharf and Duke Street, nearly all the cases were of the first (most malignant) class; in Letitia Court they were all of the second; and in the other situations, the second and third classes were nearly as numerous as the first. Dr. Monges, whose accurate knowledge of this disease, derived from much experience and long observation, both in the West Indies and this country, is so well known and acknowledged, states that he has noticed the same facts in the different epidemics of our city. In certain situations, where the disease was general, he found it light and easily manageable—all patients recovered; in other situations, it was precisely the reverse—medicine was unavailing, and his patients nearly all died" (pp. 61, 62).

This observation, the value of which cannot be denied, and which, as has well been remarked, is incompatible with the doctrine of contagion, is not peculiar to occurrences in Philadelphia; but has been made in other places visited by the disease. In Boston, during the epidemic of 1802, the fever appeared in its mildest form in Liberty Square, which is on the north side of Fort Hill, and in a more severe form in Purchase Street, the lower part of Summer Street, and a little in Sea and in High Streets.² The fever of 1791 in New York, displayed much more malignancy near Peck's Slip, where it proved more particularly fatal than in any other part of the city. Dr. Addoms, from whom we derive the information, states that that part of the city was thickly inhabited; its houses generally small and badly ventilated; many of the inhabitants being "in indigent circumstances, which is a frequent cause of the want of cleanliness."³ Four years after, in 1795, it raged with most violence in those parts of the town adjacent to the eastern shore, where the air is less pure, and the ground lower than in any other part of the city; and which, consequently must, in some degree, serve as a reservoir for the filth of the upper parts of the city.⁴

In 1798, it commenced in Front Street, near Coenties Slip, then progressed to the New Slip and in John and Cliff Streets, where it assumed a very malignant character and prevailed with great violence.⁵ The next year, the cases were principally confined to Old Slip and the streets in the vicinity of the East River, and raged more severely there than elsewhere. In 1803, the disease broke out at the Coffee-house Slip and its vicinity. "The streets lying near the margin of the river, and some of those in the upper part of the city,

¹ Caldwell, *Fever of 1805*, p. 51.

² *New England Med. Journ.*, viii. 380; *North Amer. Rev.*, x. 396; *Med. Reposit.*, vi. 338.

³ *Inaugural Dissertation*, &c., p. 7.

⁴ Alex. Hosack's *Inaug. Essay*, p. 9; M. L. Davis, *A brief Account*, &c., p. 15; Bayley, *Fever of 1795*, pp. 57, 61, 64; Seaman, *Webster's Collection*, pp. 5, 35; E. H. Smith, *ib.*, pp. 66, 67.

⁵ *Quarantine Rept.*, p. 8; Hardie, *Fever of 1798*, p. 8; *Ib.*, *Fever of 1822*, p. 2.

which were principally inhabited by the indigent, uncleanly, and dissolute, suffered most from the ravages of the disease.¹ During the early part of the epidemic of 1805, nearly all the cases took place on the eastern side of the city, in Front, Water, and Pearl Streets, and principally below Burling Slip. They afterwards became more generally diffused, and soon reached the North River. On the whole, the low grounds on the margins of the two rivers furnished a chief part of the cases.²

In a previous chapter, I have stated what were the boundaries of the fever in 1819. It remains to state that the cases were of a more severe character in the focus or the original spot of the disease than elsewhere. Of the nature and condition of those localities, Dr. Watts has left a graphic account in his history of that epidemic, and as these localities were the seat of several other visitations, I will refer to his statements in a future chapter.

Similar observations were made in 1822; when the disease began on the corner of Rector Street, in Washington Street and on the Dock. The disease, as usual, proved most fatal where it first appeared—the mortality in Rector Street, between the point mentioned and Greenwich Street—a distance of only 188 feet, being exceedingly great. The malignancy and fatality of the fever was, indeed, found to be in proportion to the filthy and close condition of the streets.³

A like localization of the disease in certain quarters takes place in other yellow fever cities where it almost universally originates on the water side, in streets, courts, alleys, wharves noted for filth, defective ventilation and like morbid agencies. This circumstance was called attention to long ago by Valentin and others in regard to Norfolk, where the fever was found to prevail more frequently, and to assume a more malignant garb in some situations—the wharves and Water Street, for example—than elsewhere, and has been verified in the later epidemics of that city.⁴ Similar observations have been made in Wilmington (N. C.),⁵ Alexandria,⁶ Baltimore,⁷ New London,⁸ Wilmington (Del.),⁹ and Charleston.¹⁰ In New Orleans, as already seen, the disease is not confined within such narrow bounds; nevertheless, it is principally rife and malignant on the banks of the river, which at the sickly season is low, leaving an extensive surface—the common receptacle of all kinds of filth—bare. It first breaks out and spreads there, and in St. Thomas, Madison, and St. Mary Streets; about the markets; at the triangle; in some

¹ Hardie, *Fever of 1822*, p. 13; *Quarantine, Rep.*, p. 9.

² Miller, *Works*, pp. 88, 89; Hardie, *Fever of 1822*, p. 16.

³ Townsend, *Fever of 1822*, pp. 82, 100.

⁴ Valentin, p. 201; Selden and Whitehead, *Med. Reposit.*, iv. 331; *Ib.*, v. 129; Hansford and Taylor, *Med. Reposit.*, iv. 205–6; Archer, *Med. Rec.*, v. 61.

⁵ Hill, *Med. Rec.*, v. 86.

⁶ *Diet. Med. Reposit.*, vii. 191.

⁷ Opinion of the Faculty; *Med. Reposit.*, iv. 355; Revere, *Letters on Fever of 1819*, p. 15; Recorder, iii. p. 237, &c.

⁸ Holt, *Med. Reposit.*, ii. 334.

⁹ Vaughan, *Med. Rep.*, iii. 368, 370; *Ib.*, *Fever of 1802*.

¹⁰ Shecut, p. 100.

of the front streets of Lafayette and in the seventh ward; about Gormsley's Basin, &c., all of which localities are filthy, crowded, and badly ventilated.¹

This was well exemplified in the last-mentioned city during the great epidemic of 1853. The 4th district contained an estimated population of 15,310, without including a large proportion of recent emigrants. The number of cases of yellow fever in the entire district is estimated to have been 7,248, being at the rate of 452 per 1,000—more than double that of any of the other districts. This district is the worst in a hygienic point of view.

A difference existed in the several wards of that district. The first gave 542 per 1,000; the second, 421 per 1,000; the third, 508 per 1,000; the fourth, 433 per 1,000, and the fifth, 452 per 1,000.

The 1st district, with a population of 60,695 inhabitants, gave 14,263 cases, and a ratio of 234 to the 1,000, viz: First ward, 459 per 1,000; second, 277 per 1,000; third, 164 per 1,000; fourth, 216 per 1,000; fifth, 119 per 1,000; sixth, 121 per 1,000; seventh, 349 per 1,000.

The 2d district contained an estimated population of 49,926, which gave 4,377 cases or 87 per 1,000. In this district, the second ward gave 173 per 1,000, or more than double the average of the whole. The fifth ward gave 123 per 1,000. The greater portion of the balance of the cases in this district occurred in the front portions of the third, fourth, and sixth wards in the vicinity of the market, and in the disgusting and horrid purlieus of this neighbourhood.

The 3d district, in an estimated population of 28,202, furnished 3,232 cases, or 114 to the 1,000. In all the districts and wards, where the disease was most prevalent, and furnished the largest ratio to the population, it was virulent and malignant in character, and gave rise to the largest proportionate mortality.²

By Berthe, we are told that in the Barrio of Santa Maria, Cadiz, when the disease first appeared, the mortality was frightful, the proportion of deaths among those who were attacked exceeding, by ten times, that of some other situations (p. 162). A similar difference, in respect to situation, was observed at Seville, in the wider and more elevated streets of which, the mortality amounted to 1 in 18 or 20; while in damp and low situations—as in Triana, and Los Humeros—the loss amounted to one-third, and even one-half (p. 163). At Malaga, in 1804, the disease prevailed most destructively in a low suburb called the Barrio de Perchel, and in other contiguous parts of the town.³

See what took place at Demerara in 1837. Although the mouth of the river of that name was the chief seat of the pathogenic influence of the epidemic, and furnished, most numerous, susceptible subjects, yet the morbid cause was not confined to this locality, but extended, more or less, along the whole coast of the colony. In Georgetown, the focus of disease seemed to be the mud lots and their immediate neighbourhood. The disease there,

¹ Barton's Report, pp. 348, 367.

² See Barton's Rept., p. 400, and following.

³ Bancroft, p. 468.

as everywhere else, was more virulent than in other localities. In the same town, in 1842, Dr. Blair found that it was more malignant and fatal in the original site, near which it had first showed itself, *i. e.* the Mud Lots.¹ In Barbadoes, in 1816, the localization of the disease, and its greater severity in some places, were strongly exhibited. The malignant nature of the prevailing disease was noticed in September. It first appeared in the persons of the families residing in the huts—low ill-ventilated buildings constructed with a single deal board and roofed with shingles, and each commonly occupied by two families composed of six or more individuals. To borrow the words of Dr. Ralph: “It was where these creatures were living, crowded and oppressed by heat, that the causes which favour the contamination of a stagnant atmosphere were most abundant; there also did the disease speedily rage with peculiar malignancy, and many fell victims to it. Our first patients were from the huts. The barracks of the men, more advantageously situated, and, in point of construction, beyond all comparison superior, having a well regulated interior economy, under the immediate eye of the officers, experienced almost simultaneously a visitation of this fever. Here, however, the disease was milder in its symptoms, the stomach was more manageable, and the deaths proportionably fewer.”²

To what precedes, may be added that, in infected vessels, the malignancy of the disease varies greatly according to the locality where the cases occur, or which the individuals attacked may have visited or worked in. Such, for example, was found to be the case on board of the *Souverain*, the *Macedonia*, and the *Rattlesnake*, to which I shall call attention in a subsequent chapter.

As I have already said, this observation—which has been noted not in one place only, but in many—is of great value; and the more it is considered, the more it will be regarded as inconsistent with the doctrine of contagion, and favourable, on the contrary, to that of local origin. It separates the yellow fever from the class of contagious complaints, and approximates it to that of malarial fevers, which, as is well known, do not arise from, and are not propagated by, contagion. In diseases produced by personal contagion, it matters not in what way or position the poison is applied, those slightly exposed or at a distance, if they take the disease at all, have it—all other things being equal—as severely as those who are exposed more closely or long. In remittent fever, the result is different, for the virulence of the poison is at all times proportioned to the approximation to its source. It is weakened by dilution in the air, and in proportion as it is more diffused it produces a slighter disease. Dr. J. Hunter, who has pointed out the difference, says: “This is clearly seen in the health of ships stationed off a marshy coast. Those that are nearest suffer most, both from the frequency and virulence of the fever; those that are further off suffer less; till at last, at the distance of a few miles, they are perfectly healthy” (p. 327).

That a different sentiment has been expressed by some who admit the fact to which attention is here called, is true; but it may be doubted whether the

¹ Blair, pp. 33, 34.

² Edinb. Med.-Chir. Trans., pp. 55–6.

explanation they offer, obvious and familiar as it may appear to them, will be considered as satisfactory by the generality of medical readers. Thus, by a writer of great pretension and trenchant opinions,¹ it is said that the effect takes place in localities where the disease first appears; "because, when the first cases happen, the inhabitants in the vicinity, not being willing to abandon their homes before they are well assured of the existence of the disease, have generally delayed too long a time within the reach of its influence. Thus it happens that the contagious poison, having had full time to propagate itself to the immediate neighbourhood, has become accumulated to so great an amount that no human means can stay its march. The spark being once kindled, it is too late to extinguish the flames." But that this explanation is not admissible we may infer from the fact that Dr. Townsend himself acknowledges that what he has said "is not always true," inasmuch as on many occasions yellow fever, "like other contagious diseases, has proved equally fatal in those places in which it had afterwards become propagated, as in that particular neighbourhood into which it was first introduced. Now, it is not difficult to show that the disease has assumed its worst and most fatal forms in localities where there was no accumulation of patients to produce the effect in question." In 1820, patients were not permitted to accumulate at Hodge's wharf more than in Letitia Court, and yet the cases were more malignant in the former than in the latter of those localities. When the disease broke out in Duke Street and the vicinity, it did so suddenly; and though there was no accumulation of sick to concentrate the force of the poison, and thereby aggravate the disease, the latter proved fatal in eleven out of twelve cases; and in Water Street, between Market and Mulberry, at a considerable distance from any supposed accumulation of cases, death spared none of those attacked.

The history of the rise and progress of contagious diseases shows, it is true, that, when epidemic or otherwise, they differ in point of malignancy under peculiar circumstances; but these circumstances have reference to the condition of the individuals attacked, rather than of the localities in which the disease makes its appearance—being modified by the state of the system or constitution of the individual attacked, as well as by the nature of the agencies in the midst of which he is more immediately placed; agencies which act perhaps by concentrating the poison, but more effectually by impairing the power of resistance of the system. But, while such is the case, the effect is obtained wheresoever such contingencies exist—whether in one part of the infected locality or in another—whether in a country or city atmosphere. In no instance do those diseases exhibit the constant and permanent differences remarked in the yellow fever—mild and simple in one place, malignant and aggravated in another—not in a few instances only, but in the large majority of those that occur there. When we notice these results in connection with the disease under consideration, and recollect that they occur in instances when there has been no accumulation of patients to concentrate and aggravate the

¹ Townsend, p. 82.

virulence of the poison, and, withal, that the disease, as already mentioned, never appears, whatever be the accumulation, out of the infected localities or in the country, we cannot feel inclined to attribute the effect solely to the peculiar state of the system of those attacked, and the nature of the agencies by which they are personally surrounded. However effectually peculiarities of constitution, temperament, and idiosyncrasies may modify the disease, the difference in question is too general to be referred exclusively to such a cause, inasmuch as it is not likely that individuals labouring under a state of predisposition calculated to render the disease more malignant and fatal will be found concentrated in any given locality, while others in a better condition to resist the pathogenic influence will be assembled elsewhere. The argument is vitiated by the fact that individuals who are attacked in those localities are often only visitors.

On shipboard, too, the difference in the malignancy cannot be due solely to concentration. Nor can the effect be accounted for from the nature of the hygienic circumstances amid which the sick are placed; for though, as we have already seen, the disease generally assumes a more aggravated form in close, ill-ventilated, and filthy localities and apartments, yet the same circumstances occur in situations which forbid us from ascribing them to such influences. In this city, as we have seen, the disease has usually been more fatal along the margin of the river, and has assumed a milder form as it travelled westwardly; and yet, as a general rule, patients in Second Street—nay, in Front Street—were as well off, in point of cleanliness and ventilation, as those in Fourth or Fifth Streets. In 1820, the individuals among whom the disease proved so fatal in Duke and Green Streets, or in Southwark, were as favourably conditioned in these respects as those in other parts of the city—certainly more so than those about Walnut Street and Letitia Court, where cases were less malignant and the mortality much smaller.

Instead, therefore, of seeking an explanation of the difference exclusively in the peculiar condition of the patients themselves, or in the nature of the apartments or houses in which they are placed, it is more natural to refer the more aggravated nature of the disease in one locality and its mildness in another to a greater concentration and virulence of an external cause, evolved at a greater or less distance from those attacked, and which, from some reason or other, becomes highly concentrated, is carried in certain directions, and poisons those submitted to its influence in a degree commensurate with its virulence or the extent of its accumulation. That the effect is not due to the crowding of the sick, and that it occurs where there are no sick to produce it, we have seen; and as a similar observation is daily made in reference to fevers arising admittedly from some of the modifications of infection, and as admittedly destitute of contagious properties, we derive from it an additional reason for referring the yellow fever to the operation of a cause allied to, though not identical with, that to which those fevers are due, and for considering it as, like them, destitute of the properties in question.

CHAPTER XX.

PROOFS OF NON-CONTAGION—CONTINUED.

Originates in Low, and in or near Impure, Localities.—As a general rule, it may be stated that the yellow fever, when it appears in a place, originates and prevails most readily—in a word, is localized—in spots which, from their depressed position and their dampness, from the quality of the soil and the nature of the substances covering or mixed up with the latter, as well as from imperfect ventilation and a neglect of proper hygienic measures, may naturally be supposed to be the seat of the evolvment of materials calculated to impair the purity of the atmosphere. This, at least, has almost universally been the case in temperate regions; and if exceptions to the rule present themselves in hot latitudes and elsewhere, their number is limited, while their occurrence proves, not that the production of the yellow fever is unconnected with the conditions of localities mentioned, but that the cause giving rise to that disease may proceed from other conditions, the nature of which is not as yet fully understood. The various visitations of the fever in Philadelphia, from the earliest to the last epidemic, exhibit this peculiarity in a striking manner.

In a former part of the present work I have briefly designated the localities affected, and stated their condition at the time of the outbreak of the disease. From what was then said, it will be seen that the fever has always originated in the lower part of this city, along the margin of the river, and in the neighbourhood of the wharves. In 1699, it broke out in the vicinity of the Dock, where the settlement had originally commenced, and near the river, at several points of which dwellings and stores had been erected. The ground had been but lately cleared, and the creek, ponds, and swamps were much as they had been found, and perhaps in a worse condition, from being surrounded by decaying timber or filled with offals, the whole exposed to the drying action of a scorching sun. In 1747, the disease originated in, and, as formerly stated, was mostly confined to, the southern and low parts of the city, below the draw-bridge, at a short distance from the river; in sailors' lodgings, which were then, as they are now, located in narrow, crowded, ill-ventilated, and filthy streets. It also appeared in the vicinity of the Dock, which was uncovered, and in a miry and offensive condition. It may be recollected, besides, that Dr. Bond assigned its origin to crowded ships in the port, which, in the absence of other proofs, would show that it early prevailed along the wharves. In the next epidemic, that of 1762, the original seats of the disease were the sugar-house wharf, the vicinity of the new market, extending to Front and Water Streets, in an eastward direction; as well as a kind of court, the entrance to which was by two narrow alleys. Even more evident was its origin about or near the

wharves in 1793, when the first cases occurred in Water Street, and for some time were almost invariably traced to that locality. The outbreak of the disease in the neighbouring village of Kensington occurred in like manner near the Delaware; while, as already stated, at subsequent periods of the epidemic, the greater number of sufferers were to be found among the residents of low, damp, ill-ventilated, close, and filthy alleys and small streets. "The inhabitants of dirty houses," says Carey, "have severely expiated their neglect of cleanliness and decency by the number of them that have fallen sacrifices. Whole families in such houses have sunk into one silent, undistinguishing grave. The mortality in confined streets, small alleys, and close houses, debarred the free circulation of air, has exceeded in great proportion that in the large streets and well-aired houses. In some of the alleys, a third or fourth of the whole of the inhabitants are no more" (p. 62).

In 1794, the disease again made its appearance in Water Street. In 1797, it began at the water side, both in the district of Southwark and in Kensington, where its ravages were chiefly confined. The epidemic of the next year broke out in the immediate vicinity of the river and in the streets adjoining, and thence spread westwardly. That of 1799 commenced in Penn Street, which lies at a short distance from the wharves of the southern part of the city, as well as along the wharves themselves between Pine and Lombard Streets. In 1802, the disease originated near the corner of Front and Vine Streets, necessarily not far from the river; and subsequently extended to other parts, though particularly along Front and Water Streets near the Drawbridge. The first location of the fever in 1803 was about Chestnut and Water Streets; the next in Water near Sassafra. The disease next appeared in Water near South; and, except in courts, alleys, and the crowded and ill-ventilated dwellings of the lower classes, its range westward was very limited. In 1805, its origin was, in like manner, traced to the river side, though beyond the limits of the city proper; and, if cases occurred in a more westwardly direction, they were few in number, and never originated beyond Second Street in the city and Fourth Street in Southwark. The fever in 1819 broke out on the north side of Market Street wharf, and, with few exceptions, was limited to that vicinity. At a later period, it reappeared in some small and narrow streets and alleys situated near the southern limits of the city. Again: the epidemic of 1820 originated in Water Street near Race or Sassafra, and in a range of buildings extending towards the river near the foot of the last-named street. Next, it made its appearance about Walnut Street wharf, and subsequently showed itself in various localities not very different from these in a hygienic point of view.

Finally, in 1853, the disease broke out on or about the wharf at the corner of South Street, and prevailed along the former and in the adjoining street—occupying an area of some 600 feet in one direction, north and south, and 300 in the opposite. At a later period, a few cases appeared in a different part of the city, in filthy rooms, forming part of an unfinished warehouse, the whole upper part of which was rented out in rooms to different families, of the lower order of Irish—everything around presenting poverty, rags, and

filth. The condition of the localities where the fever originated and spread, will be judged of from the following extract from the published account of the epidemic, for which we are indebted to Dr. Wilson Jewell:—

In one of his early communications to the College of Physicians on this subject, the author reminds us that the docks along the Delaware line, between Lombard and Almond Streets, contain large accumulations of offensive mud and other filth. Though advocating the opinion of the exotic origin of the disease, and attributing a paramount influence to the morbid atmosphere developed on board of a vessel which had arrived from a Cuban Port, Dr. Jewell admits that, “in addition to the prevalence of that poisonous atmosphere, there existed causes in the immediate vicinity of South Street wharf, sufficient to justify the supposition of their agency in the development of disease of a malignant type, when subjected to the high thermometrical influence which prevailed throughout the months of June and July. Not the least mischievous of these causes, in the production of an unhealthy atmosphere, was the outlet of the sewer into the dock at South Street Ferry, belching forth continually putrid masses of animal and vegetable filth accumulating around its mouth, and exposed at low water to the rays of the sun, exhaling streams of unwholesome and poisonous gases into the surrounding air. Besides this agent, there was a most foul wharf at the upper side of South Street; a filthy avenue between Lombard and South Streets, without any properly constructed surface drainage; numerous damp confined cellars, subject to an occasional overflow by the ebbing and flowing of the tidewater of the Delaware, and various minor causes that might properly be added to the above category, fruitful in the production of atmospherical changes injurious to health.”

At a later period (October 5), Dr. Jewell reverts to the subject, and after taking some pains to show that the disease was not sustained by an overcrowded population, he remarks: “The whole neighbourhood, however, may be considered as favourable to the production and nourishment of malarious fevers, in view of its proximity to the river docks, the open sewer at South Street wharf, the damp cellars, filthy alleys, and other local causes of disease, under such a long-continued high thermometrical atmosphere, as prevailed during the months of July, August, and September.”

To this, let me add, that the cases which occurred in the upper part of the city, appeared “in the immediate vicinity of Willow Street wharf, where the culvert along Pegg’s Run empties into the Delaware, which outlet, at low tide, is fully exposed, and where, at all times, there is a large deposit of putrefying vegetable and animal remains, and that, from the decomposition of these remains there arose, as Dr. Jewell acknowledges, ‘a miasmatic constitution of the atmosphere,’ ‘from the inhalation of which those individuals contracted the fever, resulting in the death of two of them.’”¹

The details into which I have thus entered, confirm the statement as to the origin and greater prevalence of the disease in low and impure

¹ Trans. of College of Phys., N. S., ii. p. 66, &c.

localities so far as this city is concerned; for it appears from them that the fever has almost invariably made its appearance near the river side, or in the adjoining streets—sometimes more northerly than at others, but never more than a few hundred feet from the eastern edge of the city or districts; that there it has prevailed most extensively and fatally; and that when it has broken out and raged with violence in other localities, more or less distant from the former, these localities approximated closely, in point of hygienic condition, to the more usual seats of the infection.

From the account presented of the topography of Philadelphia, and of the condition of the city at various periods, the reader will perceive that the whole of the river side, from the lower to the upper limits of the city and districts, is noted for its low position and humid condition; and that the effect resulting from the nature of the constructions erected there, so far from being beneficial, has been to increase the natural humidity of the place, and aggravate its injurious tendencies, as well as to produce an accumulation of filth, and afford obstacles to a free circulation of pure air. Over the low and humid surface of the small strip of land situated under the bank of the river, narrow streets, courts, and alleys were placed; stores and dwellings, in close approximation with each other, were erected, without the remotest reference to the rules of hygiene; and in great measure, early occupied by and crowded with the lower classes of the population and sailors, whose habits, here or elsewhere, are not of a kind likely to improve the salubrity of a place; while no precautions were taken to guard against the injurious effects accruing from the accumulation and deterioration of perishable merchandise. Again: the margins of the dock, which, as well as those of the creek running up from it, remained long uncovered, were flat and swampy, and at low tide exposed to the evaporating action of the sun; and the neighbouring soil, for a long way around, was, and is to this day, low and humid. The reader will also recollect that the city plot contained swamps, ponds, and creeks, which, until a few years since, were in part uncovered; as well as many excavations made for particular purposes, and often filled with stagnant water.

Nor is it less true, that the peculiar mode of construction and the usual condition of our wharves, tended, though more particularly in former days than since the improvements of the last fifteen or twenty years, to increase the injurious effects resulting from the state of the adjoining parts. In a former chapter, I have represented those wharves as being constructed of square casements of logs, filled up with earth, vessel ballast, and stones. It was stated, also, that, until the last fifteen years, the parts in question, as also the street or avenue running along the river, remained unpaved, and were, necessarily, often miry; that the street itself was, until recently, narrow, irregular, inconvenient, and of injurious tendency from the projection of many of the buildings fronting the river; that the wharves constructed in the way mentioned, project out in many places into the river, leaving open spaces or docks between them, which readily become receptacles of filth, and that the half dried surface of these docks, as well as the vertical sides of the wharves being exposed, at low water mark, to the powerful action of

the sun during the hot season, become the source of offensive and injurious effluvia. On this, and on much more, concerning the former and present conditions of various localities of the city and districts, having a bearing on the rise and prevalence of the fever in different years—on the condition of Pegg's Run, and other streams, and its probable injurious influence—on the low, miry, and swampy state of various places—the imperfect grading and draining of many of our streets in former days—the number and filthy condition of our alleys, courts, and lanes—the foul state of our privies formerly; and on the extreme foulness of the docks, wharves, &c., at the periods of our various visitations; on all these subjects, I say, and on the many sources of effluvia resulting from the accumulation of ships at the wharves, and from perishable merchandises, &c., I have dwelt already, and need not revert to them again. Certainly the reader will discover in all these circumstances fruitful elements of an impure and injurious atmosphere, particularly if he bear in mind the peculiar agency exercised on the materials thus exposed by the desiccating action of the direct and reflected rays of a truly torrid sun; and he will conclude that however the fever may have commenced, its rise and prevalence are in some way connected with the impurities pointed out.

To this inference it may be objected that the fever has occasionally broken out and prevailed to some extent in localities of a different kind; that in 1820 it appeared suddenly in Duke and Green Streets, at some distance from the river, and prevailed in parts which are neither lower, narrower, closer, nor more humid or filthy than others that remained healthy; that the disease, while commencing and prevailing more extensively in localities such as those above described, extended more or less in higher, better ventilated, and cleaner parts, and even invaded the jail and hospital, which then were in open situations; and that, on the other hand, it spared localities at no great distance from those infected, and no less low, humid, close, and filthy. In answer, it may be remarked that when the fever has appeared to originate in high and apparently healthy situations, there generally existed, at no great distance, some locality from which there evolved streams of impure air, and that, in the instances adduced above, the families attacked occupied houses situated in the vicinity of Pegg's Run, which, as described, was in a most filthy condition. Besides, the medical reader need not be told that the poison giving rise to the yellow fever must, like every other of the same class, when once evolved, be wafted to a greater or less distance from its place of origin; and, he knows, that because the fever originates and prevails in localities such as have been mentioned, it does not follow that it ought to do so in every situation of like nature, inasmuch as, after all, the elements requisite for the elaboration of the cause may not there be found, or may be neutralized by other elements which escape our observation. All this is noticed in the rise and progress of other febrile diseases depending avowedly on malarial poisons, and may be explained, in both instances, without reference to contagion.

That the origin and prevalence of the yellow fever is in some way or other connected with localities of that nature and the impure atmosphere resulting therefrom, and is not a mere coincidence peculiar to Philadelphia, may be in-

ferred from the result obtained elsewhere. Facts similar to those noticed here are recorded in connection with the visitations which New York experienced in 1791, 1795, and indeed at every subsequent return of the disease.

The first of those epidemics commenced, as we have elsewhere stated, in a part of the city which was low and damp, thickly inhabited, with houses generally small and badly ventilated, and with wharves as filthily kept and as injudiciously constructed as those of our city.¹ Of the fever of 1795, Dr. Hosack states that it first appeared upon the east side of the city, and next spread to Peck Slip. Throughout that season, it was confined in great measure to that part of the town where the local condition of the atmosphere was peculiarly favourable to its diffusion; for not only an unusual quantity of filth was accumulated in Peck Slip, but at the very time a great number of immigrant poor had arrived from England, Ireland, and Scotland, so that the numerous lodging-houses, especially in that neighbourhood, were unusually crowded.² Dr. E. H. Smith tells us that the district infected was the lowest, flattest, and most sunken of the whole city; that it was swampy; and that several of the streets were unpaved, had been opened on made ground, and were lined with decayed and dirty wooden buildings.³ Dr. R. Bayley, in his record of the same epidemic, has depicted the condition of the infected localities in kindred terms, and dwelt on peculiarities which I shall revert to in another chapter.⁴ Of the epidemic of 1798, we learn that it made its appearance in the neighbourhood of New Slip, that it next prevailed at Coffee-House Slip, also on the east side of the city, and at other points on the margin of the river; and principally in a thickly settled part of the city, where the houses were small, the streets narrow, and chiefly occupied by the poor.⁵ Dr. Hosack informs us that a number of circumstances occurred to diffuse the disease between Burlington and Peck Slips. "A great quantity of rain had fallen, so as to overflow the cellars in Pearl Street, which were at the same time stored with salted provisions; these were soon afterwards spoiled, and loaded the atmosphere with a highly offensive vapour."⁶

The fever of 1800 is described as having prevailed "in those parts of the town which are chiefly low, damp, and filthy, and which have heretofore been distinguished for an unhealthy character."⁷ The first public alarm in 1803 arose from some fatal cases at the Coffee-House Slip, and in that neighbourhood. "The streets lining the margin of the two rivers, and some of those in the upper part of the city which are principally inhabited by indi-

¹ Addoms, p. 7.

² On Contagion, in Essays, i. 294.

³ Webster's Collection, pp. 67-74.

⁴ An Account of the Epid. Fev., &c., p. 56, &c.; see his "Letters from the Health Office to the Common Council of New York," p. 14; see also Seaman's Account of the same Epidemic in Webster, p. 35; A. Hosack, p. 9.

⁵ On Contagion, op. cit., i. 295; see Hardie, Fev. of 1798, pp. 25, 29, &c.

⁶ *Ib.* See also Report of the Committee appointed by the Medical Society of the State of New York, to inquire into the Symptoms, &c., of the Disease that prevailed in New York during the Summer and Autumn of 1798.

⁷ Med. Rep., iv. 207.

gent, uncleanly, and dissolute classes of the community, suffered the worst ravages of the disease.”¹ During the early period of the epidemic of 1805, “nearly all the cases took place on the eastern side of the city, in Front, Water, and Pearl Streets, and principally below Burlington Slip.” They afterwards became more diffused, but, “on the whole, the low grounds on the margin of the two rivers certainly produced a chief part of the cases.”²

In 1819, the fever again made its appearance on the water side, and of the localities affected Dr. Watts gives no very flattering description: “From Coenties Slip to the Coffee-House, and from Water Street to South Street, both inclusive, is principally made ground. It is in the recollection of persons now residents of the city that the tide-waters of the East River reached nearly to Wall Street, and what was formerly the old ferry-house is still to be seen in Broad Street, above Garden Street, and what was called Hanover Square, at the foot of William Street, was near the edge of the water. Great and Little Dock Streets were parts of Pearl Street, and were the artificial margin of the East River, composed of logs, and promiscuously filled with every variety of materials. The progressive improvements of this part of the city formed a large proportion of Water, Front, and South Streets. All this part is closely built up; many of the buildings are of wood, and are now much decayed; if the houses have yards, they are principally small, and all of them surrounded by wooden fences, and many of these have long been in a state of decay. The surfaces of the yards, thus formed of an artificial soil, are exposed, with all the decaying materials of their inclosures and the accumulating deposits, to the direct and reflected rays of the sun, and are excluded by their situations from the natural agitations of the air. Very few of the houses, even in Pearl Street, within this district, have the means of carrying off the surplus or refuse water from the rear, excepting by troughs, which in a great number of instances pass through the cellars of houses in Water Street; these troughs are formed of wood, in some instances close and in others open, running along the bottom of the cellars, or at an intermediate distance from the floor and the ceiling, according to the declivity of the ground; these become occasionally choked, and are dripping or bursting in a number of instances in the houses themselves, or within a small distance of them.”³

In the various epidemics just enumerated, the fever invariably began somewhere on the East River. In 1822, it was first seen at the foot of Rector Street, a small, narrow street running from Broadway to the North River—a part of the city which had heretofore been exempt from the disease. The first cases occurred at the corner of that street and the wharf, in the immediate vicinity of which the disease was for some time circumscribed. It thence spread in various directions, but, as usual, exercising its baneful influence in narrow, confined, and sheltered streets and passages—Lumbar, Carlisle, Albany, Thames, Cedar, and Liberty Streets, Beaver Lane, &c. At a later

¹ Miller's Letter to Governor Clinton, on the Epidemic of 1803, Documents relating to the Board of Health, p. 36; Hardie, p. 13.

² Miller's Rep., Works, pp. 88, 89.

³ Med. and Surg. Register, pp. 350-1.

period it broke out in the upper part of the city, but was there limited to a few streets and blocks of houses, which in point of character were not unlike those of the lower infected district.¹

In Boston, the disease has mostly prevailed at a short distance from the water's edge. In 1798 and 1802, it broke out near this part, occurring only in persons who lived or passed much of their time about the town dock, Codman's, Stoddart's, and other wharves, as also about State Street, Liberty Square, the market place around Fort Hill, and some similar situations—that is to say, in parts which are low, crowded, narrow, ill ventilated, and in a filthy and offensive condition. The epidemic of 1819 broke out and prevailed in nearly the same localities.² The market place was, in 1798, as we learn from Dr. S. Brown, a low, sunken part of the town. “It is, from situation, the reservoir of every putrid matter, flowing in from more elevated parts of the town, and accumulated by every rain. It is surrounded with docks of stagnant water, filled with offal and all manner of noxious matters, which, becoming putrid, throw up, at every ebb-tide, a stench very disagreeable to the adjacent inhabitants.” “Fort Hill,” Dr. Brown adds, “is very much exposed to reflected heat. The western breezes are almost entirely excluded from the southeast sides of the hill, while these are so inclined as to meet the sun's rays in a perpendicular direction. These parts of the hill are by far the most thickly inhabited, and were the most sickly.” After stating that in several of the buildings near the market and Fort Hill a large quantity of green hides—some in a state of putrefaction—were stored, Dr. Brown further remarks of those parts of the town where the disease seemed to originate, “that they are in situation low, confined, crowded with buildings, and full of inhabitants—shut out from northern and western breezes, open to the south and east, exposed to the sun's heat, and this greatly increased by reflection and refraction from pavements, buildings, &c.”³

In Providence, the disease, in its several visitations of 1797, 1800, and 1805, uniformly made its appearance, and committed its principal ravages, in the south part of Water Street, or the lanes and alleys immediately adjacent. The portion of this mentioned street which was the seat of the fever, has a southeastern direction. “The houses on the water side are built as near as possible to the natural bank of the river. The wharves, of course, which are extended in the rear of them, westward to the channel, are artificially raised, partly filled with earth, and partly constructed with logs, covered with oyster shells and earth, leaving vacuities beneath through which the tide ebbs and flows. They are not spacious, very flat, and indented with a number of small docks, a part of which is left bare at low water.” These docks are receptacles of the waste of the town, and of the swamps and meadows several miles in the country, and

¹ Townsend, pp. 106-7.

² N. Eng. Journ., viii. 380; N. A. Rev., x. 395-6; Med. Rep., vi. 339.

³ A Treatise on the Nature, Origin, and Progress of the Yellow Fever, pp. 22-3, Boston, 1800, p. 22, &c.; ib. An Account of the Pestilential Disease which prevailed at Boston in the summer and autumn of 1798, Med. Rep., ii. 390-2; Of the Epidemic lately prevalent in Boston, by Isaac Rand, Ib., ii. 466-8.

which are brought down by the two fresh rivers which empty into the cove.¹ In New London, in 1798, the infection was limited to a part of the city where similar local conditions existed.²

The fever of 1802, in Wilmington (Del.), began in the low part of that town, near the margin of the river. King Street, the first infected, was contracted to the limits of a common alley, from Second Street to the Christiana River, and such was its condition that Dr. Vaughan brands it with the name of a nest of noxious effluvia (p. 6). In Baltimore, the fever has uniformly begun and prevailed most extensively at Fell's Point, which is low, flat, swampy, lies on the edge of the cove, and was a long while unpaved; and when it has appeared or extended in the city proper, it has invariably done so on the wharves, which are constructed like ours, and in no better condition, as well as in the narrow, crowded, and filthy streets lying in the vicinity. That the condition of these localities at the periods of these visitations, 1794, 1798, 1800, and 1819, were such as to insure the evolution of an impure air, will fully appear from the records we possess on the subject.³ "To every man of accurate observation," Dr. Potter remarks, "possessing the capacity of discrimination, the rise and progress of the three great epidemics of 1794, 1797, and 1800, as they appeared in Baltimore, would alone be sufficient to establish the domestic origin and non-contagious character of the yellow fever. The disease was exclusively confined to the low grounds covered with the materials of putrefaction, or to those parts composed of factitious materials corruptible in their nature. In that part of the city west of Jones's Falls, the line of demarcation between the pure and deleterious atmosphere, was obvious to the most superficial observer" (p. 20). "No case of the yellow fever ever did originate in west Baltimore, above Hanover Street, beyond the sphere of exhalation from the docks, wharves, or made ground. I defy the whole population of the city to produce a solitary exception; nor can they adduce a single instance, from the hundreds and thousands of those who, after breathing the pestiferous atmosphere below, and sickened there, communicated the disease" (p. 21).

Extending our examination in a southerly direction, we find that at Alexandria the fever originated near the river, and prevailed in three ranges of squares where the ground was low and made, and where many of the storehouses had no cellars, and are supposed to have had water under their lower floors. There, also, wharves were extended to the channel of the river—constructed, doubtless, in the usual imperfect manner; while the impurity of the atmosphere resulting from these causes, as well as from the filth incident to the accumulating of vessels, was greatly increased by the existence of a very large mass

¹ Wheaton, *Med. Repos.*, x. 333.

² Holt, *Med. Repos.*, iii. 292; Channing, *Med. Repos.*, ii. 402; Coot, *Ib.*, ii. 407.

³ Potter on Contagion, p. 21; Drysdale, *Fev. of 1794*, *Museum*, i. 26-7, &c., 361; Davidge, *A Treatise, &c., on Fever of 1798*, p. 68; *Opinion of the Medical Faculty*, *Repos.*, iv. 351; Chatard, *Med. Repos.*, iv. 254; Revere, *Recorder*, iii. 215; Reese, *Observations on the Epid. of 1819*, pp. 19, 20, 21; *Letters on Fever of 1819, &c.*, pp. 29, 49, 51, 61, 71, 97, 109, 131; *Rept. of Dist. Med. Soc., in Ib.*, 140.

of oyster-shells, many of which contained oysters or parts of oysters in a state of putrefaction.¹ In Wilmington (N. C.), a town situated in a low, flat, swampy country, the fever of 1821 began on the margin of the river, and prevailed there with most intensity. As to the condition of the infected locality, we may form some idea from the statement of Dr. Hill, to whom we are indebted for an account of this fever: "The parts of the town adjacent to the river are but a few feet elevated above its surface. The wharves are made ground, badly constructed, and are always overflowed by storms, and frequently by high tides. In the vicinity of the square most and earliest ravaged by the disease, there is one of those wharves in an unfinished state, partly filled up with decaying vegetable matter, which, inclosed within logs, and successively acted upon by the tides, exhibits a most loathsome and putrefactive source of disease. Our docks are notoriously filthy, and our cellars are so low and damp, as in wet seasons to require daily bailing."²

The yellow fever of Norfolk, where, prior to the memorable fire of 1804, which destroyed the larger portion of the lower section of the city, the disease had almost annually raged as a scourge, has invariably made its appearance and prevailed most extensively and fatally in localities kindred to those I have so often described. Its original and principal seat has been the wharves and adjoining districts, and the writers who have described the various epidemics of that place, Valentin, Taylor, Hansford, Selden, and Whitebread—and, at a much more recent period, Archer—furnish facts sufficient to show that such localities were of a nature, and in a fit condition to furnish ample causes of insalubrity. The first describes the situation of Water Street, and the composition of the new made ground which serves as a foundation to frame and other houses of that and adjoining streets; and adds that there are few of these houses, under and in the intervals of which the water does not stagnate and leave a large surface of mud exposed to the desiccating process. It is always in this part of the town, which was principally inhabited by sailors and the lower classes, that he observed the disease to prevail most extensively and severely.³ In a subsequent part of the volume, he expatiates on the improper construction and injurious effects of the wharves in that and other parts of the Union.⁴ Drs. Taylor and Hansford remark, in confirmation of Dr. Valentin, that the yellow fever, in its malignant form, always originated on the river bank, "or on low, new made grounds, and in houses built on the docks."⁵ Drs. Selden and Whitehead are more explicit on the subject, and after referring to various sources of injurious effluvia, they remark: "The line which marks out the boundary of the borough of Norfolk, on the side next the river, comprehended originally more water than land on that side of the main street. In some places, the wharves are advanced upwards of an hundred yards into the river, as far as this boundary line—in others, they are not yet carried so far, while in some parts no attempt has yet been made to disturb the original possession of the river; but that part of the

¹ Dick, *Reposit.*, vii. 191.

² *Med. Recorder*, v. 86.

³ *Traité de la Fièvre Jaune*, p. 101.

⁴ *Ib.*, p. 113.

⁵ *Med. Repos.*, iv. 206.

town where the malignant fever chiefly prevailed, stands entirely on made land, reclaimed from the river by sinking pens of large logs, and filling them up chiefly with green pine sapplings, which are slightly covered over with earth or gravel. In some places large openings are left for the formation of docks, in others wharves are formed next the channel of the river, while the more interior parts are still covered with water, and in many others the lots remain in their original state; so that from these circumstances, and the loose texture of the whole work, the water of the river penetrates, at every tide, through the whole extent of this wooden fabric, which is thus alternately exposed to the action of the water and air, assisted by the powerful rays of an almost vertical sun.”¹

In 1821, when the fever broke out, for the first time, after the conflagration of 1804, the first cases appeared on the wharves, and soon after in Woodside Lane, distant from the first about sixty feet. This lane, according to Dr. Archer, is bounded on the east by a filthy dock, which serves as a common sewer for all the offal of the neighbourhood, and at low tide is bare and exposed to the action of the sun for a considerable distance from its head. The residents of this lane were mostly Irish, recently emigrated, and persons in the lowest circumstances and addicted to filth. “The fever extended gradually to Water Street, first around the head of the dock last mentioned, from thence to the lower end of the street in a block of wooden buildings under which the tide flowed, and equally unclean with those on Woodside Lane; and, in a very short time, embraced the whole of the lower section of the town from Main Street to the river.”²

Even in Charleston, where, from the number of narrow streets, confined lanes, dirty alleys, and neighbouring swamps, causes of insalubrity are said to be spread more generally than they are in such of our northern cities as are subject to the yellow fever, the disease has usually made its appearance in the neighbourhood of the wharves among the shipping, and in confined parts; after which it spreads more or less extensively over the city. These wharves are constructed of palmetto logs, and filled up with wood, stones, and earth. In the docks, a great deal of mud, with decomposed vegetables and other materials, are thrown up by the tide, and at low water the exhalations are offensive. Independently of this in some wharves the drains pour out their contents.³ *Whatever may be the views entertained of the efficient cause and mode of propagation of the disease by some of the physicians of Charleston, they all admit these locations to contain fruitful sources of injurious effluvia.

In 1817, the fever commenced, as we learn from Dr. Shecut, in the S. E. section of the city, at or near Lynch’s Lane, and progressed northwardly and partly northwestwardly to the market, and along the lower end of Church Street; of which places, it is to be remarked, and particularly of the former, that they are on made lands, having been formerly creeks, which intersected the city.

¹ Med. Repos., iv. 330.

² Archer, Med. Recorder, v. 62, 63.

³ Letter of Dr. Campbell, in Watts, p. 248; Simons, p. 18; Strobel, p. 181.

The fever of Mobile, in 1819, commenced on the water side, which is low, damp, and was at that time in a deplorable state as regards cleanliness. From the report of the committee appointed to investigate the causes of the disease, we learn that the wharves were in a fit condition to evolve injurious exhalations. They are "built with hewn timbers, closely laid, confining the water within the outward dimension of the wharves, and filled up with rotten logs, bushes, shavings, and other vegetable matter, covered lightly with swamp mud or earth, presenting to view an immense mass in the most noxious state of decay. Two of these wharves, about 450 feet in length, and thirty or forty in breadth, were commenced in the spring; and the work of filling them up with logs, mud, and bushes, was carried on during the summer, till the storm on the 28th July, and the sickness of the workmen, put a stop to it. They were, however, nearly filled up to the length and breadth mentioned, and to the depth of 4 to 10 feet, and the surface of about a third part, covered with pieces of swamp marsh, cut in convenient sizes for the purpose, and marsh mud. When the committee viewed these wharves, the sight was most disgusting, and the smell so offensive, that they felt their health endangered by delaying about them." The other wharves, five in number, were little, if at all, better. Water Street was also filled up with the same kind of materials to the depth of four to six feet, several hundred feet in length and fifty in breadth. The lots on one side of that street were filled up with rotten logs, green pine saplings, and pine tops, with a thin layer of earth. On the water side, the docks were clogged up with timber, and drift logs and old boats, great quantities of sea-weed and other filth in a state of decay, particularly under the stores standing over the water. To these must be added the badly constructed foundations of the stores and buildings near the river, retaining beneath them much unwholesome matter or stagnant water.¹ The epidemic of the same city in 1839 began among the shipping exclusively.² That of 1842 commenced and prevailed principally in Spanish Alley, "a very filthy place near the docks, where it would naturally be expected."³ Of the localities first invaded in 1853, and of their condition, I shall speak in another chapter. What will there be stated on good authority, will not impair the value of the statement made relative to the connection between the appearance of the disease and impure localities.⁴

The city of New Orleans, from the low, flat situation, and great humidity of the soil, throughout its whole extent, and far beyond, in all directions; from the narrowness, closeness, and the filth of the majority of its streets, and imperfect nature of the paving, added to the habits and occupation of a large mass of the inhabitants, approximates closely, in a hygienic point of view, to those parts of our other cities where the yellow fever usually originates and most extensively prevails; while the particular nature of the

¹ See extract from the Report, in Watts, pp. 272-3-4, and the document in extenso in Letters, &c., on Epid. of Baltimore, in 1819, p. 200.

² Monette, p. 118.

³ Nott, N. O. Journ., iv. 566, for Epid. in 1847; see N. O. Journ., v. 38; Lewis.

⁴ Report of Sanit. Comm. of N. O., 1853, p. 112.

soil, consisting, as it does, of a bed of trees accumulated together by the alluvions of the Mississippi, as well as the infiltration of the water at only a few inches from the surface; the nature, extent, and proximity of the cemeteries where the bodies are buried, not below, but above ground; the immeasurable swamps by which the city is, as it were, encircled; the immense surface of mud exposed to the desiccating power of the sun on the recession of the river; and the vast amount of shipping—ships, steamboats, and small crafts—which line the levee and fill the basins and are attended with the inconveniences incident on such accumulations, all tend to promote almost everywhere the evolution of injurious exhalations. Yet, it is remarked that, in this almost pestiferous city, the fever usually breaks out and rages paramount along or near the levee, among the shipping, in or near the basin, or in the closest, filthiest, or most crowded streets. The history of the epidemics of 1817, 1819, 1822, 1837, and 1839, as we derive them from contagionists or others, will prove this to have been the case.¹

The same connection between insalubrious localities, and the production and prevalence of yellow fever in New Orleans, is found to have existed in all subsequent epidemics—in 1846, 1847, 1853, 1854.² Of the condition of things in 1846, let the Report of the Board of Health, through Dr. Holt, chairman of the committee, tell. After remarking that a large number of the citizens had been induced to believe that yellow fever had disappeared, the Board speak of the unusually filthy condition of the city since the spring of the year—a fact which had been repeatedly noticed by the public press, and was a subject of conversation among all classes of the inhabitants—the Board say that no attention had been paid to the banks of the river, since the water began to fall early in the summer; and filth and garbage of every description, both animal and vegetable, had been deposited on the newly exposed alluvial bank to undergo a putrid fermentation beneath the burning rays of the sun at a temperature ranging between 130° and 140° (Fah.); that several vessels had to be moved from the wharves between Hospital and Barrack Streets, to the second municipality, on account of the insufferable stench arising from the bank of the river—that in the latter part of September and beginning of October, five or six captains and mates were taken ill on board of ships lying at those wharves, and, for the greater part, died. They add that complaints of stagnant and putrid water in vacant lots, and even under houses in the back streets, were made early in the season; that the stagnant and putrid condition of the water in the gutters, and the accumulation of filth at the street crossings, was a subject of general remark, and that the stench arising from the putrid filth was, in many instances, intolerable and overwhelming.³

¹ See Piccornel, in Thomas, p. 8, &c.; Thomas, p. 112; Girardin, p. 24; Chabert, p. 17, &c.; Carpenter, pp. 17, 18, 19, 25, 29; Rept. on Fever of 1819, by Com. of Med. Phys. Society, p. 5.

² Holt, Rept. of N. Y. Legis. on Quarantine, pp. 183, 184; *Ib.*, N. O. Journ., Jan. 1847, p. 467; Fenner, N. O. Journ., *ib.* p. 455; *Ib.*, Fever of 1853, p. 72; Dowler, Fever of 1853, p. 42.

³ N. O. Journ., iii. 437. See also p. 275.

Dr. Barton, in his late excellent report on the sanitary condition of New Orleans in 1853, calls attention to the connection in question, and after remarking that it is more apparent in northern cities, where there is such a difference in elevation, dryness, and ventilation, he proceeds: "Still, there are localities here where those differences exist to a notable degree, and which are the special hotbeds of pestilence whenever it exists, as in the neighbourhood of St. Thomas, Madison, and St. Mary's Streets, the triangle, about Gormley's Basin, some of the front streets of Lafayette, and finally the seventh ward. These are damp, filthy, crowded, and badly ventilated" (pp. 348, 367).

The fever of St. Augustine, in 1821, began and prevailed along the river in a low and damp soil, and in 1839 the sickness commenced both in the vessels lying in the harbour and in several buildings situated in the back part of the town.¹ At Natchez, in 1825, it commenced under the Hill.² At Alexandria (Miss.), and Vicksburg, in 1839 and 1841, it broke out at the low parts of these towns near the steamboat landings.³

In the latter place, in 1847, the fever commenced at a distance from shipping, and in the upper part of the town, and west of the market house. "The square was composed almost entirely of old wooden buildings in a decayed and dilapidated condition, and the putrid effluvia which was perceptible in every part of it, was almost intolerable, besides which, a great deal of grading and filling up had been going on for several weeks on all the streets around this square, which presented an immense deposit of fresh earth, from which a constant and most unwholesome exhalation must have issued, produced by the intense heat which prevailed at the time."⁴

The same remarks are applicable to the fever of Galveston (Texas), of the Bay of Biloxi, of St. Louis, in 1839, and of Pensacola in 1765 and 1822, and of Augusta (Ga.), in 1839,⁵ and Savannah in 1820.⁶ At Selma, in 1853, where the fever was undoubtedly of local origin, it was remarked, that with few exceptions, those who took the disease were in the habit of resorting to that part of Water and Broad streets where the first cases had occurred. In a future chapter we shall see what was the condition of that part.⁷ The first case which occurred at Centreville (La.), in 1853, was a mulatto man, a cooper, working and sleeping in a shop on the banks of the T  che. For several years, all the chips and shavings from this shop had been carried on to the low places along its banks, and into the stream to the distance of forty or fifty feet, forming a sort of wharf for the landing of the steamboats. This man had no intercourse with any person affected with the disease, nor with any place where it prevailed.⁸

¹ Strobel, pp. 131, 152, ; Monette, p. 123.

² Merrill, N. O. Journ., ii. 217.

³ Monette, pp. 104, 106, 110.

⁴ A Hist. of the Epidemic, which prevailed in Vicksburg during the fall of 1847, by A. L. C. Magruder, M. D., N. O. Journ., May, 1848, p. 690.

⁵ Rept. p. 28.

⁶ Waring, p. 11.

⁷ Report of N. O. Sanitary Commission, p. 105.

⁸ Wood, in Fenner's Rep. Tr. of Assoc., vii. 502; see also Rep. of N. O., 1853, pp. 60-1.

From all that precedes, it may be perceived that the yellow fever of Philadelphia, in originating and producing its greatest ravages in low, damp, close, and filthy situations, does not exhibit a phenomenon peculiar to itself; that the occurrence of the disease under circumstances of this kind is not to be viewed in the light of a coincidence; and that the disease must necessarily be considered as in some way connected, both as regards its rise and its progress, with localities of the nature and condition mentioned. Else how could we explain the fact that in other parts of the United States, the chosen seats of origin and greater prevalence of the fever, have almost invariably been localities in every way analogous to those affected here. That the yellow fever has occasionally been found to prevail in situations which are high and generally salubrious, and secured against the injurious influences of wharves, docks, shipping, &c.—as at Natchez, Washington, Woodville (all in the State of Mississippi), in Germantown, near Philadelphia, and various spots in Louisiana, Alabama, and elsewhere—is true; but in admitting a connection between the origin and prevalence of the disease and the existence of localities, of the nature described, with the evolution of the exhalations resulting therefrom, I am far from wishing to represent such localities as the only ones containing the materials required for the elaboration of the poison giving rise to the effect in question. For though, as a general rule, it may be affirmed that this poison is found to arise about wharves and the like, and in the low and confined parts of towns, there is no reason to deny the possibility of its being produced elsewhere, particularly when we discover there some of those usual sources of exhalation which, when combined with a peculiar constitution of atmosphere and placed under the influence of the meteorological agencies formerly described, are proved by experience to possess an injurious tendency.

As regards Germantown, enough has already been said to show that the locality affected was of a nature such as to assimilate it to the usual seats of the disease elsewhere; and the history of the fever of Natchez, Washington, and Woodville, as given by Drs. Perlee,¹ Merrill,² and Cartwright,³ certainly exhibited a state of things—large accumulations of filth, masses of vegetable and animal matter in a state of putrefaction, extensive digging, filling, and other derangement of the soil, for the purpose of levelling, &c., by which large surfaces were left, after being wet, to the desiccating agency of a sun of unusual power—which, in the minds of unprejudiced inquirers, will leave no room to doubt that there the sources of an impure and injurious atmosphere were sufficiently ample to give rise to an effect similar to that produced here and in localities of kindred character; while the few instances in which the occurrence of the fever appears to be unconnected with agencies of the kind, and to be due to causes which elude our observation, must be regarded as of an exceptional character, and classed with those in which the ordinary forms of malarial fever break out and spread extensively in spots, very unlike those universally regarded as the natural abode of those diseases. We all know

¹ Philad. Med. & Phys. Journ., iii. 3.

² Ibid., ix. 240; N. A. Journ., ii. 217.

³ Medical Recorder, ix. 3, 225.

that the localities usually affected, are characterized by a geological formation of tertiary and cretaceous secondary deposit with argillaceous and rich alluvial soil, more or less impervious to water; or where, whatever be the appearance of the soil, water is found at a very short distance from the surface; or where the latter is dotted with marshy fields, and traversed by sluggish streams; or by swampy, low, flat level land, as along the banks of lakes and winding streams; or by level plains, ravines, or deep valleys, either dried or drying on the surface, after having been thoroughly wet, and perchance encased between mountain elevations, covered, as in India, with jungles; or, in the West Indies, with impenetrable mangroves or thick forest, and containing a large amount of organic remains. In localities thus characterized, we may almost certainly count on meeting with malarial fever in some one or other of its varied forms, after a certain continuance of high atmospheric heat. In others, differently circumstanced, we may safely anticipate an exemption from that disease. And yet, it would not be difficult to point out instances of remittent and intermittent fever breaking out in places remarkable for aridity and want of water, and almost destitute of vegetation. These also are exceptions to a rule, and if from their occurrence, we are not justified in refusing to acknowledge the existence of a close connection between swamps, marshes, river shores, and the like, and periodic fevers, and in refusing to admit the association as effect and cause between these, and the exhalations issuing from those surfaces; it would be unsafe to conclude from the occasional appearance of the yellow fever, in high, dry and clean localities, that its origin is in no way connected with the condition of places differently situated; that it is not due to a cause appertaining specially to these localities, and must be the effect of an imported contagious poison.

It is not in this country only that facts of the kind mentioned have been noticed. What I have described as occurring here, has been observed in various parts of Europe and in tropical climates, thereby establishing still more conclusively the connection between the origin and prevalence of the yellow fever and localities of a nature and condition calculated, when placed under the influence of certain meteorological agencies, to produce an impure and injurious state of the atmosphere. The fever of Cordova, in 1804,¹ was confined within the limits of the low parts of the city, in ill-ventilated, crowded, and filthy streets, in which are situated a number of factories. At Espajo, also in 1804,² the quarter of St. Sebastian, to which the fever was almost completely limited, is relatively low, exposed to the reflection of the sun, ill ventilated, and horribly filthy, both in consequence of its position and the squalidness of its inhabitants. The same may be said of Montilla,³ of Asco in 1821,⁴ Mequinza,⁵ and Nonaspé.⁶ The yellow fever of Gibraltar has usually prevailed about the lower declivity of the rock, and here, or when it

¹ Chervin, *Mal. Sanit.*, p. 26.

² *Ibid.*, p. 27.

³ *Ibid.*, p. 28.

⁴ Pariset, p. 61; Chervin, *op. cit.*, p. 30

⁵ Candallero, in *Decanas de Medicina*, vi. 413; Rochoux, p. 141; of Tortosa, O'Halloran, p. 113; Malaga, *ib.*, p. 126; Puerta di Santa Marie *ib.* p. 134.

⁶ Chervin, p. 30.

appeared in higher situations, as occurred in 1804 and 1813, it has usually been more general among the lower classes, particularly where the population was dense, the houses badly ventilated, where due attention to personal and domestic cleanliness was neglected, and where the localities were of such a nature and in such a condition as to produce a contamination of the air. Or again, it has shown itself in or near a line of the drains, which, as well as other sources of exhalation, appear to have been prolific of injurious effects. On this subject, the reader will be fully satisfied on turning to the writings of Drs. Hennen,¹ Amiel,² Smith,³ P. Wilson,⁴ and Chervin,⁵ and to the reports of the various commissioners appointed to examine into the causes of the epidemics of that place, all of whom have pointed out the connection, as existing in that city, between the appearance of the fever and the various causes of insalubrity on which I have dwelt, and prove, beyond a doubt, that whenever the yellow fever has made its appearance in Gibraltar, it has always commenced in the filthiest and most crowded spots, among the lower and more disorderly classes of inhabitants, and in the vicinity of the drains and other sources of impurities. In 1804 and 1813, Boyd's Buildings were the first spot on which it showed itself. These covered a space of 29,200 cubic feet, and stood about 200 feet above the level of the sea, in the central part of the town, on ground which appears originally to have been washed from the mountain down two gullies, at the bottom of which they were situated, and the streams of which they obstructed in the rainy season. These buildings were formerly subdivided into small, dirty, and ill-ventilated tenements, into which individuals of the lower classes were generally crowded to excess, and were considered for a long time as the filthiest spot in Gibraltar.

In 1814, the cases which first alarmed the garrison occurred at Cavallero's Buildings, situated at Arengo's Gully, and at the top of the central part of the town. "Those buildings competed at the time with Boyd's, for want of cleanliness. They were inhabited by about 300 Portuguese, of the lower order, and, close to them, there was an accumulation of filth (one of several deposits in the place), which emitted a very offensive stench and attracted an incredible swarm of flies, which, infecting the whole neighbourhood, became at the time the subject of general observation and surprise."⁶ As a further example, I shall cite the epidemic of 1828, when the fever broke out in what is called District No. 24, situated at the southeast angle of the city, and surrounded by hills or walls. On this subject, Dr. Hennen remarks: "The whole of these cases, as I am informed by Mr. Wilson, of the Civil Hospital, are in the line of a drain which comes from Mr. Martin's house, and within

¹ Topography, p. 46, &c.

² Edinb. Journ., xxxv. 263; Johnson on Tropical Climates, p. 257.

³ Ibid., p. 15.

⁴ Page 29, &c.

⁵ Lettre à M. Monfalcon, p. 10; Réponse à M. Guyon, p. 10, &c.; See also, Humphrey, Edinburgh Journ., No. 46; Bancroft, p. 473, Engl. edit.; Letters to the Lieutenant-Governor, published by Mr. Frazer, Med.-Chir. Rev., xiii. 337, &c.; Gillkrest, Cycl. Pract. Med., ii. p. 291; Second Quarantine Report, p. 163.

⁶ Amiel, Edinb. Journ., xxxv. 263.

a few yards of the doors of the tenements where the individuals have been taken ill there is a large open grating, from which very offensive vapours are said to arise; and Mr. Wilson states to me that it is hardly possible to be otherwise, as no fewer than four privies empty their contents into the drain." In another letter, Dr. Hennen, states that he has minutely examined District No. 24, and adds that at almost every step he took he had reason for surprise, "not that fever had broken out there, but that it had not extended further; from whatever causes it may have proceeded, the pauper population is dense to a degree incredible, except to those who have seen it. In sheds without ventilation, without drainage, and generally composed of the slightest materials, in tiers of beds as close as in a crowded transport, numerous individuals sleep; they go out to their work at an early hour, and return at gun-fire, locking up their miserable places of nocturnal shelter during the day, and leaving them saturated with the steam of their bedding, their food, and the overflowing receptacles of their ordure."

Cadiz, at the highest point of its surface, is situated only forty-seven English feet above the level of the sea, and at its lowest, where there is but little declivity, at only nine feet.¹ In 1800, and the subsequent sickly seasons of 1804, 1810, and 1813, the fever broke out in this low, moist situation. From Arejula² we learn that in the first-mentioned year the disease remained long stationary in that quarter, which is denominated Santa Maria; and Berthe (p. 52), who repeats the statement, adds that there the streets are narrower and dirtier than in the rest of the city, and inhabited by sailors, workmen, and custom-house officers; that the houses are not only smaller, but more crowded; that the mortality was there greater than elsewhere; and that the disease prevailed there longer than in other parts of the city (p. 162). Of this quarter, Mr. Doughty, in his account of the epidemic of 1810, gives the following picture: "From the time I took up my residence in the St. Elena Barracks, situated in the above quarter, and not more than fifty yards from the back of its several lanes and buildings, it was my custom, in my daily visits to the Hospicio and the San Juan de Dios Hospitals, to pass through the narrow streets of this quarter, and I particularly observed the striking difference, as to cleanliness, in this part of Cadiz, where the poor classes dwell, to what is found in the open streets facing the sea, where the mercantile and higher orders of society reside. In the latter, there was nothing offensive to either sense; while in the former the olfactory nerves were assailed with the most noxious exhalations, and the eyes disgusted with every sort of filthy and excrementitious matters thrown indiscriminately into the streets. Fish-bones, rotten vegetables, and rotten matters of every description, mixed together with the contents of the receptacles of the night, formed the covering of most of those extremely crowded and ill-ventilated streets." Well could Mr. Doughty ask whether, if Cadiz were built of adamant, and its streets covered from time to time with matters of this description, on which the solar influence might operate to a degree of heat equal to 95° or 100°, often

¹ Fellowes, p. 1.

² Feb. Amarilla, p. 346; Bancroft, Seq., p. 303, note.

experienced out of the shade in that city in the summer months, there would not be just grounds to expect the generation of fever. Dr. Chervin has proved, on the authority of several native physicians and others, that at the time of the several epidemics of Cadiz the air of the infected district was highly tainted.¹

A distinguished writer of our own country, who visited Andalusia in 1805, for professional purposes, the late Dr. Pascalis, of New York, remarks: "The whole city (of Cadiz) is traversed by sewers, which are cleansed by the tides. When the east or Levant wind blows, the water is carried off from the port; the tides are leeward, and can no longer wash the filth from the sewers. Sometimes, in the greatest heat of summer, this violent wind blows fifteen or twenty-one days without intermission, and pestiferous gases from the filth of the city are continually emitted through the air-holes of the sewers."²

The epidemic of Seville, in 1800, first declared itself in the suburb of Triana, on the southwest of the city, and only separated from it by the Guadalquivir. It is inhabited by mariners and workmen. M. Berthe³ states, that when once introduced in this suburb, the fever was confined to a small space for more than fifteen days. It then spread all over, caused much mortality, extended to the suburb of Los Humeros, directly opposite Triana, and which, like the latter, adjoins the river, and is very low and filthy, and inhabited by the lower classes of the people. In Seville itself the disease prevailed most extensively and fatally in low situations, and filthy, narrow, and crowded streets and houses.⁴ Similar observations were made at Xeres, Port Sta. Maria, and many villages, the disease being more prevalent and fatal in proportion to the lowness and humidity, as well as the crowded state of the situations.⁵

The fever of Seville, in 1819, broke out in the quarter of Santa Cruz. Pariset, in his fantastic work already mentioned,⁶ states, that this quarter made a strong impression upon him from the singular nature of its structure. It is composed of a mass of houses, of moderate height, whitewashed, like all other similar buildings in Andalusia, but separated by tortuous streets, so narrow as not to afford room for the passage of two men abreast. "There are indeed turns and angles where I touched with my elbows the houses of both sides of the street." "These narrow lanes are paved with pebbles, which are easily detached and form hollows in which the filth accumulates; so that the city is considerably dirtier in that quarter than it is in any of the others. One may readily understand that, in streets so narrow, tortuous, and irregular, which had already so greatly surprised me in other cities, and particularly at Cordova, the atmosphere is scarcely ever renewed; and that during this long repose, which nothing disturbs, it has full time to be-

¹ *Fièvre Jaune d'Espagne*, p. 22.

² *A Statement of the Occurrences during a malignant Yellow Fever in the City of New York, during the Autumnal Months of 1819*, p. 7.

³ *Précis de la Maladies*, &c., pp. 59, 60.

⁴ *Ibid.*, p. 163.

⁵ *Ibid.*, p. 161; Chervin, *Fev. of Spain*, p. 63; O'Halloran, p. 153, &c.

⁶ *Observ. sur la Fièvre Jaune Faites à Cadiz en 1819*, pp. 21-2.

come saturated with all the vapours evolved from substances and offals of all kind. Add to this, that during very hot weather, the inhabitants of the quarter of Santa Cruz are accustomed to stretch awnings across from one house to another, so as to intercept the light and temper the ardour of the sun. Thus plunged in the shade, they are equally so in their own emanations. Now, it must follow, that if the inhabitants are crowded in dwellings so constructed, their animal emanations evolved by the temperature, and accumulating without end in an atmosphere stagnant and already charged with other vapours, these dwellings will acquire the greatest aptitude, not only to receive and propagate, but also to produce spontaneously the most fatal diseases."

Of Malaga, it is admitted by no less a contagionist than Sir James Fellenows, that, "even with the least torrent of the Guadalmana, the streets are overflowed, which, upon the waters retiring, are left full of mud and clay" (*Fellenows*, p. 158); and that the people there, contagionists as they all were, thought that such occurrences could have an influence in occasioning the disease, we may infer from the fact that Mendoza, in his history of the epidemic of 1803, '4, says that since that period measures have been taken to prevent the water entering the lower part of the town.

The memorable epidemic of Barcelona, in 1821, is well known to have commenced in the port of Barcelonette, and to have subsequently extended to the city proper, committing its greatest ravages in low, crowded, and filthy localities, inhabited by the lower classes of society. That such localities existed, and, together with the filth incident to a crowded seaport, tended to contaminate the atmosphere, may be gathered from the description of the events furnished by Roehoux,¹ O'Halloran,² and Chervin,³ and which are summed up in the following statement made by competent professional authorities: "From the neglect of the public police for many years, the sewers, drains, canals, and other channels for carrying off the impurities of the city, have been choked up or become foul to such a degree that, towards the end of June, it was impossible to pass by the sea wall, where they are discharged into the harbour, without being incommoded by the stench of accumulated and putrefying animal and vegetable substances. Notwithstanding the work which was executed some years ago in the bed of the Condal, the shallowness of its water, the slowness of its course, the constant action of an ardent sun throughout the day—all these causes produced a stagnation of the impurities of the city, and rendered their discharge difficult, giving occasion to deleterious emanations from all points of the canal. The careful examination of the committee charged with cleansing the port, has shown that this water-course was obstructed at its mouth by a band of sand, which, hindering its discharge, had occasioned a considerable collection of stinking water, the product of various manufactures, slaughter-houses, wash-houses, and other

¹ Rech. sur les dif. Mal. qu'on appelle Fièvre Jaune, pp. 86-7.

² Yellow Fever of the South and East Coasts of Spain, pp. 6, 7, 12.

³ Fever of Spain, p. 76; Report, p. 54.

establishments situated on its banks exhaling an insufferable stench. The same committee found that the foul water stagnant round this sand bank was one foot higher than the level of the sea, and more or less in other places. The modern works of the port have converted it into a sort of stagnant pool, of which the cleansing has been neglected for several years, producing a focus of infection which had not previously existed." Add to this, that the disease prevailed more extensively, and proved more fatal, in the neighbourhood of these impurities, and that it, as also the mortality, lessened in proportion to the distance from these.¹

Finally, at Leghorn, in 1804, the disease began in the worst ventilated and most unhealthy part of the city. Surrounded on all sides by lofty buildings, defended from every wind, filthy from the offals of filth and butcher meat, and filled with stagnant putrid exhalations, it seemed as if it were contrived to be the seat of such a disease. At first slow in its progress, and almost unobserved, it next spread in other directions; but always gradually, and with difficulty, along certain narrow streets and lanes, ill ventilated, with high, confined, and filthy houses, crowded with inhabitants of the poorest classes.²

The result is not different in tropical climates, for there, as elsewhere, the disease is confined in all the island to the sea-coast—can only spread into the interior of continents where the country is flat and low, possessing little elevation above that level, and retaining a temperature such as has been pointed out in a preceding chapter; and is uniformly more apt to appear in places calculated, by their position, their hygienic condition, the nature of the soil, and other contingencies, natural and artificial, to give rise to the evolution of atmospheric impurities.³ We have already seen that in the West Indies, as with us, the disease generally occurs in cities, towns, or villages, or in situations containing a large number of individuals collected together within a narrow compass. If in our latitudes we discover in the construction of the cities and towns visited by the disease—in their peculiar location, in the nature and condition of many of our streets, and in the state of our ports, ample cause for the production of atmospheric impurities, we shall, on examination, find that matters cannot take a different turn in the West Indies; and that there also a connection exists between the evolution of such impurities and the rise and progress of the yellow fever. Dr. Ferguson remarks, that the generality of West India towns, and consequently of places for the garrisons of the troops, are situated on the leeward shores of the country, at the bottom of the deepest bays that can be found, as a protection to their trade against the winds from the sea. The soil of such localities must always be alluvial, and is often marshy. Nine-tenths of the towns are inclosed by high hills rising immediately behind them, which exclude the

¹ Manifest, Maclean on Quarantine, p. 129; *An. de la Méd. Physiol.*, i. 420–1.

² Palloni, p. 29; see also *Edinb. Med. and Surg. Journ.*, ii. 89.

³ Ferguson, *Med. Ch. Tr.*, viii. 138; Pinckard, ii. 487; Pugnet, p. 331; Dariste, p. 34; Lefort, p. 9; Kéraudren, p. 23; Lempriere, ii. 4, &c.

breeze which, in its natural course, ought to reach them from the windward side of the country; and if, besides, we bear in mind that their elevation is generally little above the level of the sea, we shall have abundant reason for concluding, that if the highest degrees of reflected tropical heat, and defective ventilation, when combined with marshy soils, or the dryer alluvial ones under the agency of heavy rains, can give rise to atmospheric impurities of an injurious character, such impurities will be the more readily found in localities like those described, and “which, so far from being corrected by an enlightened police of towns, are often forced into unnatural activity by the utter disregard of those regulations of cleanliness and order, that all well-governed communities take pride in observing.”¹ To this it must be added, as a further indication of resemblance of these towns to those infected in temperate climates, that they are generally seaport towns, where are collected ships and sailors—so often, indeed, as almost to justify the assertion that it seldom occurs elsewhere;² that the fever usually breaks out and prevails—as might indeed be inferred from the name formerly given to it, *fièvre metelotte*—more extensively among or in close proximity to the shipping; and that in whichever town it may occur, the disease, as experience shows, exercises its most baneful influence in low, close, and ill ventilated situations—as small streets, lanes, alleys, the lower stories of houses, and the like—in all which we are more certain to discover some of the many causes of impure air, which, when aided by the meteorological agencies already specified as existing in greatest abundance in tropical climates, must necessarily give rise to injurious results.³

The history of even the earliest epidemics on record furnishes abundant proofs of the correctness of the statement here made; for it shows among other circumstances that the disease has usually broken out in the harbours of the towns visited, among the shipping and in the parts adjoining. Such was the case in Martinique in 1690, when to the ordinary sources of impurities incident to a West India seaport, was added the effluvium resulting from a large quantity of putrid meat; in the same place at subsequent periods;⁴ at Port de Paix and Léogane in 1691; at Cape Haytien in 1696, 1705, 1733, 1734, 1743, and 1755;⁵ at Carthage in 1729, 1730, &c.,⁶ and such will be found to be the case up to the present day in these and other localities of tropical climates. In all, sources of atmospheric impurities arising from the vicinity of shipping, from peculiarity of position, from the nature of the soil, &c., no less than from the absence or neglect of proper hygienic regulations continue to abound. Take we Vera Cruz for an example, we shall find that those causes of insalubrity exist both within and beyond the walls of the city. The latter is built on a low, level, sandy, and barren soil. The population, as we learn from Humboldt, is exceedingly crowded; the external walls, which are high, impede the free circulation of air; during the summer, the S. E. and E. S. E.

¹ Ferguson, Med.-Ch. Tr., viii. 127-8.

² Dariste, p. 34; Lempriere, ii. 32.

³ Chervin, in Rufz, p. 94.

⁴ Chanvalon, Voyage à la Martinique fait en 1751, p. 148.

⁵ Moreau de St. Méry, ii. 534, 701.

⁶ Ulloa, i. 41.

wind, which at best is feeble, is felt only on the terraces of the crowded houses; and these, as well as the streets, being usually in a filthy condition, it follows that the inhabitants are immersed in a stagnant, overheated, and impure atmosphere, highly detrimental to the health of strangers unaccustomed to its effects.¹ The port, besides, is crowded with vessels from various parts of the world. While such are some of the sources of impurity existing within the walls of the city, those in the immediate vicinity of the latter are not less abundant; for the sandy plains by which the city is surrounded, so far from being arid, are intersected with marshy depressions, in which luxuriant numbers of shrub trees, and among these the mangrove, whose roots, like those of kindred plants when uncovered and exposed to the action of the sun, have been found by Humboldt and others to evolve emanations highly prejudicial to health.²

At Allenton, Key West, where nearly all the cases occurred during the epidemic of 1824, many sources of impure air were found to exist, and gave rise to emanations of the most concentrated and powerful kind. The surface of the whole island is level and low—a large proportion of the interior is occupied by lagoons or ponds of stagnant water and marshes. The former containing a great quantity of dead animal and vegetable matter, and the latter being covered with a rank vegetation. Besides that, there is an immense quantity of marine substances, both animal and vegetable, which are thrown up upon the beach, and from which evolve the most offensive effluvia. Allenton, itself, is situated on a part of the island most abundantly supplied with materials for decomposition; its site is the lowest and most level portion of the island that is not covered by water, and is deprived of the grateful influence of the trade wind and sea breeze.³ Add to this, that the public buildings occupied by the men among whom the fever prevailed, were situated in the midst of these localities, and were of a kind and in a condition well fitted to concentrate and activate, if not to produce injurious emanations.

The city of Havana, as we are told by Dr. E. H. Barton, is situated on a closed bay of six or eight miles in circumference, land-locked on every side with lofty hills, with the exception of one narrow outlet to the sea, at the north, with marshes about the estuaries of the several small streams that empty into it, bringing the organic detritus of the surrounding country, mixing its fresh with the salt water of the ocean, occupying near two-thirds of the marginal circumference of the bay.

This bay receives all the filth of a city containing near two hundred thousand inhabitants—is in many places very shallow, exposing, at low tide (the tide here being three to four feet), extensive surface, with all kinds of putrefiable materials, to the sun. The water of the bay is often very offensive. All vessels pump their bilge water into it. It cannot be changed; it is so

¹ Humboldt, *op. cit.*, p. 764; Luzuriaga, *De la Calentura Biliosa*, i. 65. See also Thorne, *An Account of the Situation and Diseases of Vera Cruz*; *Reposit.*, iii. 46, 77; Barton, *Sanit. Condition of New Orleans*, 1853, p. 370.

² *Ibid.*, *op. cit.*, p. 763; *Personal Narrative*, iii. 190, 392; Barton, *Rept. on Sanit. Condition of N. O. in 1853*, p. 370.

³ Ticknor, *N. A. Journ.*, iii. 213, 215, 217.

full of decomposing materials that the British naval service has a standing order not to use the water for any purpose on board their ships of war. From experiments made with it, it putrefies on standing a *single* day, while sea water, taken at a distance of fifty leagues from land, requires *three* days. The streets of the city proper are very narrow (about twelve feet wide), and very badly ventilated, from being irregular and very crooked, and there is a high wall still further obstructing it. The habits of the mass of the people are of the very worst description, and from the high price of food of every kind (from the heavy tariff imposition of a despotic government), the mode of living is wretched in the extreme. That yellow fever should exist here every year, is not at all astonishing, with a high temperature and great moisture.¹

The island of St. Domingo is not less calculated to be the seat of injurious exhalations, nearly two-thirds of it consisting of salt marshes, filled with mangroves and crustaceous animals and insects of various kinds, on a level with the sea, and covered by it at each return of high tide.² “The mixture of these substances,” remarks Desportes, “constitutes the focus and the matter of exhalations, which poison the atmosphere, and the bituminous odour of which sufficiently points out its injurious quality.” The city of the Cape, situated at the foot of a mountain which shelters it on the north and west, is skirted on the south side by one of the above-mentioned salt marshes, which extends to a distance of more than half a league, and, like all the others, is overflowed, at every tide, by the sea; and as, within the tropics, these tides do not attain the same height during the summer solstice as in winter, it follows that in the former season this marshy surface is left partially uncovered and exposed to the action of the sun, and necessarily becomes the seat of noxious exhalations. This is an effect greatly aggravated in seasons marked by the existence of droughts.³ To the exhalations arising from these localities, and the increase of which, as formerly seen, is attended with an increased prevalence and malignancy of the fever, and to those arising from the accumulation of ships in the port, where the disease often commences and rages,⁴ were added others at the time of the memorable epidemic of 1802; for the French army which arrived at that period, and was doomed to be more than decimated by the disease, found the town destroyed by fire. From the ruins of the houses and the masses of merchandise exposed to the decomposing effects of the atmosphere: as well as from the crowded cemetery, there arose, towards morning particularly, mephitic miasms of the most offensive kind.⁵ The other towns and localities of the island in which the fever prevailed, and spread so extensively and with such marked virulence during their occupancy by the British army about the close of the last century—Port au Prince, Fort Bizoton, St. Mark, St. Nicholas Mole, l’Arcahaye, Jérémie, &c.—are, for the most part, in a condition not unlike that of the

¹ Sanit. Rept. N. O. 1853, pp. 369, 370.

² Desportes, i. 16; Gilbert, p. 11; Bally, p. 347.

³ Desportes, pp. 51–2; Bally, p. 347; Gilbert, p. 80.

⁴ Desportes, pp. 55, 166.

⁵ Gilbert, pp. 70–1.

Cape, being often located amid, or in the vicinity of saline marshes, possessing porous or calcareous soil, beset with mangroves. In many cases they are encased between mountains which impede the free circulation of air and concentrate the heat, and, in general, situated on the margin of the sea.¹

In Jamaica, Lind informs us, the number of English sacrificed to the climate was formerly hardly credible, and only to be guessed at from the common computation. Until lately that it has become healthy, this island buried to the amount of the whole number of its inhabitants once in five years. There, to this day, epidemic fever prevails more frequently than in other West India stations; and there, also, sources of exhalation are found in abundance on the plains and sea-coast. These exist, either in the shape of mangrove marshes, or, rather, wet and sandy surfaces, covered with those and other kindred shrubs which grow and run to decay rapidly, and constitute a kind of water forest—the lower part undergoing decomposition, while the upper is in a state of luxuriant growth and beautiful verdure, or are furnished by the filth of the towns, or that incident to shipping, or crowded population or barracks. Such is the case at Port Royal, Kingston, Fort Augusta, Spanish Town, Savannah, La Mar, &c.²

From the nature of the localities around the town of St. John's, in the island of Antigua, and the condition of the streets, houses, and port, we may infer that it is often the seat of exhalations highly injurious to individuals unaccustomed to their action. Such was the case at the time of the epidemic of 1816, described by Dr. Musgrave,³ and that of 1835, for an account of which we are indebted to Dr. Furlong;⁴ both of which broke out at the bottom of what is called the Point, where the washings from the upper part of the town accumulate, and the locality of which must strike the most careless observer as being, even under the most favourable circumstances, a hotbed of malaria. Of this Point, Dr. Musgrave tells us that the houses are not merely exposed to currents of air which have previously traversed a marshy surface, but absolutely standing in a swamp. Three or four streets cannot with safety be passed on horseback, and the path which those on foot are obliged to select with the utmost circumspection is afforded only by artificial ground. The house from which the first two cases emanated forms one of a row terminating the town to the northwest. From the very threshold of the door, stretching to the northward and northwest, an extent of marshy ground proceeds for nearly two miles, and also to the northeast for a considerable, though not so great a distance; and as three or four streets to the southward of this are absolutely built in a swamp, the inference is plain that from whatever quarter the wind may blow, it will bring with it

¹ R. Jackson's *Outlines*, p. 52, &c.; *Ib.*, *Sketch*, ii. 443; John Wilson, p. 135, &c.; Pinckard, ii. 428; H. McLean, pp. 9, 10, 11.

² Tullock's *Rept.*, pp. 46, 53, 55, 56; Lempriere, i. 61, 63, 84, 117, 123; ii. 34; J. Hunter, p. 14, &c.; R. Jackson, ii. 428; Doughty, p. 3; Wilson, pp. 163-4; Arnold, pp. 139, 157, 164.

³ *Med.-Chir. Trans.*, ix. 101.

⁴ *Med.-Chir. Rev.*, xxv. 289; *Report of Dis. and Mort. of Brit. Army*, p. 34.

a noxious impregnation—nay, from the imperfect manner in which these huts are constructed, miasmata must spring up from beneath the very beds they contain. “Perhaps,” says Dr. Furlong, “there is no tropical town more favourably situated for the production of malaria than St. John’s; it is surrounded on three parts by gently rising hills, and consequently is a receptacle for the *debris* and all kinds of malaria engendering matter from the surrounding acclivities.”

Equally evident were the sources of exhalations at Montserrat, at the time of the formidable epidemic of 1821. “In every corner of the town, but more particularly in the immediate vicinity of the house in which the epidemic first manifested its presence, heaps of animal and vegetable filth commingled were, to the disgrace of the police, suffered to accumulate.”¹ The condition of Georgetown (Demerara), where the fever prevailed extensively and fatally in 1803–4, 1819, and 1837–’42, is not more favourable than the preceding. The town is intercepted by numerous dykes, embankments, and canals, in which the water sometimes remains stagnant for entire months, and during the rainy season it is exceedingly wet and marshy, and even overflowed; the surrounding country being very flat and level, and covered with weeds.² At all times, the town, and perhaps more particularly the harbour, where the fever prevailed, is in a filthy condition. During the epidemic of 1803 and 1804, this was evidently the case; for Dr. Frost, in his account of the disease, speaks of the “filth and nastiness that American vessels, particularly, collect from their cargoes on their outward-bound passages, which cargoes are composed principally of salted fish, meats, both dry and pickle salted, lard and butter, cheese, onions, potatoes, live stock, such as oxen, horses, &c.”³ It was no better in 1837, as Dr. Blair’s excellent volume on the epidemic of that year clearly shows. After alluding to the sparse position of the houses, Dr. B. states that two portions of Georgetown were and are exceptions to the general description, viz: the mercantile parts, Water Street and Robb’s Town. There the houses are nearly in contact, and, instead of a construction of open pillars or uninhabited cellars, the ground floors are used and occupied during the day as shops or stores. It was in this portion of the city that the epidemic of 1837 was most prevalent, as it was in the mercantile part of the town (America Street) that the fever chiefly prevailed in 1819. Robb’s Town has no peculiarity, except that of the houses being huddled together; but Water Street has others. Its highway is formed by the embankment that prevents the overflow of the river tide, and one side of the street, comprehending stores, dwellings, kitchens, stables, &c., is built on the bed of the river itself, supported above the level of the water by piles and platforms. This side of the street is designated the *mud lots*, or Water Street. To almost every “mud lot” there is also attached a *stelling* or landing

¹ R. H. Dyett, *Med.-Chir. Rev. and Journ.*, iv. 1003.

² Tullock’s *Rep.*, p. 14; Chisholm, ii. 200, 203; Ferguson, *Recol.*, pp. 195, 196; Pinkard, i. 332, &c.; R. Jackson, ii. 403, 407, 415, 420; Blair, *Some Account of the last Yellow Fever Epidemic of British Guiana*, pp. 2, &c.; Frost, *Med. Repos.*, xii. 209, 210.

³ Frost, *Op. cit.*, xii. 226.

wharf, composed of piles, with a platform from four to seven feet broad, and extending beyond the buildings, into the shelving alongside of the river. Across Water Street, six sluices discharge the drainage and sewerage past the stellings into the river. It will be perceived, as Dr. B. remarks, that by the system of stellings over the mud lots of Water Street, the current of the river, for about two miles in length, is subjected to a kind of coarse filtering, and that bulky materials of any kind, floating among the piles, are necessarily entangled and detained below the stellings (pp. 2, 3).

Roseau, the capital of the island of Dominica, is built on the south side, on a low patch of land, which, by its projection into the sea, forms two small bays. Portions of the surrounding land are level, and in wet weather converted into swamps. Mount Bruce, a table-rock, about 450 feet high, overlooks the town, from which it is about a mile distant. This hill is almost surrounded by two deep and winding ravines, one of which forms the bed of a mountain torrent, the other contains many patches of moist and marshy ground.¹ According to Dr. James Clark, the town contained formerly a number of swamps, which occasioned great unhealthiness. These, however, were filled up, with decided effect in diminishing the fever; which, nevertheless, prevailed with great fatality in 1793, '4, '5, and '6; next, in 1817-'21, 1837-'8; and, lastly, in 1842.² In all these epidemics, the fever commenced near or in the harbour, which, like every other such locality in tropical climates, furnishes an ample supply of materials for the evolution of febrific poisons; while from the marshes, covered with brushwood, situated in some of the neighbouring ravines, from the nature of the barracks, and the peculiarity of the soil, we can have no difficulty in understanding that Mount Bruce will, under certain conditions of atmosphere, become the seat of similar emanations.

There is little doubt that the early Spanish adventurers suffered from fever, though we do not hear of the occurrence of epidemics among them, probably from want of denseness of population or other causes. The first settlers at Darien, as recorded by Peter Martyr, paid the penalty of having selected an improper locality. They settled, in 1512, at Santa Maria, Antigua, where they lost many of their associates; the air, according to Peter Martyr, being there more pestilential than in Sardus (Sardinia). The European inhabitants of the place he represents as pale and yellow, "like unto them which have the yellow jaundice." This he ascribes to the local circumstances of the place—"situated on the banks of the river of *Dariena*, in a deep valley, and surrounded on every side with high hills, by reason whereof it receiveth the sunbeams at noontide directly perpendicular over their heads; and they are therefore sore vexed by the reflection of the beams," &c. "The place is also outrageous by the nature of the soil, by reason of which it is compassed about with muddy and stinking marshes, the infection whereof is not a little increased by the heat;" and he speaks of their being "vexed by the contagion of the

¹ Diseases and Mortality of British America, p. 32; J. Clark, pp. 52, 53; Chisholm, ii. 256.

² Imray, Edinb. Journ., liii. 78; Ib., lxiv. 319.

soil and heat of the sun, besides the corrupt water and infectious air, by reason of venomous vapours and exhalations arising from the same.”¹

The island of Barbadoes, the most southern of the Caribbean chain, being free from marshy surfaces, standing water, lagoons, and other sources of effluvia, and possessing a soil mostly calcareous, extremely light and absorbent, and all under cultivation,² is, and has always been, regarded as one of the healthiest spots of the West Indies. But while such must be admitted to be the case with respect to the island generally, the result at Bridgetown has been very different, for there the fever has often prevailed with great violence, from the middle of the seventeenth century to the present day. Richard Ligon, in his history of that colony, published in 1657, while giving an account of the disease which prevailed at Bridgetown ten years before, presents a portrait of the place, from which we may perceive that causes of infection existed there in great abundance. “Upon the most inward part of the bay stands the town, which is about the bigness of Hounslow, and is called the Bridge, for that a large bridge was made at first over a little neck of the sea, which was rather a bog than a sea; a town ill situated, for, if they had considered health as they did conveniency, they never would have set it there.” “But the main oversight was to build their town upon so unwholesome a place; for the ground being somewhat lower than the sea-banks are, the spring tides flow over, and there remain, making a great part of that flat a kind of bog or morass, which vents out so loathsome a savor as cannot but breed ill blood, and is no doubt the occasion of much sickness to those that live there” (p. 25). If to this we add the low, moist, filthy, and crowded condition of the town, and the injudicious mode of its construction,³ as also the condition of the harbour, it will be perceived that Bridgetown is no more a stranger—other circumstances contributing—to injurious exhalations than other tropical towns. As in other places, the fever has often broken out in the harbour and among the shipping. This was the case in the early epidemic mentioned by Ligon (p. 22), and subsequently. At other times it has principally affected the troops whose barracks are located on an eminence overlooking the bay and the town.⁴

In speaking of the epidemic of 1816–’17, of which he has given an interesting account, Mr. Ralph states some facts which, when taken in connection with the peculiar meteorological phenomena of that season, leave no room to doubt the formation of noxious exhalations in the localities occupied by the troops. In reference to these barracks, he says: “The soil surrounding the buildings is an extremely thin black mould, spread upon calcareous rock, the general physical character of this island. The parade-ground, a plain of about a mile in circumference, situated in their immediate front, had numerous excavations in its surface, and at the time was badly drained. A chain of ill-constructed huts, in the rear of the barracks, was appropriated to the married

¹ Robert Even’s translation of Peter Martyr, 6th chapt., 3d decade.

² Report, pp. 26, 27; Wilson, pp. 95, 136; Hillary, *Introd.*, p. 2, &c.

³ Williamson, *Med. and Misc. Observations*, &c., i. 27.

⁴ Tullock’s Report, p. 27.

soldiers." These huts were low, ill-ventilated buildings, constructed with a single deal board and roofed with shingle. Each was commonly occupied by two families, composed of six or more individuals. As already mentioned, the fever was first "notieed in the persons of the families residing in the huts. Where these creatures were living, crowded and oppressed by heat, the causes which favour the contamination of a stagnant atmosphere were most abundant; there, also, did the disease speedily rage with peeuliar malignaney, and many fell vietims to it. The first patients were all from the huts." When these families were removed to a dry, sandy soil, fever ceased.¹ It did not prevail in another barraek, on a rock a few feet below the level of the parade ground, and in other huts on a sandy beach, on the same level with the sea.²

In 1852, the first eases elearly originated, as we learn from Dr. Sinekler, in a native having a stagnant gutter at his door, from deeomposed animal matter from a slaughter-house, and a piggery in his yard. He died of blaek vomit. In a fortnight the disease broke out near a erowded ehurehyard in a low situation. Here, four died in one house. It was confined to this locality for some time, but ultimately spread widely.³

The town of St. George, in the island of Grenada, next elaims our attention from its having been rendered memorable by the disastrous epidemic of 1793, reecorded by Chisholm, and the eontroversy to which the origin of the disease gave rise. Situated on the leeward side of the island, at the foot of an amphitheatre of hills, encireling an extensive bay, it lies low, on the edge of the bay, has marshy ground in the neighbourhood, and especially an offensive tract at the east end of the eareenage, or harbour. The shore of this harbour, on the town side, is low, narrow, filthy, and erowded with houses and other buildings, separated only by lanes reaehing close to the wharves, most of them small, built of wood, and filled with low and profligate inhabitants.⁴ The vessels, of which there are usually a large number in the harbour, are moored elose to the wharves. What the eondition of this erowded, ill-ventilated, and badly built town and this harbour was in 1816-17, and 1828, when the fever prevailed, I have no means of aseertaining, but that in 1793 it was sueh as to favour the evolution of injurious miasms, we may eonelude from the testimony of Dr. Leblond, who was there at the time of the fever, and unhesitatingly affirms that "the malignaney of the disease was greatly aggrated by the existenee of sueh exhalations both in the harbour and in the town of St. George."⁵ The siekness broke out and prevailed in great measure in the erowded harbour among sailors and in the shipping, and thence extended to the neighbouring parts, where it remained limited, there being nothing to prove that the soldiers who suffered in the barraeks situated at some distance up the hill, or others out of town, did not contraet the disease

¹ Edinb. Med. and Chir. Trans., ii. 60; Ib., in Bancroft, Seq., p. 450.

² Ralph, in Bancroft, Seq., p. 452.

³ Report of Sanitary Commission N. O. 1853, pp. 203, 374.

⁴ Bancroft, p. 474; E. H. Smith, Med. Repos., i. 483.

⁵ Observations sur la Fièvre Jaune, p. 165.

by visiting the infected district.¹ But, however this may be, the reader will immediately perceive that localities such as have been described will readily become the seat of effluvia when placed under the influencing agency of a more than usual tropical heat, and that fever having broken out and prevailed in that district, a connection may be reasonably supposed to have existed between its appearance and the existence of the impure atmosphere thereby created.

Writers upon the yellow fever of the West Indies agree in regarding the towns of St. Pierre and Fort Royal, in the island of Martinique, as possessing all the requisites for becoming, under favourable circumstances, prolific sources of noxious miasms. They are situated on the edge of the sea, and incased, as it were, by high mountains, which more or less impede the free circulation of pure air, and in some parts—as in the bays—produce a calmness of the atmosphere which prevents the escape of noxious effluvia, and thereby prove injurious. Savarésy, who, more than any other, has investigated the subject, points out the various causes of those miasms, and has left a gloomy picture of the condition of the streets and various localities of those towns. From him we learn that the streets are often—especially near the harbour—crowded and filthy, and emit—as well as the market-places—in particular states of atmosphere, an odour highly offensive to those unaccustomed to it.

The surface of Fort Royal is flat, smooth, and low; in some places below water-mark. That of St. Pierre, though rather less objectionable in that respect, is flat and low in the vicinity of the harbour, and is, besides, surrounded by water. Around and in close proximity to the town, are marshes and wet ground, containing large quantities of mangroves, &c. The harbours are in a neglected and filthy condition, in some measure choked up with aquatic plants, and filled with carcasses of old ships and logs of wood in a state of decomposition—the whole, as well as the mud being exposed at low tide to the evaporating action of the sun, and giving rise to an abundant evolution of miasms. The small canals, dug for various purposes, are usually the receptacles of animal and vegetable matter in a state of decomposition, and contribute to the above effect, while the houses are low, ill constructed, and kept without regard to cleanliness (pp. 174, 192, 203, 210, 225).

Equally explicit as to the existence of mephitic effluvia in these localities, or some portions of them—the harbour, arsenal, barracks, &c.—are the accounts contained in other publications;² and, on reference, it will be found that the yellow fever has usually broken out in the harbours, or in the low, crowded, and filthy parts of those towns, and that it has always prevailed more extensively and fatally there than in other situations.

The reader can scarcely fail to know that St. Lucia is considered one of

¹ Bancroft, Appendix, p. 474.

² Thibault de Chanvalon, *Voy. à la Martinique*, p. 135; Clarke, p. 52; Dutroulau, in *Rufz*, p. 62; Gillespie, p. 22; Chisholm, ii. 78; Pinckard, ii. 78; Ferguson, viii. 119; Leblond, p. 134; *Rufz*, pp. 25–26; Catel, p. 19; Rochefort, *Hist. Morale des Iles Antilles*, p. 17, 4to., 1658; Robin, *Voy. dans l'Inter. de la Louisiana*, i. 46, 47; Lefort de la Saignée, &c., p. 591; Mourraille, *Topogr. de la Ville de St. Pierre*, p. 6.

the most unhealthy of the West India colonies.¹ This unenviable reputation, which is fully merited by many parts of the island, is particularly so by the town of the Castrie, which has often suffered from yellow fever in its most aggravated form. How far it may be regarded as the seat of injurious effluvia will appear from the following brief description. The town is situated on the leeward side of the island, at the bottom of a deep and narrow bay, on a plain scarcely elevated above the level of the sea, and in many places even lower. "It is surrounded by hills of considerable elevation, which are intersected here and there by ravines. Those parts of the town in the immediate vicinity of the sea have been formed artificially by filling up the shallow water of what was formerly a lagoon. The plain immediately to windward serves, during the dry season, as a parade ground for the militia; it is entirely uncultivated and swampy. From the north to the east the town is bounded by extensive fens filled with mangrove trees, and covered partially at high tides by the sea. To the southward it is bounded by the river, the lagoon, and the burying-ground, the lower part of which is often inundated, always humid, and covered by a luxuriant but rank vegetation. It is well paved for the most part, with a kennel running through the centre of each street; these, partly from the low situation of many parts of the town, and partly also from want of proper police regulations, are often exceedingly filthy."²

At Rio Janeiro, the disease broke out in 1850 in a particularly offensive spot, noted for its low, crowded, ill-ventilated houses situated on low, flat ground, and intersected by filthy, ill drained streets, frequently turned up to dry—the neglected receptacles of all kinds of impurities. The city, as remarked by Dr. Pennell, is situated on a bay, nearly land-locked, with little tide; the soil is sandy, and occasionally clayey; night soil is thrown every night into the bay, and becomes very offensive from the flux and reflux of the tide. "There has not been much clearing of land in the immediate vicinity, nor great disturbance of the soil; except in the streets of the city, which for some years past have been more frequently lying turned up than paved; constantly disturbed by some work that has been going on. When in this (lately their usual) state, they are most offensive, being a receptacle for all kinds of filth, and are left in the most abandoned state of neglect. In the day, the city smells badly enough, but at night it is almost intolerable. The drainage is all by open gutters, having a very small declivity; consequently, after two or three weeks dry weather, many of the streets are full of black offensively smelling mud, principally derived from emptying slops, &c. To improve matters, the junta hygienica (President, Dr. Paula Candido) have directed that the mud should be daily removed. In order to do so, that which is collected in the middle of the streets (and might lie there comparatively innocently) is first spread over the whole surface of the street, to dry in the scorching rays of the sun, that it may exhale all its pestiferous influences, to the discomfort of the inhabitants; and is then (when dry) carted away."

¹ Report, p. 31; Evans, p. 7; Rollo, p. 3; Chisholm, ii. 126; Poissonnier, *Maladies des Gens de Mer*, i. 391; Pugnet, pp. 261, 343; Levacher, pp. 5, &c.

² Evans, *Endemic Fevers of West Indies*, pp. 6, 7.

The original cases were clearly of spontaneous and local origin. The first occurred in one of the described lodging-houses in the street de la Misericordia—a Danish sailor, direct from Finland. The second, belonged to a vessel from Bahia; and the next ten were all from this filthy neighbourhood. The locality which next became infected was the Saude, at exactly the opposite end of the city, about a mile and a half from the other. “The Saude, for the most part, is only a little higher than the level of high water mark, and is but partially paved. The soil is clayey and intersected with open gutters. The surface being low, flat, and very uneven, affords a ready lodgment to small pools of water, which stagnate; and as they contain a certain amount of black masses of decaying animal and vegetable matter, they at all times emit a most offensive odour. It is bounded on more than two sides by water, which daily, at low tide, leave exposed an immense surface of a dark mud that produces a most loathsome smell.”¹ “The yellow fever was unusually severe in this locality.”

In a geological point of view, the island of Guadaloupe is divided into two distinct parts by an arm of the sea called Salt River. The western portion, or Guadaloupe proper—has, in its centre, from north to south—high volcanic mountains tipped with forests, from which rivers and torrents flow. The eastern part, or Grande Terre, is low, flat, fertile, and reposes on a calcareous basis.² Pointe à Pitre belongs to Grande Terre. It is situated at the bottom of a bay of the same name. It is S. and S. E. of the above-mentioned river, and is washed, by the sea on the N. N. E., and N. W. by S., and is belted by mangrove marshes. The soil is argillaceous, and the position low and flat. Before the destruction of the city in 1843—the houses, in the old town, were built of stones and located on piles, or made ground. The new town is situated to windward of the other, and is inhabited by coloured people. The surface is low, flat, and clayey, and is converted often into a marsh by the overflow of the sea and the rain. This part escaped the effects of the fire. The houses are of wood, low, damp, filthy, and ill ventilated; the streets are muddy and badly kept; the cellars are constantly filled with water. The S. E. and N. N. E. winds, before reaching the city, pass over several marshes—those of La Source, and Forts Union and Fleur d’Epée. The N. E., N. and N. W. currents pass over the marshes of Petites Abimes, or carry along with them the effluvia issuing from the banks of Salt River.³

The details into which I have entered, relative to the nature and condition of those localities in tropical climates where the yellow fever usually prevails, might be extended.⁴ But enough has been said to show, that such localities usually possess the materials requisite for the evolution of miasms,

¹ A short Report upon Yellow Fever, as it appeared in Brazil during the Summers of 1849–50, pp. 7, 8. See also Report of San. Com. of N. O., 1853, pp. 154, 374.

² Dupuy, Archives Gen., xxii. 415.

³ Arnoux, An. Marit., 1844, ii. 749, 750.

⁴ St. Thomas, Chisholm, ii. 318; Santa Cruz, ib., pp. 332, 342; St. Vincent, Chisholm, ii. 143.

injurious to health, and that fever breaks out and spreads more extensively, and with greater severity in those places where circumstances tend to increase the production of those miasms. That such is not always the case—that the yellow fever is unknown or prevails but seldom and mildly in situations proverbial for the evolution of insalubrious miasms; and again, that it breaks out and spreads extensively in localities where the usual sources of an impure atmosphere are wanting, are facts I am far from wishing to deny. Until very recently it did not prevail—except sporadically—and even in that form not frequently, on the coast of Brazil. It shows itself seldom in Guiana or in Honduras, and in many other places; though there the palpable causes of mephitic miasms abound, while it has appeared in localities, on Stony Hill¹ (Jamaica), Rockport, at Fort Haldane,² on Brimstone Hill,³ in the island of St. Christopher, and other elevated situations, where no such causes apparently exist; and where, judging from all that can be seen, and from a comparison with the condition of other places visited by the fever, we should not expect to find it prevailing. But, on examination, it will be found that, in those places where materials productive of morbid effluvia exist, without being associated with the manifestation of yellow fever, these effluvia present the characters of simple marsh miasmata, which, as I shall have occasion to point out in a subsequent chapter, are not the usual source of the disease in question; while the other localities visited by it, though destitute of swampy or humid surfaces, and of some of the other sources of exhalation pointed out, are nevertheless characterized by peculiarities of soil and other conditions—natural and artificial—of surface, which are no less prolific than the former of injurious exhalations. Lind, long ago, pointed out the insalubrity of particular sandy soils, “such as that of Pensacola, Whydah, and the island of Boa Vista;”⁴ and Dr. Ferguson has shown that, in Europe and the West Indies, many dry soils, naturally or made so by want of rain,⁵ were the seat of exhalations of the most noxious kind.

By Dr. John Wilson, too, many facts have been adduced to prove that such exhalations are more rife, not in common surfaces, but in places characterized by a calcareous soil and the existence of ligneous substances in a state of decomposition. If such be the case, we must admit that the presence of the fever on Brimstone and Stony Hills, cannot at all militate against the truth of the connection I have shown to obtain between the appearance of the fever and the existence of causes of effluvia; for though the sources of these may not be precisely the same there as elsewhere, their presence is equally evident, and they may give rise under the action of a tropical sun, and in particular conditions of atmosphere, to the same result. Of Stony Hill, which, as its name implies, is stony in external structure, and in the vicinity of which there is neither marsh nor marsh-like land, it is remarked that from

¹ Bancroft, Seq., p. 435; Reports, p. 59; Wilson, p. 129.

² Report, p. 38; Wilson, p. 97; Jackson, Sketch, i. 16, 18.

³ Wilson, p. 129.

⁴ Hot Cl., p. 139.

⁵ Ferguson, Recollections, pp. 185, 198.

its visible base in the plain of Liguana to its top—a distance of about a mile—it is one great mass of calcareous rock intersected by deep fissures, and split, in many places, into large fragments, pushed often from their original position, heaped on each other, and crumbling into powder. Over its whole surface, there is scarcely any soil, and little grass, weed, or herbage of any kind. But it is generally covered with forest trees, the roots of which are seen expanded over, and clinging to the native rock, the extreme fibres dipping into the crevices and hiding themselves amid the detritus of rock, decaying leaves, and other ligneous matter which are lodged there.¹

The combination of calcareous soil and decaying roots, and other kindred substances, is found to prevail in most of the localities visited by the fever in which the more common causes of exhalation do not exist—and in many where they do exist—and are well ascertained to give rise, under atmospheric influences of a particular character, to morbid effluvia. When we bear all this in mind, and, in addition, learn that the fever which there occurred broke out in seasons peculiarly fitted to promote the evolution of those effluvia,² and prevailed mostly in barracks and other buildings which, under the most favourable circumstances, are usually in a condition to aid in impairing the purity of the air, and that other varieties of fever exist in the same or similar localities which are acknowledged to be due to some forms of miasma, we shall not feel disposed to regard cases of the kind cited as conflicting with those that precede; but, on the contrary, derive therefrom further proof of the truth of the opinion that the appearance and prevalence of the yellow fever is invariably connected with the existence of sources of febrile infection.

What the nature of that connection is—whether it is the effluvia arising from those localities and the manifestation of yellow fever, that are to be viewed in the light of cause and effect; or whether the local peculiarities to which attention has been called, can only be charged with predisposing the system through means of the effluvia emitted, the morbid influence of some other efficient cause, or, by mixing with the latter, increasing its energy; or whether, again, they only tend to furnish a matrix in which a poison introduced from elsewhere, multiplies itself and spreads far and wide, are points which must be reserved for future consideration. All that need be said now is, that as in many of the instances mentioned, the disease has not only assumed a more malignant and fatal garb in the localities described, but has actually originated there, and broken out under circumstances preventing its being ascribed to the introduction of a contagious poison, we must admit that the cause which gave rise to it was elaborated there. At any rate, the connection in question approximates the yellow fever to the whole class of autumnal or malarial fevers, which require for their production peculiarities of locality not very different from those associated with the manifestation of the other, and the morbid agency of which depends on the evolution from their surfaces of peculiar effluvia. Now as those fevers are admitted to be of local origin, and are devoid of contagious properties, the inference is natural that the yellow fever is, like them, dependent for its origin and diffusion on local

¹ Wilson, pp. 99, 129.

² Bancroft, Seq., p. 437.

influences, and also devoid of the properties alluded to. This being granted, we may with strict propriety conclude, that what holds good with reference to the yellow fever of tropical regions, will do the same so far as regards the yellow fever of temperate climates; that the same kind of localities which prove the place of origin of the disease in the former, will under like conditions of atmosphere, be the seat of origin of the fever in the latter; and that if the fever is thereby shown to be non-contagious in the one region, it cannot be endowed with different qualities in the other. We might rest the decision of the question upon the existence of this connection. But other facts which have a strong bearing on the points at issue, and which, while further illustrating the reality of that connection, are calculated to enable us to form a decided opinion as to its nature, remain to be stated.

CHAPTER XXI.

PROOFS OF NON-CONTAGION—CONTINUED.

Injurious Effects of upturning the Earth.—Every physician knows that the digging of canals, the opening of ditches, the cutting down of bluffs, the levelling of lots, the filling up or digging down of streets, the opening of roads, the establishment of brickyards, &c., have proved highly injurious, not only in this country, but in parallel or hotter latitudes, as also during the hot season of cold regions. The results of operations of the kind in the East and West Indies are on record, and, like those in Algeria—an account of which has recently appeared—are of a nature not to be easily forgotten.¹ Many an epidemic of bilious intermittent and remittent fever, may be traced to occurrences of the kind, as the writings of Lancisi,² Drake,³ Evans,⁴ Blane,⁵ Cassan,⁶ Maccullock,⁷ Usher Parsons,⁸ Auderbach,⁹ Davy,¹⁰ Gaultier de Claubry,¹¹ Merrill,¹² Grant,¹³ Stevens,¹⁴ Villermé,¹⁵ Burchardt,¹⁶ Barton,¹⁷ and Caldwell,¹⁸ will fully attest. To the same cause, under peculiar conditions

¹ Jacquot, *Fièvres à Quinquina*, pp. 13, 29.

² De Noc Palud. Effl., lib. 2, ii. 210–11.

³ Op. citat., pp. 182, 229, 235, 239, 372. ⁴ Op. cit., pp. 75, 266.

⁵ Dissertations, i. 332.

⁶ Mém. de la Soc. Méd. d'Emulation, v. 142.

⁷ On Malaria, pp. 83, &c.

⁸ On Malaria, in vol. of Essays, p. 206.

⁹ N. Y. J. (N. S.), ii. 75.

¹⁰ Second Report on Quarantine (Lond.), p. 57.

¹¹ Mém. de l'Acad. de Méd., xiv. 120.

¹² Memphis Med. Recorder, i. 85, 94.

¹³ Am. Med. Journ., July, 1853, pp. 74, 115; Ib., N. O. J., May, 1852.

¹⁴ On the Blood, p. 239.

¹⁵ An. d'Hygiène, xi. 352.

¹⁶ Gaz. Med. de Strasbourg, xii. 407.

¹⁷ Report to State Med. Soc., p. 34; also, Report of 1853, p. 324, &c.

¹⁸ Thoughts on Febrile Miasms, Boston, Med. and Surg. Journ., iii. 504.

of soil, and during the prevalence of certain thermometrical and hygrometrical states of atmosphere, the outbreak of yellow fever, at various periods, has been, with apparent justice, ascribed. Dr. Merrill, in an essay on the yellow fever of Natchez, has taken great pains to show, and with every appearance of success, that the epidemic of 1823 arose from the levelling of the streets, and the consequent exposure of the fresh soil to the action of the hot sun.¹ This city, Dr. Merrill tells us, in a subsequent publication, is built upon a bluff; in 1816, the city authorities began to put into operation a plan for reducing the irregular superficies to what was considered a more suitable grade. During all that year a large amount of work was done, digging down and filling up streets and lots, without due regard to the maintenance of a proper drainage. The succeeding autumn the first epidemic yellow fever occurred, and produced a frightful amount of mortality. As soon as the shock of the epidemic had partially subsided, the work of grading was resumed. No one could perceive why it should be considered the cause of the disease, and the work went on, with a recurrence of the epidemic visitation every second year, until the population having been several times decimated by death, and business and property having declined, the city found itself scarcely able to continue the improvements, and scarcely worth them if made.

"After the dreadful visitation in 1823, the work gradually fell off, and the subsequent return of the disease diminished in violence and fatality *pari passu*, until the last of the series in 1829. Soon afterwards a new era commenced. Exuberant prosperity overspread the land. A new population was brought into the city, and, in 1834, '5, '6, the grading was resumed to some extent. The doctrine of domestic origin and artificial causes had again lost ground. The voice of experience was not heard, or overruled, and the penalty suffered. Many of the older inhabitants foresaw the result in fear and trembling, and the epidemics of 1837 and '9 sent many to their untimely grave. The eyes of the living were again opened to see their danger and its causes, and since that time little grading has been done. To this Dr. M. adds: "Now, that these experiments did render Natchez sickly, there can be no reason to doubt. The coincidences were too striking to be viewed as accidental. Besides, the same effects following the same causes have been observed elsewhere."² Dr. E. H. Barton, of New Orleans, to whose report on the sanitary condition of that city in 1853 reference has been made on several occasions, and who has borrowed the above facts, states, on good authority, that again, notwithstanding previous warnings, in 1853, at Natchez, the levelling of the streets by the cutting down the adjoining banks, and superposing the fresh earth on the streets, resulted in the fever. "It is said to have first broken out in the immediate neighbourhood where this took place—that here occurred its largest mortality, and thence it spread to the neighbourhood" (p. 317).

The example of Charleston may be cited. The following statement is borrowed from Professor Dickson. Situated on a neck of land, between two

¹ Phil. Med. and Phys. Journ., ix. 340.

² Memphis Med. Recorder, i. 87, 88.

marshy rivers, the city is thickly built up along the western shore of the eastern one of these—Cooper River—for more than a mile from their junction. Along the east bank of the other—the Ashley—the houses are less crowded, and the soil is more sandy and somewhat higher. The former is the business part of the city, the latter contains chiefly private residences. A considerable portion of the ground in different parts of the peninsula is either entirely *made*, having been anciently covered by creeks, which intersected it in every direction, or has been filled up much above its old level. The materials used for thus filling up low, swampy lots, have been various. In place of stones, which are not to be procured but at great expense, pine logs, oyster shells, rubbish of all sorts, and even scavengers' offals, constitute the general mass of the soil of those comparatively modern spots. The wharves, too, are built chiefly of wood—palmetto and other logs forming the framework or outline, which is filled up in the way above indicated, and with mud drawn by machinery from the shallow docks which they embank. Dr. Dickson adds that it will be readily inferred that grounds thus *made* will be eminently fitted for generating and giving out deleterious effluvia. Accordingly, it is not difficult to denote the sections of the peninsula which deserve to be considered healthy places of abode, and mark out such as bear a different character; and this knowledge influences the choice of residences and the value of rents.¹

The injurious effects of these works are further illustrated by occurrences in the same city, in 1842 and 1852. "In 1842, white labourers, strongly predisposed to yellow fever, were employed in opening drains and other works, and transferring the earth to different portions of the city; and where the drains were opened and the earth was deposited, there yellow fever occurred, and the unfortunate beings who performed that work were the greatest victims. In 1852, the same thing occurred."² Dr. Simons remarks, in a note, that, in reference to the last-mentioned year, at the new custom-house, a great number of Irishmen were employed in excavating the earth, and piling; that of these a great many were taken sick and died; and that the sale and distribution of the earth through different portions of the city had a baneful effect. The same correct observer states, in addition, that "in 1849 an extensive drain was opened in Hasel Street, excavating the most filthy and offensive materials; and, likewise, an extensive drain in Market Street, from Church Street to the wharf. Yellow fever occurred earliest in those localities, and was more fatal. Again, the earth, so filthy and offensive, was transferred to King's Street, from Horlbeck's Alley to Hasel Street, and in this particular spot there was sickness among a class of persons who are generally exempt."³

For the following communication I am indebted to a highly distinguished physician of Charleston, already often cited in the present work—Dr. Wragg. After expressing doubts as to the introduction of the disease from abroad in 1854, the writer says: "My reasons for this opinion are that there were cases

¹ American Journ. of Med. Sci., ii. 67.

² Simons, Charleston Med. Journ., viii. 364.

³ Ibid., p. 363.

of yellow fever brought into Charleston last year, and that an epidemic of that disease did *not* result. Again, that there was abundant reason for the local origin of fever in the condition of the city. The local causes to which I allude are the recently filled up low grounds about the city, for the reclaiming of which the common refuse of the streets and yards was used. It is true that this system of filling up has been going on for many years, but it is also true that there is some check put to the process after each yellow fever season. The city authorities seem to take the alarm, and, for a time, the deleterious agents are diminished. But the lesson is soon forgotten, and the warning needs to be often repeated. That these local causes are chiefly conducive to the origin, existence, and spread of yellow fever, and that they have been so this year is, in my opinion, proved beyond any reasonable doubt by the fact that all the early cases of the fever occurred in localities where these accumulations of filth existed. And these localities were not few, nor near together. They were numerous, and many of them far removed from each other. The fever this year invaded portions of the city in which it never was known to have gone before, and, for a time, all of the infected spots were those which were poisoned by the decomposing matters alluded to. It is true that, after a time, other locations suffered also; but this was later, when the whole atmosphere was poisoned. Let me give *one* instance. The jail and Marine Hospital stand adjoining each other. For forty-five years, yellow fever patients have been treated in the hospital, and no case has ever been known to exist in the jail until this year. This year there have been eighteen cases in the jail. Further, the quarter of the city in which these institutions are has always been exempt from yellow fever till this season. It has suffered severely this year. What is the reason? Contagionists are citing these facts on their side; and it must be admitted that the bare announcement that yellow fever existed first in the hospital, and then broke out in the jail, adjoining, looks plausible. But when we add that these buildings have held their relative positions for forty-five years, and that fever has never before spread from one to the other, it becomes necessary to look for some other reason. In my opinion, this is to be found in the fact that, in the last three or four years, large tracts of low land in the immediate vicinity of these buildings have been partially filled with this street-sweeping refuse, so that they are now in the most filthy and offensive condition which it is possible to conceive of."

Guided by an extensive experience in such matters, Dr. Barton establishes as a proposition that from 1796-'7 to the present time there has been no great epidemic yellow fever in New Orleans without an extensive breaking, disturbance, and exposure of the original soil of the country; that this has consisted in digging canals and basins, or cleaning them out, either in the city or in its immediate neighbourhood; digging or excavating the streets of the city, for the purpose of laying down gas and water-pipes, and relaying the streets, &c. And the extent and malignancy of the disease has been pretty much in proportion to the extent of these exposures. Dr. Thomas, in his work on the yellow fever of New Orleans, long ago referred the epidemic of 1796 to the

digging of the canal of Carondelet.¹ Dr. Barton, in like manner, refers to the coincidence of the fever with the opening of that canal, and more especially of its basin, in 1797. "I am informed," he tells us, "by a highly intelligent and observing creole gentleman, that the fevers during the period of digging this canal were awful in its neighbourhood, even with creoles, and that last year the sickness in the vicinity of the excavation of its new basin was very extensive, although there were few but natives and acclimatized exposed to it."

The same canal was cleaned out in 1811, and once more the fever prevailed extensively. Next we have the severe epidemics of 1817, 1819, and 1822, simultaneous with extensive exposures in the streets, for pavements, large fillings up and inclosures of the batture, and the cleaning out and deepening of the same canal. Then follows the epidemic of 1832-'3, resulting from the immense exposure of the soil in digging the Bank Canal from the city to the lake. Again, we have the epidemic of 1837 coinciding with the digging of the extensive trenches and canals to drain the rear of the First and Second Districts. The next large mortality "arose from the large new canals, and clearing and exposure of the soil between the two canals, in the rear of those districts, without regard to season, and the immense excavation of two acres of ground, and with the removal of upwards of 336,000 cubic feet of earth for the foundation of the new custom-house, in the heart of the city—beginning the latter part of October, 1848, and ending in the succeeding August." "And, finally, what has contributed so much to produce the great calamity of last year (1853) was the extensive exposure of the earth in making a new basin for the same canal (Carondelet); cleaning out the canal; dredging the Bank Canal; extensive exposures of the earth in deepening the ditches between Conti and Common Streets, and also in the rear of the Third District, and exposure for the erection of a levee between the two canals on Lake Pontchartrain; the large excavations on miles of streets, in the centre and front of the city, for laying down gas and water-pipes, and making and relaying pavements; the extensive exposures for laying the foundations of new buildings; and the excavations and exposures, for railroad purposes, in the rear of the First and Fourth Districts, and at Algiers." "Here, then, we have a combination of materials of exposure of the original soil unprecedented in our annals, and the consequences have been correspondingly destructive, in combination with meteorological conditions (before expressed); in proof of which, this mortality continued large as long as this exposure continued" (pp. 312-314).

To the credit of Dr. Barton, it may be stated that, so early as the 6th of June of that momentous year, he predicted the forthcoming fearful mortality. At a meeting of the New Orleans Academy of Sciences, held that day, he exhibited a chart of the mortality of that city since 1787. Among many other interesting facts developed by the chart, he called attention to some recorded above; and "to the inquiry as to the probability

¹ *Essai sur la Fièvre Jaune d'Amérique*, p. 70.

of an epidemic during the coming season," he replied that, "judging from the past, if the facts exhibited by the chart were not merely coincidences, he was compelled to apprehend that the present year would be marked by a great augmentation of disease. The simultaneous construction of four railroads in and around the city; the digging of a new basin of vast extent in the rear of the city; the enlargement of the canal Carondelet; the open sewers, scarcity of water, insufficient drainage, and the practice of spreading over the streets the horrible filth of the gutters, to fester and reek in the sun—if all these are continued during the hot months, with the proper meteorological condition, our exemption from a severe epidemic should almost seem miraculous."¹ Here, let it be remembered, is an epidemic predicted on data of a positive kind, by an experienced and observant physician; and when that epidemic arrives, some are found disposed to attribute it to importation from abroad, and quarantine measures are suggested, to guard against the further introduction of the disease!

The Report of Dr. Barton, and the documents by which it is accompanied, contain accounts of several other interesting instances of occurrences similar to those already mentioned. Such was the case at Lake Providence, at Fort Adams, Centreville, Clinton, Trenton, Lafourche, Natchitoches, Algiers, Selma, Montgomery, Hollywood, and Gainesville. In all these, the outbreak of the fever was preceded by extensive disturbances of the soil, and its exposure to the action of the sun. "The severe epidemic yellow fevers at St. Francisville, in 1827 and 1829," says Dr. Barton, "were to be clearly debited to the cutting down the hills, spreading the materials on the streets and grading them, digging cellars, &c., during the summers of those years; and the epidemic of 1839, at Bayou Sara, was equally due to the filling up, by spreading fresh earth over brushwood, and filling up low places. And for the only epidemic known to have occurred there since its settlement, that of 1853, may, with much propriety, be ascribed to parts of a levee made and ditches dug—acres of saw-dust from a saw-mill spread, and low places filled with it, and for years previous, working extensively on the streets just previous to the epidemic and extensive swamps levelled off and dried up in the vicinity" (p. 317). Dr. Levert, of Mobile, in a document addressed to Dr. Barton, and published in the Report of the Sanitary Commission of New Orleans, observes: "One point," I think, "is satisfactorily established in connection with its appearance (the yellow fever) in Mobile upon almost every occasion, viz: that it has invariably occurred in those seasons when large excavations have been made, or extensive surfaces of fresh earth exposed to the action of the sun and air during the heat of summer; while, on the contrary, our city has been almost as invariably exempt from this scourge in those seasons in which no such causes existed." After citing a number of facts in corroboration of this statement, Dr. L. observes, "that, in the year 1853, more grading has been done, and more extensive excavations have been made in the city, and consequently much larger surfaces of fresh earth have

¹ Proceedings of the New Orleans Academy of Science, i. 11.

been exposed than in any one year since 1825, and the epidemic of that year has been more general than any former period" (p. 111.)

This position has been farther confirmed by occurrences in other cities during the present year. In the introduction to the report just referred to (p. xvii.), we find the following statement: "In the city of Savannah, the epidemic of this year is with great probability attributable to the exhumation of a large number of vessels sunk just below the city during the revolutionary war and that of 1812, to the filthy land and other debris derived from the city and the tide, which was thrown upon the bank near the town and even spread upon the streets, over which the wind constantly blew, and to the excavation of the soil of the streets (at least a mile) for the purpose of laying down water pipes.

"The epidemic at Augusta was as fairly owing to the cleaning out of filthy canals in the city, and exposure of the offensive mud to the hot summer's sun, to the emptying the city filth on the bank of the river, which was unusually low, and to the disturbance of the soil of the city for the purpose of laying down gas pipes."

Influence of Made Ground in the Production of Yellow Fever.—As connected with this subject, it may be mentioned that the occurrence of yellow fever in localities the greater part of which is composed of made ground, and the apparent connection, as effect and cause, existing between such occurrences and the peculiar nature of those localities, have long been subjects of observation. Dr. Richard Bayley noticed the connection in relation to the epidemic of New York in 1795. It has been the policy of the corporation of the city, he says, at different times, to make *new ground*, as it is called, or to sell to others the privilege of doing so. The new made ground from Whitehall to Catharine Street, along the East River, was, at that time, on an average four hundred feet in breadth, creating fifty acres of ground, all of which was built upon. The ground made on the west side of the city was about ten acres. In this part and the vicinity, the fever principally prevailed, and there it was most malignant (pp. 59–61). Other instances of the kind have already been referred to.

The result of observations furnished by the occurrences in British Guiana are apposite. I have already alluded to the mud flats and sand banks which form the foreshores of the colony. They shelve out into the sea in front of the poldered land, and are composed, as Dr. Blair informs us, of banks or flats, of fine silicious sand (caddy) or shell-sand, or drift mud, in alternate ridges, or superimposed in the order stated. The sea sometimes encroaches on the land, and these foreshores are washed away, and plantation embankments injured or destroyed, and the safety of the town endangered. Sometimes the sea again recedes, new land rises in the sea front, demanding laborious and expensive operations to keep the draining trenches and sluices patent. These oscillations are, as it would appear, periodical and alternating, and may probably be found to be connected with meteorological cycles, in which the force and direction of the wind will be found material elements. Each of these periods of encroachment must have been pre-

ceded by a corresponding epoch of alluvial deposition. "The periods of acme of accumulation seem singularly coincident with the prevalence of epidemic yellow fever, and the wash or encroachment of the sea with the disappearance of the disease." The plan of the military grounds published by Dr. Blair, shows the condition of the foreshores in 1836 and 1847. "The land, which" as he says, "has been making all along the shore steadily till after 1837, has been, for the last three or four years, melting away, and now the military works are threatened with destruction; the river bank on both sides is washed. Old barriers are required to be renewed; churches must be removed; and along the east coast a retreat is in many places required for the whole plantation embankment. Simultaneously with these scavenger operations of the sea, the yellow fever epidemic has disappeared. In connecting these phenomena with each other, Dr. Blair properly thinks, it can be done now in a general way only. They are simply associated phenomena—whether by accident, or in the relation of cause and effect, or as joint effect of a common cause, very many future observations may be required to decide. The present coincidence, however, is of importance, as a finger-post to the whereabouts the pathogenic causes are to be sought.¹

This connection of accumulations of the kind with the appearance and prevalence of yellow fever in tropical climates, constitutes another point of approximation between that disease and other fevers dependent on local causes, and devoid of contagious properties.

The injurious effects resulting from the receding of the sea, and the consequent extension of the land by the deposition of alluvion, have been noticed at the mouth of the Tiber, of the Po, of the Ganges, of the Orinoco, of the Mississippi, &c. The partial revolution which has taken place in the first of these localities, as is remarked by Dr. Carrière, has not resulted merely in pushing back inland the old city of Ostia, formerly the seaport of Rome, and separating from the sea the original mouth of the Tiber; it has placed the city in the centre of an insalubrious territory, of which it formerly occupied only the edge, and has been the cause of its progressive depopulation. We know that formerly Ostia was too small for the number of its inhabitants, and figured among the magnificent cities of Latium. At present, it contains only a priest, a tavern-keeper, four or five soldiers, and a few families, who do not venture even to reside there all the year round. But this insalubrity is not limited to the city; for, inasmuch as the miasmatic elaboration is effected on a large scale, in consequence of the increased extent of surface, there has arisen, since the time of the Romans, a new source of morbid effluvia, which must be taken into account when we inquire into the sanitary conditions of the country, though more especially when the wind blows from the sea.²

Another case in point is mentioned by Sir W. Burnett. In former days, there existed a large marsh near Mahon. It has now been converted

¹ *Op. cit.*, pp. 114–116.

² *Amer. Journ.*, July, 1851; Carrière, *le Climat de l'Italie*, pp. 20, 21; La Roche, on *Pneumonia and Malaria*, p. 243.

into productive gardens. Since the draining of the greater part of this marsh has been completed, the sea has been gradually receding from the head of the harbour, leaving (particularly during the prevalence of the easterly winds) a large portion uncovered, from which, in the summer and autumn, the most offensive exhalations proceed.¹ I have already alluded to the effect of the annual receding of the Nile. The same effect results from the diminished extent, noticed every year at particular periods, in certain other streams and bodies of water, by which a large portion of their banks is laid bare. The river Biviere, and other watercourses in Sicily, lose, yearly, two-thirds or more of their dimensions; Lake Cagliari, in Sardinia, loses almost as much. All are hotbeds of fever.

Connection of Inundations with the Occurrence of the Yellow Fever.—Nor is it less certain that the appearance of yellow fever in an epidemic or even sporadic aspect has coincided with the occurrence of inundations, general or partial. That such has been the case in relation to the ordinary forms of autumnal or malarial fevers, I shall not stop to demonstrate here, inasmuch as I have gone over the ground in considerable detail in another publication,² and feel convinced that facts corroborative of the statement are familiarly known to the reader. What is true of those fevers is true also, though perhaps less frequently, of the one under present consideration. The yellow fever epidemic of La Guayra, in 1797—the first known to have occurred in that place—has been referred, with much plausibility, to the overflowing of the river of that name. “This torrent, which in general is not ten inches deep, was swelled, after sixty hours of rain in the mountains, in so extraordinary a manner, that it bore down trunks of trees and masses of rocks of a considerable size. During this augmentation, the waters were from thirty to forty feet in breadth, and from eight to ten feet in depth. Many houses were carried away by the torrent, and the inundation became more dangerous for the stores, in consequence of the gate of the town, which could alone have given an issue to the waters, being accidentally shut. It was necessary to make a breach in the wall in the sea-side; more than thirty persons perished.”³

Dr. Barton calls attention to the circumstance that, for several years past, most extensive inundations have prevailed over large portions of the State of Louisiana—over the cultivated as well as the uncultivated portions of it—that, as these became partially desiccated, they reached the condition of all dried swamps, which are known to be highly injurious to health everywhere, and that with the concurrence of certain meteorological conditions, they formed that combination of circumstances necessary for the existence and spread of a great epidemic. Making the application of these well-known facts to the yellow fever, Dr. Barton considers that these inundations are evidently connected as one of the prominent causes of the great epidemic of 1853, and in support of this position, states that those parts of

¹ Fevers of the Mediterranean, p. 14.

² On Pneumonia, &c.

³ Humboldt, Pers. Nar., iii. 292.

Louisiana which have suffered most from the epidemic have been the greatest sufferers, so far as can be learned by the inundations. After a reference to the results consequent on the subsidence, after overflow of the Nile and other rivers, Dr. Barton remarks (pp. 337-8), that the same effect occurs on the subsidence of the Mississippi, and that its early or late subsidence materially influences the result; the period of decline being the period of fever. That the inundation of the banks do not usually produce their disastrous effects until the second year, is not difficult of explanation. The ordinary condition of our swamps (not marshes half dried) is not injurious to health, as is well known throughout the State. When this is vastly increased by a crevasse, large additions are made to the swamp water. The cultivated country is inundated, and, by the natural subsidence of the water, is converted into a marsh, and has to undergo the successive poisonous stages of desiccation, with the evolution of results through solar influence, which takes a season or two fully to develop. That the first year of inundation is not injurious, clearly results from the immediate removal of filth; that the second year the effects mentioned follow as results, is proved by the following authentic data of its influence in New Orleans aided and aggravated by causes of a meteorological character.

Extensive crevasse, inundating a large part of the city and neighbourhood, in	1816.
An extensive epidemic yellow fever in	1817.
The hurricane inundating the city to Bourbon Street, in . . .	1821.
Epidemic yellow fever in	1822.
The rear of the city inundated by a storm to Dauphin Street, in	1831.
Great epidemic of cholera and yellow fever in	1832.
A severe blow drove the waters of the lake to Dauphin Street, in	1846.
An extensive epidemic occurred in	1847.
Extensive inundation of the city to Carondelet Street, in . .	1849.
About 3,000 cases of yellow fever, and an increase of more than one per cent. in the general mortality of the city above the average in	1850.
There was a crevasse opposite the city in	1852.
Large mortality of the epidemic in Algiers, which was attributed to other causes, in	1853.

Whatever be the explanation of these facts, the connection to which I have alluded cannot for a moment be doubted; and it must be admitted by all who examine the subject, that these inundations greatly aided in the production of the epidemics in question. It need scarcely be remarked, however, that such occurrences are not essential to the manifestation of the disease in New Orleans, for the epidemics of 1819, '29, '33, '37, '41, &c., were not preceded by occurrences of the kind; and we know that the exemption has been noticed elsewhere.

Connection of the Yellow Fever with the Occurrence of Large Fires.—It is a fact well worthy of consideration in reference to the matter more particularly before us at this moment, that epidemics of yellow fever of the most

devastating kind, have occasionally occurred in connection with, and apparently as a consequence of, extensive fires. This certainly has been the case so far as concerns the city of Charleston, and may be explained—and has never been explained otherwise—by the fact of a large surface of decomposing materials being thereby exposed to the action of the sun. Dr. Joseph Johnson, of that city, has dwelt on the subject, remarking that after each extensive fire years of yellow fever have succeeded, desolating in proportion to the surface laid bare by the ravages of those fires respectively. “The first instance of this kind was after the devastating fire which broke out in the evening of the 13th of June, 1796, extending from Lodge Alley, near East Bay, to the corner of Broad and Meeting Streets, on which the City Hall now stands. The summers of 1796 and 1797 were terribly marked by the prevalence of yellow fever in Charleston; so was the summer of 1811, subsequent to that fire which commenced on the evening of 9th October, 1810, back of St. Philip’s Church, extending on both sides of what is now State Street, to Broad Street and East Bay. So also with other years, particularly the last and most destructive of fires in April, 1838, by which twenty-five acres of land in the centre of the city were laid open to the lodgment of water and exhalation of deleterious vapours from animal and vegetable substances in a state of putrefaction.”¹ “By this,” says another of Charleston’s choicest sons, “vaults and cellars containing offensive and decomposing matters were exposed to the rays of the sun; and yellow fever, the most severe which we have had during the present century, made its appearance.”²

Professor Dickson seems disinclined to attribute any share in the agency to the exposure of the surface thus exposed, and merely admits that: *a.* Something may be safely attributed to the quenching of so many culinary fires; devastation, solitude, and disease being often connected. *b.* That the want of shade must have injured the workmen exposed over that irregular, unsheltered expanse, when thermometers placed among them would scarcely have fallen lower than 140°. *c.* That high wages must have acted as a stimulus to over-exertion, more injurious in an untried climate—to protracted labour and unaccustomed fatigue.³ All this may be true so far as it goes, and might be sufficient had the disease affected only those who suffered from the quenching of *culinary fires*, and the unacclimatized workmen who were exposed to the scorching heat, and overworked themselves in the untried climate; but it extended far and wide, and committed unusual havoc among individuals who could not be placed in those categories. Besides the injurious effects of ruins and rubbish, especially when fresh, and when exposed to the action of a hot sun, are well known; and fires have been the precursors of yellow fever in too many places where the contingencies mentioned could not be influential, to be viewed as acting solely in the way suggested by the distinguished writer referred to.

¹ Charleston Med. and Surg. Journ., iv. 157.

² Gaillard, Rept. on Public Hygiene, Trans. of American Med. Assoc., ii. 579 (1849).

³ Bell’s Eclectic Journ. of Med., iv. 112, 113.

The epidemic of Savannah, in 1820, was preceded in January by a conflagration which laid bare a large portion of the city. This, as Dr. Waring admits, "exposed to the operation of the sun a great number of vaults and cellars, which co-operated with other local impurities, and added to the insalubrity of the climate" (p. 29). Dr. Johnson, already cited, adds, that a similar disease followed the destructive fires of Augusta, and, he believes, Wilmington, N. C. Of these events I cannot get any information. On the 15th of the republican month of Pluviose, the French army, under the order of General Leclerc, landed at port Lacul, twelve leagues from the City of the Cape. On the same day that city was almost entirely destroyed by fire, and the troops were established amidst its ruins. Not long after, the yellow fever, in its most aggravated form, broke out among them, and destroyed the greater portion of their number. Gilbert, and others who have furnished us with accounts of that dreadful epidemic, have assigned to that conflagration a large share in its production.¹

Not always in Seaports.—We have seen that one of the reasons assigned in support of the contagious character of the yellow fever of this city and country—and of its importation from abroad—is, that the disease invariably commences in seaports which entertain a commercial intercourse with infected places, while inland towns, which are equally exposed to the ordinary causes of autumnal fevers, escape; that in such ports the disease first appears near the shipping, and thence extends in various directions; that in several instances of its occurrence in Philadelphia and other cities, the suburbs as well as the adjacent towns, so far from being equally visited, were as healthy as in corresponding seasons of other years and even sometimes freer than usual from disease; and that in ports visited by the fever, the latter is circumscribed within comparatively narrow limits, the balance remaining free unless cases are brought there from less favoured spots. To a certain extent, this argument, which has been equally urged in reference to the yellow fever of other countries, is founded on correct data; but the inferences drawn from these are erroneous. Many of the facts adduced are found to admit of a very different interpretation, while others are neutralized by a different and larger set of an opposite kind, the authenticity of which cannot be doubted.

As a rule, the yellow fever certainly seems to require for its development, the close atmosphere of cities or towns. The disease is usually found also to arise in, and to be limited to, particular portions of these, while the rest remains free, or nearly so; not only from the disease itself, but sometimes from febrile complaints generally. Nor is it less true, that the fever is oftener found in seaports, or on the seaboard, than elsewhere; that it breaks out near the shipping; and that, commencing at a particular spot, it radiates thence to a greater or less distance. From all this, however, the conclusion of its importation and contagious character cannot necessarily follow. It would not do so, even were there no reason for doubting or denying the universal occurrence of such a point of origin or location. In the first place it

¹ Gilbert, Hist. Méd. De l'armée Française à St. Domingue, pp. 7, 70.

may be remarked, that though in tropical regions the yellow fever is often found to be diffused over a larger surface than it is in our latitudes, it is for the most part as much a disease of seaports or of the seaboard as it is with us or in Europe; that there, as here, it very usually breaks out in the vicinity of or among the shipping; that from thence it irradiates, and that it seldom appears in inland towns. In a word, much of what takes place in temperate regions in these respects, takes place in tropical climates; and hence, if from the facts observed in the former we conclude that the disease is there imported and contagious, we cannot refuse our assent to the conclusion, that in the latter also it is imported and contagious.

Contagionists, of course, subscribe willingly to the latter part of the conclusion; but, while doing so, they know and admit that the disease *originates* in tropical climates. They must also admit therefore, that in these, the air of seaports or of cities, and more particularly that portion of them situated in the vicinity of the shipping or the water, is requisite for the production of the disease; and if they do so in reference to those regions, it is difficult to perceive on what they ground their denial of the disease arising in this country or in Europe from the same causes, and infer from its appearing here in like localities that it must have been imported. All that can be adduced from the circumstance of its selecting such localities, here and elsewhere, is that a peculiar morbid agent is formed and exists there better suited to produce the fever than is found to be the case in other portions of the same cities, and more particularly in inland towns, or in adjacent and rural situations. If that peculiar cause is found to exist in one region, it may exist in another; and there is no necessity, when the fever appears in the one, to ascribe to it a mode of origin different from that recognized in the other.

As regards the limited extent of the disease—the exemption from it of parts of the affected cities situated at even a short distance from the infected districts, of adjacent villages and towns, and of the surrounding country—I shall say nothing here. So far from affording support to the doctrine of importation and contagion, it tends, when taken in connection with the fact of the non-transportation of the disease from the localities affected to such adjacent places, to establish the contrary doctrine. But all that need be said on these subjects has been already, or will be subsequently, noticed in other chapters. At present it will be sufficient to call attention to the fact that, however true it may be that the fever has generally approved itself to be one of seaports and places having commercial intercourse with foreign or distant places, and spares inland towns, the opinion of its exotic origin and contagious character is far from deriving from the peculiarity of this location the support alleged; for facts may be, and have been, adduced to show that the disease has not only originated in places distant from the sea-coast, where the influence of wharves or shipping of any sort could have had no agency in its production, but has not unfrequently taken birth and prevailed in places very different from those mentioned, and at a greater or less distance from seaports.

Thus, although in this country it has very generally been confined to the Atlantic plain, or tide-water region, extending from Boston to Galveston, and

has usually visited the seaports of that region; although in Europe it has been nearly confined to localities of kindred nature, and has prevailed in cities situated along the sea-coast—Barcelona, Cadiz, Malaga, Carthagen, Leghorn, &c.; although, as we have seen, the fever in the tropics, however more extensively diffused, is, as it is in temperate climates, most commonly limited, when in the epidemic form, to cities and towns situated on the sea-coast, or on rivers opening on the latter; and although it usually commences and prevails most along the wharves or quays, or among the shipping—the experience of the United States, Europe, or the West Indies does not by any means confirm the opinion that it never originates at a considerable distance from the sea, and that it appears exclusively in seaports, particularly in those which have commercial relations with infected places far or near. We have already seen that, when it breaks out in seaports, its starting point is not always discovered amid the shipping or along or near the wharves. In many instances, indeed, it has appeared at a considerable distance from these. Again, there is reason to believe that it may occur in places remote from seaports, or in parts or portions of the sea-coast where vessels from infected countries never show themselves, provided always the temperature and local peculiarities of such localities be such as are required for the production of the disease elsewhere.

We need not insist on those instances of fever mentioned by Volney (ii. 310), Professor Potter, of Baltimore (pp. 16–18), Dr. Walker (iv. 74), Dr. W. Harris (*ib.*, p. 75), Dr. Anthon,¹ and several others recorded in some of the early volumes of the *Medical Repository* and other publications, as occurring in New Design in 1797, Bald Eagle Valley (Pa.), the Eastern Shore of Maryland, on the Illinois, &c.

The febrile complaints observed on those occasions were then, and are still, regarded as instances of true yellow fever, by the above-mentioned and some modern writers. Nevertheless, it may reasonably be suspected that the greater number, if not the whole, of these were cases of bilious remittent fever—of a malignant character, doubtless, but still not yellow fever—especially as we are aware of the frequency of the latter fevers in those and like localities, and that the writers mentioned made no distinction between bilious and yellow fever proper. Nor shall I call attention to the fever which prevailed epidemically in Virginia about the middle of the last century, and the oft-cited description of which was given by Dr. J. Mitchell,² inasmuch as doubts are justly entertained as to the nature of the disease. But, laying these aside, we find sufficient evidence, in this country, of the disease having occurred at a distance from seaports. In 1803, it broke out at the village of Catskill, situated more than one hundred miles from the city of New York.³ In 1825, we find it, as already stated, in the village of Washington (Miss.), and, in 1844, at Woodville, in the same State; both of which places are at a considerable distance from a port of any kind. It has prevailed on several occasions (1817, 1823, 1825, 1837, 1839, 1847, 1848, and 1854) at Natchez, the distance of

¹ Miller p. 143.

² See Med. Mus., i. 1, &c.; Med. and Phil. Reg., iv. 181.

³ Med. Reposit., viii. 105.

which from New Orleans is three hundred miles. It has shown itself at Vicksburg, Grand Gulf, Memphis, and scores of other places which lie on the Mississippi River, considerably above the mouth. It has prevailed on the Red River, and other tributaries of the Mississippi, to which, as in the former, ships from infected places of tropical regions do not reach.

In the summer of 1853 it broke out in Brandywine village, which cannot well be regarded as a seaport. In 1819, a yellow fever year in several of our Atlantic cities, a bilious remittent fever, of a malignant tendency, prevailed in the States of Missouri and Illinois; and although the disease assumed a milder character than the fever that was prevailing on the seaboard, yet many cases reported by the army physicians to government "had the characteristic features of typhus icterodes so decidedly marked, that the surgeon-general entertained no doubt of the identity of the disease."¹ In 1822, Marietta (Ohio) was visited by a serious epidemic, which carried off many individuals. The disease was characterized by most of the pathognomonic symptoms of the yellow fever—pain over the eyes, in the back, loins, and joints; a paroxysm of forty-eight or seventy-two hours, followed by a metaptosis; light coated tongue; continual and great nausea; anxiety and distress about the præcordia, and incessant vomiting, in the latter stage, of dark-coloured matter mixed with mucus, and resembling coffee-grounds; yellow and saffron hue of the eyes and skin, first commencing in the former, thence about the forehead and neck, and thence over the whole surface. In this instance the disease, as often occurs in true yellow fever, was preceded by excessive drought, heat, and aridity, as well as by swarms of insects. It commenced on the 1st of August, was very general in September, and gave way to the heavy frosts of November.²

Long before this—in 1796—a fever broke out at Gallipolis, a town on the Ohio, below the Great Kanawha, which bore so close a resemblance to the yellow fever, as to be viewed as such by able authorities. It was first noticed by Mr. Ellicott,³ who visited the place on the 15th of November of that year. Mr. Ellicott states that the mortal cases were attended with the black vomit, and adds that this disease certainly originated in the town, and, in all probability, from the filthiness of the inhabitants, added to an unusual quantity of animal and vegetable putrefaction located within the village. "The fever could not have been taken there from the Atlantic States, as my boat was the first that descended after the fall of the waters in the spring; neither could it have been carried there from New Orleans, as there is no communication, at that season of the year, from the latter to the former of those places; moreover, the distance is so great, that a boat would not have time to ascend the river, after the disorder appeared that year in New Orleans, before the winter would set in."

I am aware that by several authorities the claims of this disease to be re-

¹ Statistical Rep. of the Sickness and Mortality of the Army of the United States, p. 8.

² Hildreth, Phil. Med. and Phys. Journ., ix. 110, &c.

³ Journal of a Voyage down the Ohio, in Nov. 1796, by Andrew Ellicott, p. 13.

garded as yellow fever have been impugned. Dr. Hosack adduces the testimony of Mr. Ellicott himself, in opposition to such claims, and states that in a conversation he had with that gentleman on the subject, a short time previous to his death, and in the presence of Dr. Francis, he declared that the fever he had noticed wanted the more violent symptoms associated with yellow fever; adding that, "at the time he made the observations in his journal, he had very little acquaintance with the malignant yellow fever, but that he had seen much both of the pestilential fever of New York and Philadelphia, and of the lake or marsh fever in different districts of the United States, and was now convinced that the yellow fever and the fevers of the interior of the country were of an entirely distinct character."¹ More recently, the late Dr. Drake, while remarking that Galliopolis is the only town of the Ohio basin which has been charged with generating yellow fever, is decided in the opinion that the disease alluded to by Ellicott was nothing more than the malignant remitting fever, with which the physicians of the West are now so familiar. To this opinion he is inclined from having witnessed the autumnal fevers of sickly localities on the banks of the Ohio, since the year 1800, without having seen a prevalence of the yellow fever, though occasional cases have closely resembled that disease; and also from the fact of the distance of Galliopolis from the sea, its elevation above its surface, not less than the rural, instead of urban, character of the infant village (i. 292).

There is no doubt that, had we no other notice of the fever in question than the one we owe to Mr. Ellicott, we should be justified in joining in opinion with Dr. Drake; for, as correctly stated by him, the former has not described the disease in such terms as to show that it really was identical with that prevailing at the time on the Atlantic coast. The only symptom mentioned, capable of establishing the identity, is black vomit; and we know that under that term very dissimilar substances have been described by unprofessional, and even professional, writers, and, reversely, that the real black vomit occurs in diseases different from the yellow fever; while the infrequency of the disease in the basin of the Ohio, and the frequency of fevers which, though bearing sometimes a strong resemblance to it, are different in many essential points, would incline us to believe that Galliopolis suffered from the latter rather than the former. But when taking into consideration the fact that it is yet to be proved that the distance of the village from the sea can be urged as an obstacle to the prevalence of the yellow fever there; that Galliopolis, the elevation of which above the level of the sea is not more than six hundred feet, is not by half as high as some spots where the fever has raged with much violence; and that the rural character of a village has been no barrier to its occurrence elsewhere—when, I say, with these facts before us, we turn to other statements within our reach, I feel disposed to view the subject in a different light. In a communication made to Dr. Potter, of Baltimore, by Major Prior, of the United States army, who witnessed the rise and progress

¹ *Laws of Contagion*, in *Essays*, i. 343. See also Letter of Dr. Francis to Dr. H., on this subject, *Appendix to Dr. H.'s Med. Police*, p. 71; also *Essays*, ii. 78.

of the epidemic, both among the inhabitants and troops contained in the place, we find a more graphic and detailed account of the disease than that furnished by Mr. Ellicott. From that account we learn that in the course of five days half the garrison was on the sick list, and in ten days half of them were dead. "They were generally seized with a chill, followed by headache, pains in the back and limbs, red eyes, constant sick stomach or vomiting, and generally, just before death, with a vomiting of matter like coffee-grounds. They were often yellow before, but almost always after, death. The sick died generally on the seventh, ninth, and eleventh day, though sometimes on the fifth and on the third.¹

If the disease which presented such symptoms—which destroyed one-half of those attacked, and killed in so short a space of time—is to be considered as nothing more than the common bilious remittent of the country, all we can say is that it resembled so closely the yellow fever, that were a similar disease to break out at a corresponding season in one of our seaport towns, it would be, in all probability, considered by our contagionists as a legitimate specimen of that fever and, as a matter of course, of foreign origin. To this it may be added that the disease was preceded by intensely hot and dry weather; that it appears to have been circumscribed within the village and cantonment; that it was, with apparent propriety, attributed to exhalations evolved from a large pond, which had been partially filled up, two or three years before, by felling a number of large trees that grew on and near its margin, and covering the wood with earth; that, previous to the sickness, the water contained in this pond had been considerably evaporated, leaving a large quantity of muddy water, with a thick, slimy mixture of putrefying vegetables, which emitted a great stench; and, finally, that the disease was arrested by cutting a ditch, conveying off the remaining water, and covering the whole surface with fresh earth.² All these circumstances are more like those connected with the production and prevention of yellow than of ordinary remitting fever.

In Spain, the fever has more frequently than in this country appeared in inland situations, away from the influence of wharves and of shipping. It prevailed with much severity at Seville, the distance of which from the mouth of the Guadalquivir, on which it stands, is some eighteen leagues, or upwards of fifty miles. Nor was it less fatal at Cordova, which lies at thirty leagues from the Mediterranean, and forty-five from the ocean. Argo, Fraga, Xeres, Tortosa, Mequinanza, Nonaspe, Lebrixa, Medina Sidonia, and Carlota, situated at distances from the sea varying from three to twenty-six leagues (fifteen to seventy-eight miles), all suffered extensively at various periods.

Finally, even in the West Indies, where, from the mountainous character of the inland localities, and the consequent restriction of towns and cities to the flat alluvial lands of the sea-coast, examples of the prevalence of the yellow fever at a distance from the influence of seaports must necessarily be rare, we discover evidence of the impropriety of limiting the disease to these.

¹ Potter on Contagion, p. 15.

² Ibid., pp. 15, 16.

Stony Hill, in the island of Jamaica, lies at a distance of nine miles from the city of Kingston, and at an elevation of 1,360 feet above the surface of the sea, and yet the soldiers stationed there have been visited by the most deadly epidemics of yellow fever. Spanish Town, five or six miles, and Lacovia, twelve miles from the same city, have likewise often suffered severely; Ivonet, in St. Domingo, was, during the English occupancy of the island, the seat of a fatal visitation of the same disease; and we know, from Humboldt, that the fever is occasionally seen as far as ten leagues from Vera Cruz (p. 780).

Doubtless the contagionists will answer, and have said over and over again, that in all these situations the fever has been introduced from seaport towns. But they have said, and continue to say, the same of seaports themselves, and of every place where the disease has appeared, and where its local origin has been established beyond the possibility of doubt. Even were these views well founded in relation to some of the inland situations mentioned, the local origin of the fever at Medina Sidonia, and other inland towns of Spain, for example, rests on evidence of such indisputable character, as to have satisfied Arejula himself; and if we admit the fact that the disease may even occasionally originate at a distance from seaports or shipping, the opinion of those who contend for the necessity of the agency of these, and derive from this necessity a proof of importation and contagion, must fall to the ground; while we reach the conclusion that if the occurrence of the fever in inland situations is not frequent, the cause must be sought in the fact of the more usual absence there of those local peculiarities associated with want of ventilation, continuance of high atmospheric heat, &c., which are found to exist so generally and so conspicuously in seaport towns.

The facts I have adduced in the foregoing portions of the present chapter, as having reference to the connection of the disturbance of the soil, inundations, and of extensive fires, with the manifestation of yellow fever in an epidemic form, are too numerous to be held in the light of coincidental casualties. Such occurrences observed so frequently, in so many diversified places, and bearing so close an analogy to others followed by the manifestation of different forms of malarial fevers, may very naturally, when the disease produced is the yellow fever, be supposed to exercise some agency in reference to its production and prevalence. They show that, in all the instances in which they are observed, the productive agency is of a domestic nature, and cannot fail, therefore, to afford a powerful aid to the opponents of the doctrine of importation, for if the disease can on some occasions arise and spread through the operation of such agencies—under circumstances which impress us with the belief that no extraneous causes have been at work—there can be no necessity for attributing the manifestation of the disease in other instances to a contagious and importable poison, especially when we know that it has often sprung up in places where such an importation was totally out of reach of possibility.

CHAPTER XXII.

PROOFS OF NON-CONTAGION—CONTINUED.

Local Origin of Yellow Fever illustrated by its Occurrence on board of Ships.

—Enough has been said to show that the annals of the yellow fever in this country, in Europe, and in tropical climates, are not deficient in facts tending to establish the local origin of the disease, and its complete independence of a contagious principle or virus emanating from the sick, and transmissible from one individual or place to another, through means of a personal intercourse with these, or the indirect agency of fomites. Of such facts none appear better calculated to illustrate the reality of that origin than those derived from the records of the rise and spread of the fever on board of ships, occurring, as they often have done, under circumstances which forbid the supposition of a foreign or contagious source of contamination, and point to the vessel itself as the focus of infection. That by some writers, more or less entitled to our regard, the spontaneous development of the disease on ship-board, from causes of the kind, has been denied or strongly doubted, and that attempts have not unfrequently been made to impugn and explain away the many facts adduced in its support, is doubtless true. Faithful to his belief in the doctrine of contagion and importation, Bally, whose intelligence and learning it would be unsafe to question, but whose soundness of judgment no one, judging from the course he has pursued in the controversy, will feel disposed to commend, boldly affirms that the yellow fever has never originated, in the way mentioned, in ships.¹

Not less certain is it that a still higher authority among contagionists of the purest water, Kéraudren, whose official position as physician-in-chief of the French navy necessarily afforded him ample means of information, may be appealed to, by the opponents of that mode of development, as a warm and intelligent supporter of their views.² Nay, among non-contagionists themselves, there are not wanting those who, while admitting the local origin of the yellow fever on shipboard, discard the idea of referring the disease there—or, indeed, anywhere else—to the agency of sources of malarious infection, and have taken special pains to show that it is due to changes of habits, exposure to the sun, sudden vicissitudes of temperature, cold, heat, humidity, fatigue, fogs, moral causes, and a variety of other morbid influences of like import.³

But however willing we may be to respect the authority of these writers,

¹ Du Typhus d'Amérique ou Fièvre Jaune, pp. 357, 8.

² De la Fièvre Jaune, observée aux Antilles et sur les Vaisseaux du Roi. 8vo. Paris, 1823.

³ The Influence of Climate and other Agents on the Human Constitution, &c., by Robert Armstrong, M. D., pp. 57, 61, 76, &c.

and of others entertaining kindred views—however true it may be that some of the statements on record, and pointed out as illustrative of the mode of origin in question, cannot be fully relied upon, and that there is reason to believe the disease in some instances to have not been the true yellow fever, and that in others it was the offspring of extrinsic influences, and not of a cause evolved on board of the infected vessels—still, a careful perusal of the numerous facts we possess on the subject before us, so far from justifying the propriety of disarding as spurious and erroneous all that has been said in support of the malarial origin of the fever on shipboard, will fully sustain the opinion of those who ascribe, in very many instances, the appearance and spread of the disease in such localities to the operation of morbid effluvia generated in the timber of the vessels themselves, or the materials these may contain.

Let it not be supposed that the correctness of these conclusions is upheld by none but anti-contagionists. So far from this being the case, they include among their advocates writers who have taken an active part in support of the contrary doctrine, and who, strikingly as the admission conflicts with the views they entertain respecting the usual source of the disease, and impossible as they may find it to reconcile the facts they adduce with the theory they profess on the latter subject, do not hesitate to express their belief in the occasional spontaneous development of the yellow fever poison on board of ships. Sir Gilbert Blane himself, than whom we scarcely know a more zealous and uncompromising contagionist and importationist, and who consequently was too deeply interested in the matter to make an admission in opposition to his recorded views which he did not know to be well founded, at the same time that he was too well acquainted with the disease to commit a gross error of diagnosis, remarks that experience has proved that “one of the most prolific sources of foul air and bad smell in ships has been the putrescent matters absorbed and retained by gravel, sand, and other earthy substances heretofore used for ballast.” He alludes to those cavities under the floor of the holds, which used to be receptacles of filth and of all manner of vermin, dead and alive, proving perpetual reservoirs and sources of foul air and of offensive and noxious exhalations.¹ In another essay on yellow fever, he more pointedly states that the West Indian fevers “sometimes are found to arise from the foul vapour of ships replete with filth from long neglect of cleanliness;”² and in his excellent work on the *Diseases of Seamen* we are told: “With regard to the effect of putrid exhalations, I need only mention that at the time of the battle of the 12th of April, 1782, there was not a sickly ship in our fleet, but many of the officers and men who were sent to take care of the French prizes were seized with the yellow fever; and it was observed that when at any time the holds of these ships, which were full of putrid matters, were stirred, there was an evident increase of those fevers soon after” (p. 609).

¹ On the Health of the Navy, Med.-Chir. Trans., vi. 507, 510; Select Dissertations, i. 36, 38.

² Select Dissertations, ii. 119.

A distinguished countryman of ours, the late Dr. Currie, of this city, who never could discover the most remote reason to admit that the yellow fever had ever originated in this country, thought that "crowded transports or ships of war generally, if not always, constituted the original and proper sources of the matter of contagion or the poison of the disease."¹ Other admissions, equally conclusive, by contagionists of high authority, will be adduced as we proceed, and would alone prove sufficient to counterbalance the doubts and denials to which attention has been called. But even were we unable to support the opinion of the origin of the yellow fever on ship-board from local sources of infection, and independently of external influences, by appealing to the testimony of Blane, and other writers who entertain views similar to his own respecting the etiology and mode of propagation of the disease, we should be led to its adoption by a survey of various occurrences on board of government and merchant vessels of this country, England, and France.

Regarding many of these as establishing the reality of that origin beyond the reach of cavil and disputation, and considering, besides, that they lend a strong support to the doctrine which ascribes autumnal and other febrile diseases to the agency of specific poisons floating in the atmosphere, and lead to the inference that on land the yellow fever, when it breaks out, must likewise be due to effluvia issuing from kindred sources of contamination, and not to the importation of a contagious virus from some distant infected place, I have thought that a condensed account of them, collected from various publications—many of which are now little known or difficult of access—might serve a useful purpose, as affording important materials for the settlement of the long-mooted question of the mode of origin of that formidable disease.

In the performance of this task, attention might perhaps be called to the history of the disease as it prevailed on board the *Princess Caroline*, at Curaçoa, in 1763, as so graphically described by Rouppe;² on board of the *Majestic*, at Port Royal, Martinique, in 1795;³ of the *Ganges*;⁴ of the *Peacock* and *Grampus*;⁵ the *Sea Island*, at Middletown, in 1820;⁶ the *Polly*, at Chatham, Conn., in 1796;⁷ the *Ten Brothers*, at Boston, in 1819;⁸ the *Favourite* and *Ocean*, at Perth Amboy, in 1811;⁹ the various vessels from New Orleans, at Staten Island, in 1848;¹⁰ the *Bann*, at Ascension, in 1823;¹¹ the

¹ Diseases of America, p. 60.

² Diseases of Seamen, p. 410.

³ Gillespie, p. 31.

⁴ Cartlet, Med. Reposit., iv. 243.

⁵ Barrington, Am. Journ. of Med. Sci., xii. 308.

⁶ Beck's Report, in Hosack on Med. Pol. Essays, ii. 65; Tully and Miner's Essays, p. 319.

⁷ Tully and Miner, p. 356. See also Letter to Dr. Hosack, N. Y. Med. Journ., i: 153; Webster on Epidemics, ii. 344.

⁸ New England Med. Journ., vol. viii.; North Am. Rev., x. 397; Watt, in Med. and Surg. Reg., p. 226.

⁹ Report of Drs. Hosack, Bayley, and Douglass, Med. and Phil. Reg., iii. 95; Edinb. Med. and Surg. Journ., viii. 165.

¹⁰ Communication from the Committee appointed to inquire into the Propriety of the Removal of the Quarantine Establishment, p. 14, &c.

¹¹ Burnett's Report, p. 3, &c.; Blane, Letter to Mr. Croker, N. Y. Journ., iii. 173;

Palinure, at Martinique;¹ the Néréide;² the Expéditive;³ the Gloriole;⁴ the Eglantine;⁵ the Africaine;⁶ the Middleburg;⁷ the Chichester;⁸ the Herminie;⁹ the Vestal;¹⁰ the La Ruse and the Grayhound, at the Wallabout, in 1804;¹¹ the Ann Maria, at the New York Quarantine, in 1821;¹² the Alban, at Port Royal, Jamaica;¹³ the Snake, at St. Jago, Cuba;¹⁴ the Tartarus, Crocodile, Dee, Satellite, Hecla, Megæra,¹⁵ and others of which we have the records.

But although the disease in those instances very apparently originated on board, yet as in some it commenced while the ships lay at anchor near an unhealthy shore, or in sickly ports, and were hence within reach of external morbid influences, or, in others, a short time after they had left such places, and as, consequently, serious doubts may be entertained as to the propriety of ascribing it positively to causes evolved on board, the cases in question may be laid aside as inconclusive. For a similar reason it may perhaps be proper to omit adducing, as has been often done, the epidemic which occurred on board the U. S. ship Delaware, in 1799–1800, at Curaçoa, an account of which will be found in an early volume of the *New York Medical Repository*.¹⁶ In this account, Dr. Anderson, while stating that during the prevalence of the fever the crew suffered from a highly nauseous smell between decks—which was not the result of effluvia from the accumulation of the sick, as it remained long after they were removed—says nothing of the cause of the disease being in the ship, and seems even to attribute it to exhalations from the shore. And yet, from the following statement by the late Dr. Vaughan, of Wilmington (Del.), contained in a letter to Dr. E. Miller, of New York, it would appear that the disease on that occasion broke out under circumstances which render its origin from local sources of infection on board more than probable. Let the impartial reader judge. In speaking of the ship in question, Dr. V. remarks:—

“Dr. Anderson, of that ship, informed me that the disease unquestionably

Thompson on Anat., &c., *ibid.*, p. 168; *Med.-Chir. Journ. and Rev.*, iv. 916; *Ibid.*, N. S., ii. 3; *Brit. and For. Med.-Chir. Rev.*, i. 62.

¹ Sir G. Blane, ii. 141; *Dict. des Sci. Méd.*, vol. xv.; Strobel, p. 19; Palloni, pp. 42, 43; Opinion de M. Lefort, pp. 15, 16; Moreau de Jonnes, p. 121; Chervin, Report, p. 8; Dickson, in Bancroft's Sequel, pp. 172, 173 (note).

² Kéraudren, p. 37.

³ *Ibid.*, p. 38.

⁴ *Ibid.*, p. 40.

⁵ *Ibid.*, p. 42.

⁶ *Ibid.*, p. 42.

⁷ Lind on Hot Climates, p. 133.

⁸ Doughty on Yellow Fever, p. 16.

⁹ Maher, Relation de deux Épid. de Fièvre Jaune.

¹⁰ Wilson, Stat. Rep. of Health of British Squadron, p. 109.

¹¹ N. Y. Med. Rep., viii. 97, 98; Smith on Epidemics, p. 80.

¹² History of the Proceedings of the Board of Health of New York in 1822, pp. 7, 8; Smith on Epidemics, p. 80; Letters on Yellow Fever of 1821, and Report of Jos. Bayley thereon, p. 6, &c.; Vaché, Letters on Yellow Fever, &c., p. 8.

¹³ Bryson, Statistical Report of the Health of the Navy (Brit.), p. 95.

¹⁴ *Ibid.*, p. 96.

¹⁵ *Ibid.*, pp. 101, 106, 121.

¹⁶ An Account of a Bilious Yellow Fever which prevailed in the United States ship Delaware, &c., by S. Anderson, *Med. Reposit.*, v. 280.

originated on board, in the harbour of Curaçoa, while the inhabitants of the island were perfectly healthy. As soon as the nature of the disease was known, they put to sea, in hopes of receiving advantage from a free circulation of air; but the sick-list increased daily, and they returned to the harbour of Curaçoa in a much worse condition than they left it. Forty sick were landed; and, though there was no restriction in intercourse with the inhabitants, there was not a single suspicion of contagious influence."¹

But, whatever uncertainty there may be thought to exist respecting the ship Delaware, there can be none as regards the origin of the disease on board the U. S. frigate General Greene, in 1799. The history of that visitation has often been cited, and must be familiar to many American medical readers. The ship left Newport (R. I.) on the 3d of June, and reached the Havana on the 4th of July. The disease commenced during the passage, and extended rapidly. The crew thus affected had had, to use the language of Dr. Kollock, to whom we are indebted for an account of the occurrence,² no communication with any vessel at sea, nor had they touched at any place in their passage, or even had intercourse with the town, or vessels in the harbour, till after the breaking out of the disease. Other vessels in port were no more than usually sickly, and the inhabitants of the island were remarkably healthy. "The principle of disease," continues that writer, "seemed to have been generated on board, and to have gradually acquired virulence and activity as they approached the place of their destination." "Those who frequented the hold, and were stationed in the neighbourhood of the pumps, suffered more especially during the continuance of the disease, as they had been the first attacked by it." It may be remarked that the ship being new, may be supposed to have been in a cleanly condition. But, having been caulked in winter, her seams opened as the warm weather advanced, and her ballast consisted partly of iron, and partly of earth, taken from a clay shore, intermixed with soft slate stone, shells, and marine vegetables. During the passage, the vessel experienced a violent storm, which was followed by very hot weather, and the extreme putrefaction of a quantity of codfish that was on board. From these causes combined, the air in the hold became soon so contaminated as to extinguish lights immediately. The disease was identified as yellow fever by Dr. Halliday, an experienced practitioner of the Havana, and author of a clever essay on the disease as it appeared there in 1794.³

In the following year, 1800, the fever once more broke out in the ship, after a long cruise in warm climates, where, however, she had not been exposed to morbid effluvia from the shore or contaminated ships, and where the crew had remained all the time unusually healthy. The frigate reached the Chesapeake on the 11th of July. Thence, after a detention of three days,

¹ Med. Repos., iv. 245.

² An Account of the Malignant Disease which appeared in the U. S. frigate General Greene, Med. Repos., iv. 2, 3.

³ A Short Account of the Origin, Symptoms, and most approved Method of Treating the Putrid Bilious Yellow Fever, &c., which appeared in the city of the Havana, &c. Boston, 1796.

she sailed for Newport, where she arrived on the 21st. Some cases of bilious fever had occurred prior to her reaching the Chesapeake, but they were readily controlled by the usual remedies. "Every customary method of preserving purity of air and the health of the seamen had been assiduously observed. The ship was freely ventilated, scoured, whitewashed, sprinkled with vinegar, and the nitrous fumigation" "was frequently excited between decks." Nevertheless, the yellow fever broke out soon after she had left the mouth of the bay, and continued to prevail after her arrival at Newport, when it assumed a more malignant and fatal character, and attacked some individuals from the town who worked on board, or bathed near her at the time her bilge-water was pumped out.¹

The next case I shall mention is that of the United States ship *Hornet*, which was visited by the yellow fever in September, 1828, while lying at Sacrificios, a small island about three miles from Vera Cruz. Dr. Barrington, of the Navy, to whom we are indebted for an account of this fever,² states that there "was no epidemic in the city of Vera Cruz, excepting the dengue; nor was the fever prevalent at any place where the ship had touched on her cruise." From this, we may conclude that the fever originated on board—an opinion rendered the more probable by the circumstance that the vessel was at the time in a condition well suited to the evolution of noxious effluvia. "In the winter of 1827, the *Hornet* had undergone extensive repairs; she was 'salted' and consequently very damp; the moisture collecting in drops on the beams, &c., when the atmosphere was charged with vapour. When she was 'broken out' at New York, after her return in 1828, great quantities of mud and other filth were taken from her hold; and in her timbers and lower works was discovered a considerable collection of chips and shavings in a putrid state, which had fallen there during repair. The bilge-water and smell from the hold of this ship were exceedingly unpleasant.

"During the two months previous to this time (the breaking out of the disease), the weather had been clear and oppressively warm, with very little rain—the thermometer averaging 87° at noon; at no time above 90, or under 79°. It was kept on the berth deck, immediately under the main hatch, in as cool a situation as any between decks.

"When the *Hornet* arrived at the Navy Yard, Pensacola (in lat. 30° 17' N.), from Vera Cruz, we had," says Dr. Barrington, "but two new cases. After leaving Pensacola, on our way to New York, no one was attacked until we got into the latitude of the southern capes of Florida (about 24° 30'). While here, the temperature varied from 78° to 82°, and we had an increase of seven new cases to our sick-list, two on the 5th, and five on the 7th Nov., and one more, being the last, on the 8th. From this time, as we increased our northern latitude, and the thermometer gradually descended towards the freezing point, we had not another case, and those already attacked rapidly convalesced" (p. 307).

¹ Account of the Yellow Fever which originated on board the U. S. frigate *General Greene*. By E. T. Waring, *Med. Repos.*, iv. 234.

² *Amer. Journ. of Med. Sci.*, xii. 307-8.

Here again there could have been nothing like the introduction of a contagious germ. While at Sacrificios, the vessel was at too great a distance from Vera Cruz to have been contaminated by morbid effluvia, evolved in that city and wafted through the agency of the wind. But even had the ship been lying within infecting distance from that city, no yellow-fever epidemic existed there at the time. Nor could the disease have been derived from a malarial poison originating at Sacrificios or on the neighbouring coast, as the fever that usually prevails there is of a character different from that which appeared on board. Independently of this, it may be remarked that the ship contained materials which, on land, have often been found associated with the development of the disease, and was in a condition such as to suggest the idea of the evolvment of deleterious effluvia; as well as placed under atmospheric influences calculated to favour or produce that result. It must be noted, also, that the disease disappeared on the vessel reaching a cold latitude—an occurrence unusual in regard to fevers not due to local causes.

Much to the same effect is the case of the United States ship *Levant*, the crew of which suffered severely from the yellow fever at Pensacola, in 1841. This case, I am aware, was adduced by the late Dr. Carpenter, of New Orleans, a warm and uncompromising advocate of the doctrine of contagion,¹ in proof of the introduction of the fever on that occasion into Pensacola from the West Indies. But no facts of a satisfactory kind have been brought forward to establish the reality of that introduction, while a variety of circumstances induce the belief that the fever originated on board the ship. Dr. Hulse, of the U. S. Navy, who, in a clever essay on the yellow fever,² adverts to this case in evidence of the local origin and non-contagious character of the disease, makes the following statement which goes far to establish the correctness of that opinion:—

“On board of the *Levant* sloop of war, we have numerous facts to prove that there existed a cause additional to that of the atmosphere, in the foul state of the hold of the ship. This ship was lying opposite to Pensacola, during the month of August, and on the last day of that month, four cases of yellow fever were sent from her to the hospital. In three or four days, she dropped down to the navy-yard, dismantled, and her crew were sent on shore at the yard—a portion of them still communicating with her; and on the last day of September, ninety-nine cases of yellow fever had been admitted into the hospital from her officers and crew. New cases continued to occur among the ship’s company, now located at the navy-yard, until we received thirty-nine more, making in all one hundred and thirty-eight cases from this single ship! The disease did not disappear till the 5th November, after several severe frosts.” During the whole of this time, not more than one or two well-attested cases occurred among the men connected with the yard; and as they were within infecting distance of the ship, and all the other cases

¹ Sketches from the History of Yellow Fever; showing its origin, and disproving its domestic origin, and demonstrating its transmissibility, p. 29.

² Maryland Med. and Surg. Journ., April, 1842, pp. 392-4.

were traced to the latter, the conclusion is natural that the disorder was due to causes therein contained, and not to any external influence.

Much more interesting than the preceding, and more deserving of our serious consideration, is the case of the prevalence of the yellow fever on board of the frigate *Macedonia*, in the summer of 1822. This case gave rise to much controversy, and resulted in a Court of Inquiry,¹ from the record of which the following narrative is derived.

The *Macedonia*, under the command of Captain James Biddle, sailed from Boston, at the navy-yard, of which she had recently been fitted out for the West Indies, on the 2d day of April, and reached her destination with a healthy crew, about the close of the same month. While in the harbour of the Havana, where she lay from the 28th of April to the 4th of June, with the exception of one day, and which, like Cape Haytien and Port au Prince, where she subsequently touched, was remarkably healthy, malignant yellow fever broke out aboard. The first case occurred on the 8th of May, and ended fatally on the 11th; another died on the 19th, after which the disease extended rapidly among the crew, and carried off several of the officers. On the 4th June, the ship sailed for the island of St. Domingo, and on the passage the disease continued to prevail; though the number of new cases lessened, and the sick appeared better while at sea. But at Cape Haytien and Port au Prince, the sickness and mortality continually increased, and the ship became so infected that there was no reasonable ground for expectation that the crew could be relieved but by change of situation or climate. The captain, therefore, after returning to the Havana, and staying there a short time, sailed for the Chesapeake, on the 24th of July (p. 40).

The ship seems to have been, apparently at least, in an average or even clean state, except as regards her hold, the air in which, after she had arrived at the Havana, was found by the officer in charge to be very dense, close and disagreeable, as well as hot. The hold was not at first offensive to the smell or otherwise, except once, when, on removing four casks, situated under the fore hatch, a quantity of dirt was discovered. While at the Havana, no other of the ground tier of casks were removed, and no other dirt discovered at the bottom of the hold. The latter, at the time of the fitting out of the ship, which had lately arrived from the coast of Brazil, was in a very dirty condition, for a large quantity of mud was found in her; the casks were rotten, and the water pumped out was black and muddy. Much of this was removed, so that when the ship sailed she appears to have presented nothing highly objectionable. Nevertheless, Capt. Biddle, in his letter to the Secretary of the Navy, positively asserts that the great bulk of the ballast, although covered with mud and dirt, was neither moved nor properly cleansed, and that the hold was stowed with this horrid and offensive matter remaining it. "Offensive, indeed, it might not have been at Boston, in the winter season;

¹ Minutes of the Proceedings of the Court of Inquiry ordered by the Secretary of the Navy, on the application of Capt. J. Biddle, in October, 1822, Washington City, 1822.

but it is obvious that it would, and must become highly so, after it had time to ferment within the tropics" (p. 43).

Water was let in the hold at the Havana, between the 28th of April and 7th of May. After this, Capt. Biddle, "having been informed by some English officers, who arrived there after the *Macedonia*, that there was a standing order in the English service that water should not be let into their vessels in the port of the Havana" (p. 4), the practice was discontinued. Soon after this, the bilge-water was found to be very filthy and offensive, to such an extent, indeed, that when it was pumped out, in one instance, the captain "sent off from the ship as many of the men as could get into the ship's boats, and the rest were sent into the tops. There was a gelatinous substance of a very offensive character on the chain cable when hove in; and on taking out the starboard cable, a part of it was found to be wet, in consequence of a leakage from one of the berth-deck scuppers. The casing of this being removed, about two buckets full of very offensive filth was found (pp. 6, 7). It may be remarked that by the time the vessel arrived at Norfolk, a change had taken place in her condition; for it is stated by the master (John Robinson), who then attended the breaking out the riding and ground tier, that there was not more dirt in the hold than usual. About twelve casks of lime had been thrown into the hold before that operation was performed; then a quantity of water was let in, and when this was pumped out it was not found dirty. Mr. Robinson was in the hold after all the casks had been taken out, and did not find then more dirt than might have naturally been expected. The hold was not offensive before broken out, and even appeared to be sweet. Two of the limber-boards were removed, and the timbers appeared to be clean (p. 24). Purser Thornton likewise states he witnessed no unusual filth, and that the hold was not offensive (p. 26).

Be this, however, as it may, the sickness and mortality on board, according to Capt. Biddle, were greatest among the persons employed in the hold and among the carpenter's crew, who, by working the pumps, were most exposed to the offensive bilge-water discharged from the ship (p. 43); and by Dr. Chase, one of the surgeons, it is stated that the disease commenced near the pumps (p. 28). The boat crews were, on examination, found to have suffered less than the rest of the ship's company (p. 9). "It appears that the awning was constantly spread while in the Havana, and that the men were very little exposed to wet or to the sun, or to duty in boats, or to fatiguing duty on board, being excused from keeping watch at night" (p. 39).

From these various circumstances the inference is natural that the disease, which carried off one hundred and one individuals out of a complement (including officers) of three hundred and seventy-six, arose from the operation of causes located in the vessel itself. There was, as we have seen, no yellow fever at the time in the city or port of the Havana, and the same fact has come to my knowledge through other channels. Other vessels, at no great distance from the frigate, did not suffer from the disease, and neither officers nor men could have communicated with individuals already affected. Hence, it was impossible for the fever to have arisen from the introduction on board

of a contagious germ, or from morbid effluvia proceeding from the port, and wafted from a distance. The disease thus produced continued on board during the passage to St. Domingo, and therefore could not have depended on a cause existing in the harbour or city of the Havana, for in that event it would have ceased soon after the vessel put to sea. It increased at Port au Prince, and as the fever was not prevailing there at the time, its aggravation was not the result of external influences having their source at that place. It is to be remarked that the disease continued to prevail during the passage back to the Havana, as well as during the stay there, and did not cease before the arrival of the vessel at Norfolk, and the landing of the crew. To this must be added that the ship's hold was the receptacle, while at the Havana, of materials which, in other localities, have, under atmospheric and thermometric conditions of like nature, given origin to morbid effluvia of a most pestiferous character; that the disease first attacked and prevailed most severely among those most exposed to the effluvia from the hold; that the boat's crew, who were less exposed to those effluvia or to the contaminated atmosphere of the ship, were less affected by the fever than those who remained on board; and that the medical officers of the ship, Drs. Cadle and Chase, and Dr. Dayers, of the navy, whose opinion was asked by the court, and who was familiar with all the circumstances of the case, expressed the opinion that the fever originated from noxious effluvia generated in the ship's hold (pp. 4, 35, 50).

Whether the effluvia proceeded from sources of infection previously existing in the frigate, and carried in her from Boston to the Havana (innocuous and unperceived there, but efficient within the tropics); whether they were furnished by the water improperly and imprudently let into the hold at the Havana, and which, as it would seem, was impure; or whether, as is more probable, the poison was created by the operation of that water on materials already collected—mud, dirt, the timber of the ship, or the wood of the casks, &c.—matters not to the question more particularly before us. All we need look for is, that the cause was located in the vessel itself, followed it wherever it went, and never ceased to exercise its baneful influence, till, in a more northern climate and in a cooler season, the hold was emptied and purified.

Other causes were suggested. 1st. Sudden transition from a cold to a hot climate. 2d. Tropical heats acting upon constitutions unacclimatized. 3d. Predisposition to disease from debility, arising from previous disposition or disease—the crew having suffered much from catarrhal complaints during the passage. 4th. Dampness between decks. 5th. A want of suitable clothing, and neglect of personal cleanliness, as also a privation of those articles of diet which seamen are in the habit of purchasing of the purser. 6th. Sleeping on the decks, and exposure to the night air and dew, as also sleeping in a crowded state on the berth deck. 7th. Fatigue, arising from the frequent exercise at the large guns in the harbour of the Havana. 8th. Despondency, arising from an impression that the causes of sickness were on board the vessel, and the apparent inefficacy of medicine. 9th. Too long sojourn in the port of the Havana; on account of the greater degree of heat the men were

exposed to than they were accustomed to, or would have experienced at sea, as well as exposure, during that time, to the foul and deleterious air of the port. 10th. Neglect, during that time, of hoving up the ship broadside to the wind, so as to enable the latter (which blew pretty regularly from morning till sunset) to penetrate, cool, and purify all parts of the vessel.

To all who have some knowledge of yellow fever, and of the causes which usually give it origin, it need scarcely be stated that the above enumerated circumstances may have predisposed to, but could not have produced the disease. The limitation of the latter to the frigate, and the general nature of some of those influences, as well as the fact that their operation extended to the crews of all the other vessels in the harbour without producing the disease in one of them, sufficiently indicate the improbability of their being the efficient cause of the epidemic in question; while the others have never been known to occasion the fever, and are not of a kind to justify the idea of their doing so in any locality and under any circumstances.

Scarcely less important than the preceding is the history of the outbreak of the yellow fever on board of the U. S. brig *Enterprise*, in 1822. This case has been cited by contagionists, and supporters of the exotic origin of the disease, as one strongly illustrating the correctness of their views; while, by others, it has been more correctly regarded as showing the origin of the disease from sources of infection located in the vessel itself. The *Enterprise* arrived at the New York quarantine, on the 8th of July, from a cruise in the West Indies, via Charleston. Twenty-four days previous to her arrival, she had been three days off the Moro Castle, Havana, when she sailed for Charleston, somewhere between the 20th and 24th of June. She remained at Charleston eight days. The disease broke out in her the day she reached there, in the person of one of the lieutenants. He died on the 1st of July. The cases soon multiplied, so that by the time the vessel arrived at New York, they amounted to ten. On the next day they increased to thirteen, all of whom were transferred to the Marine Hospital, Staten Island. On the 11th, the number of cases had reached to twenty. It was then thought proper to bring all the men ashore, and to have the brig thoroughly whitewashed and cleansed, and her hold daily fumigated with nitrous oxide gas. Lime was slacked in her limbers, her iron ballast was whitewashed, and she was well ventilated by four windsails hoisted constantly in her hatchway.

Dr. Joseph Bayley, the health officer, in an official communication to the President of the Board of Health, says: "The crew were so intemperate during the few days that they remained on shore, that we were under the necessity of sending them on board. The brig had been purified in the interim. But it was soon evident that she was still an infected vessel, for in six days after the crew were sent on board, four men were taken sick with yellow fever, and, in the course of five days, seven more had the same disease, making one-fourth of all the men on board; and five of the eleven taken sick died. These persons must have been infected after their return to the brig, and subsequently to her purification, otherwise the disease would have been excited in them, as it was in the case of some of their shipmates, from

their irregular living, and exposure to the weather, by lying on the ground at night." * * * * *

"This fatal evidence of the cause of the disease still lurking in the *Enterprise*, induced us to have the crew brought on shore again, and recommence her purification, which was done by using two more casks of lime, by letting into her hold daily several feet of water, and keeping up windsails. Six men were left in charge of her, whose duty it was to pump out the water and trim the windsails," and they were directed not to sleep below in her hold. "On the 2d of August, twenty-five days after her arrival, and after repeated whitewashing, letting in water, and constant ventilation, one of the sailors obtained permission of a lieutenant to take his wife on board; this woman was taken sick with yellow fever on the 9th of August, and she died in the Marine Hospital on the 18th of that month."¹

In this case, every unbiassed mind must recognize the operation of a febrile poison originating in and restricted to the vessel. The disease could not have been derived from Charleston, for that city appears, from all the accounts to which I have had access, to have been healthy then, and to have remained so during the whole season. Besides, had it been otherwise, the disease, as we have seen, broke out the day after the arrival of the vessel, certainly too soon to justify the idea that the officer first attacked could have taken the disease there. Nothing is said, in the accounts of this visitation, of the sanitary condition of the Havana, at the time the *Enterprise* was off Moro Castle, and I have looked in vain in other publications for some definite information on the subject. Nevertheless, we are warranted in the conclusion that the port and city were then free from the fever in an epidemic or sporadic form, inasmuch as the *Macedonia*, which there lost so many men the same year, and left on the 24th of July, was the only vessel that suffered—a circumstance which would not have occurred had the port been sickly some weeks before. Again, the *Enterprise* did not enter the port, but remained off the Moro Castle, where vessels usually ride with impunity. Neither is anything said about boat communication with the port, and about the officers and others who first sickened being those who *did* communicate, supposing such communication to have taken place.

The cause continued in full operation after all the crew, sick and well, had been removed, and attacked individuals who ventured on board, precisely as occurs in infected localities on shore, after all the inhabitants have been removed, and that, too, after the process of disinfection had been gone through to a sufficient extent to destroy any amount of contagious germs or external effluvia that could have been introduced in the vessel. It is doubtful whether the contagion of smallpox would have proved so obstinate.

Other cases of evident infection generated on board of American vessels, and spreading among the officers, crew, or passengers, or on land in the

¹ History of the Proceedings of the Board of Health of the City of New York, in the Summer and Fall of 1822, p. 142; see also pp. 15, 123, of same work, and Bayley's Report of the Epidemics of 1822, N. Y. Med. and Phys. Journ., i. 426.

vicinity, are on record, and are of a nature which admits of no doubt. A few more references will be sufficient.

In 1799, the sloop *Mary* was sent into Philadelphia, as a prize to the ship of war *Ganges*. She was not from a sickly port, and at the period of her arrival there was no one sick on board. As soon as her cargo was removed, her decks were washed, and the hatches and ports all shut down. In this way she remained three weeks, the weather being at the time very hot and dry. The hold and the interstices between the timbers contained a quantity of vegetable matter (coffee), which—being mixed with the bilge-water and that which had fallen from the deck at the time of washing, aided by the high temperature and close confinement of the air—fermented, and gave rise to the development of highly offensive effluvia.¹ “The noxious effluvia,” says Dr. Caldwell, “that were generated in abundance, having no vent to escape and be dissipated in the atmosphere, mingled with the air in the vessel’s hold, and produced in it an extreme degree of vitiation. A smell resembling that of common bilge-water, but more offensive, became troublesome to those engaged about the wharf, and was at length traced to the place where the *Mary* lay. She was soon suspected as the source of this nuisance. Her ports and hatches were accordingly thrown open, when the foul air rushed out in torrents, and spread through the neighbourhood a suffocating stench.”²

Several persons exposed to these effluvia were a few days after seized with decided symptoms of yellow fever.

The following case, though happening in an English vessel, may, from the development of the disease taking place in one of our ports, find a place here. The British three-decked ship *Hibbert*, six hundred tons, arrived at New York in July, 1803, from Portsmouth (England), in ballast, and was ordered to ship a cargo of pine timber for the Bay of Honduras. The *Hibbert* had been used as a transport for soldiers between England, Nova Scotia, and the Bahamas. “About the 20th of July, while the people were employed in clearing away the rubbish and stuffs between decks and below, it was observed that she contained a quantity of sand ballast, which had remained there a number of years without being changed. It was, however, not taken out nor shifted.” On scraping and overhauling the lower and middle decks, these and the timbers were found to be overspread with all manner of corrupt and excrementitious materials. The stench occasioned by the collection and removal of this trash and offal was intolerable. The men were obliged to run to the port-holes and hatches for fresh air. This filth was thrown overboard, beside the wharf, into the salt water; but in performing this the whole ship became offensive. Several of the men employed, both among the sailors and hired labourers, were taken ill. “One of the seamen, after two days’ illness, had symptoms of hemorrhage and black vomit, and was sent to the Marine Hospital, where he died.” “The stevedore who loaded the ship was poisoned by the effluvium, and died, after hemorrhage and black vomit, early on the fourth day.”

The heat at the time was very great, being from eighty-five to ninety-three

¹ Caldwell’s Med. and Phys. Memoirs, pp. 93, 94. 1800.

² Ibid., p. 95.

degrees in the shade, "during the shining of the sun." It should be added that the city was then perfectly free from yellow fever, and that the disease produced in the *Hibbert* was not communicated by the sick. This vessel sailed for Honduras, and, as was anticipated, continued to be in an unhealthy condition. Several of her people died on the passage. After arriving at her place of destination, she was unloaded about two miles from the settlement of Honduras. While the work was going on, a number more of the persons employed on board fell sick and died of yellow fever. The disease was limited to them, and did not spread.¹

If we now turn to the history of the yellow fever in English vessels at sea, or in British or foreign ports, we shall find many striking instances of the kind under consideration. Of these outbreaks, not the least interesting and conclusive are those which occurred in the year 1821, on board the armed transport *Dasher*, the frigate *Pyramus*, and an unarmed transport. They are recorded in an official report by Dr. Hartle.

The *Dasher* left Barbadoes for Tobago on the 26th of August, but, in consequence of severe gales, was obliged to go to St. Lucia. Proceeding thence to Tobago, she there received on board a company of the ninth regiment, and sailed for Grenada, in order that the men might avoid the endemic fever of the former place. This company, while on board, was perfectly healthy. On their landing at Grenada, the men were immediately placed in quarantine, and remained so for the space of fourteen days, during which period not an individual sickened. "The *Dasher*, after landing this company at Grenada, proceeded hither (Antigua); but, a few days before she reached this port, yellow fever made its appearance among her crew, and previous to her arrival six men had been attacked, two of whom died." The crew was landed, and the disease ceased among them. Blacks were employed to remove the ballast and clear the hull. At the urgent solicitation of Dr. Hartle, the limber-boards were taken up.

"Here, to the astonishment of every one, lay the mischief. On taking up these boards, the noxious effluvia surpassed anything that I had before experienced, and it was with difficulty that the blacks, who were accustomed to this work, could remain. The ship-carpenter, who had been constantly accustomed to work in the dock-yard, and on many such occasions, assured me that he had not before experienced so putrid a smell from any ship's hold. Between the timbers there was a collection of carpenters' shavings, &c., in great quantities. These had so completely choked up the limber-holes that the water could not pass to the well of the pump, and lay stagnant. The vegetable matter was therefore in a state of decomposition, and this, acted on by high atmospheric temperature, became neither more nor less than a marsh in the centre of the ship. That the fever," continues Dr. Hartle, "was generated on board, by noxious effluvia received into crowded and badly-ventilated berths, is, I think, fully proved; for the moment the crew and marines were removed from the sphere of this hidden enemy, the disease ceased."

¹ New York Medical Repository, vii. 87, and viii. 71, 72.

Nothing like the most distant appearance of contagion could be traced, for none but those residing on board the ship, or exposed to the effluvium from her hold, prior to her expurgation, suffered by the fever.¹

The *Pyramus* left English Harbour perfectly healthy on the 19th of October, for St. Kitts, where she remained until the 28th, when she sailed for French Harbour. A day or two prior to her arrival at the latter place, fever of a most alarming type made its appearance among the officers and crew. The sick were landed, and the ship sailed for Barbadoes; but the disease continued to prevail. A medical board was assembled, in order to investigate the probable cause of the sickness. In the first part of their report, the board attributed the cause, in some measure, to the coal-tar with which she was injected, for they particularly observed the offensive effluvium arising from that substance mixing in the hold with the bilge-water. In the second paragraph, they remark that the ship lay thirty-four days, at different times, in English Harbour; and, in the next, consider, with respect to the predisposing cause of the disease, that something must be attributed to the stay of the vessel in that harbour. The vessel, at the suggestion of the board, sailed from Barbadoes, and cruised as far as 28° north; but finding this avail nothing, and that the disease became more alarming, the captain hastened to English Harbour, where he arrived on the 3d of January, 1822. The crew was there landed, the ship emptied of her stores, shot, tanks, ballast, &c. On the opening of her hold, the effluvia which issued surpassed anything Dr. Hartle had ever witnessed, and affected every one exposed to its influence.

The quantity of filth which was taken out was sufficient to fill four large mud-boats, consisting of shavings mixed with coal-tar and the water which, in consequence of the choking of the pumps, had accumulated under the limber-planks. All the cases which occurred during the process of expurgation were easily traced to exposure to this bog; and Dr. Hartle very justly refuses to refer the disease to the influence of English Harbour, inasmuch as other ships of the squadron that lay much longer there escaped the infection. It may not be amiss to remark that the sick of this ship were landed and placed in the capstern of Antigua dock-yard on the 15th of January; that between that day and "the 30th only eighteen cases occurred; but that on the 31st six fresh attacks were added to the list, and the disease again appeared with increased violence and malignity." Dr. Hartle adds: "This sudden reappearance and violence of the disease induced me to believe that the people had some communication with the ship, which was then undergoing a general expurgation. This, with a little trouble, I ascertained to be the case." Changes were made in the distribution of the convalescents and the rest of the crew, and the disease was put a stop to completely. The crew re-embarked on the 14th of March, in excellent health, and remained so.²

¹ Facts and Observations in refutation of Sir G. Blane's Doctrines as to the Contagious Nature of Yellow Fever, by A. Musgrave, M. D., Appendix B—Med.-Chir. Rev. and Journ., iv. 994.

² Musgrave, loc. cit., pp. 995–998. See also Burnett's Report on Fever of Ascension, p. 34.

Dr. Robert Armstrong ridicules the whole account of this severe visitation, denies the

As regards the transport above alluded to, the disease broke out on her passage from Barbadoes to English Harbour. The sick and all the troops on board were landed, and the vessel, after a partial cleansing, proceeded to St. Kitts; whence, having landed the stores and baggage, she returned to English Harbour. She then underwent a general purification, when a portion of the troops re-embarked and sailed for Grenada, where they arrived in good health, the fever not having reappeared on board.¹

"It is a pleasing reflection," adds Dr. Hartle, "and a source of great gratification to me, that, notwithstanding 147 cases of yellow fever, as distressing and malignant as any I before witnessed, have been by the three vessels imported into this island since September, 1821, we have not had a single instance of any individual but those directly exposed to the local causes being attacked."²

Dr. J. H. Dickson, in a report to the Transport Board, alludes to the generation of the yellow fever in several vessels—the Blonde, Gloire, Star, Wanderer, and particularly the Dart. The disease broke out on board of the latter in April, 1807, and was satisfactorily traced to effluvia exhaling from offensive matter collected at the bottom of the water-tanks.

"So many people," Dr. D. remarks, "were taken ill after going on board this vessel, lying guardship at Barbadoes, that it was difficult to account for it, except on the principle of contagion, until the peculiar construction of the ship—viz: her being divided into compartments below, so as to prevent the circulation of air, and the stowage of the water in *bulk*—was adverted to; and on examining the large tanks or cisterns, their bottoms were found covered with an offensive slimy mud or deposition."

On the removal of some of these causes—the knocking down of the bulk-heads and the cleansing out of the cisterns, the fever was put a stop to. In the Thetis, in 1809, "the fever did not appear until the hold was broken up, when about a dozen of men so employed were taken sick, and four out of five carpenters who lifted the limber-boards died."³

possibility of so much filth having accumulated in the ship—or, if it had so accumulated, of its having produced the disease—and adds: "The following year, when serving in the West Indies, I met with two officers who belonged to the Pyramus at the time the fever prevailed; and, on making inquiries as to the truth of the various reports in circulation respecting her, was informed by one, and his statement was corroborated by the other, that a certain domestic kept an open grog-shop, and that drunkenness and irregularities had more to do with the fever than a little bilge-water or a few dead rats in the hold. A staff assistant-surgeon, whom I met at Barbadoes, informed me that he had visited the Pyramus, but added, with a certain sarcastic sneer, that his vision was then defective, and that he could not see so far as other people" (op. cit., p. 60). When the reader is informed that the disease which prevailed on board of this ill-fated vessel was the true yellow fever, that the number of cases amounted to one hundred and two, and that of these thirty died, he will feel no hesitation in admitting that drunkenness has seldom before or since produced such an extraordinary effect. The case should be recommended to the attention of Father Mathew. Other instances, similar to that of the Pyramus, fare no better at the hands of Dr. Armstrong, and are disposed of much in the same way.

¹ Musgrave, loc. cit., p. 998.

² Ibid., p. 999.

³ Dickson's Report in Bancroft's Sequel, pp. 208-9; Dickson on Yellow Fever, Edinb. Journ., xiii. 36.

In his account of the fever which prevailed on board the *Nyaden*, the surgeon observes: "In clearing the after-hold, which was very offensive, several men immediately took the fever, some of whom died. This effect," continues Dr. Dickson, "is the more evident when contrasted with the healthiness of some vessels *close* to them, which were either accustomed to the climate or differently employed." Well could Dr. D., with these facts before him, remark: "The power of *impure* but strictly *local* effluvia in producing yellow fever on board ships also, as well as on shore, is unquestionable."¹

Dr. Gillespie, whose work on the Diseases of the Leeward Island Station² contains much useful information on the subject of the yellow fever, relates several facts of an interesting character relative to the matter before us. In speaking of the events on board the ships cruising in the West Indies during the year 1795-6, he remarks that at that time all the vessels of the squadron had their crews in good health, except the frigate *La Pique*, which had been captured and carried into English Harbour, in Antigua, to be refitted.

"In the beginning of November that frigate arrived at Martinico, and the remains of the crew had acquired a good state of health, though they had the sallow complexion which men generally have when confined in impure air. November the 12th, she sailed for Barbadoes, having received a draft of seventy-five men from the *Ganges*; from being embarrassed with a convoy and from unsettled southerly weather, the passage was long; two hundred French negroes were taken out of a vessel which was in danger of foundering, and were kept on board the *Pique* until her arrival at Barbadoes. They were confined some time in the hold. Such a mixture of men, strangers to each other, has been often found to occasion sickness in ships, and together with other causes fatally operated here, before the arrival of the ship at Barbadoes. A malignant yellow fever had made its appearance, and continued to rage with destructive violence amongst the crew of the *Pique*, and which is supposed to have proved fatal to 150 men. Out of the *Ganges* draft twenty-eight alone are said to have survived the epidemic. The negroes, it is probable, were saved by being disembarked on the arrival of the *Pique* at Barbadoes. This," continues Dr. Gillespie, "is a melancholy instance of the generation of a fatal epidemic on board of a ship, at a time when the inhabitants of Barbadoes and the crews of the other ships in company remained free from any such disease" (p. 53).

To this we may add that Dr. Gillespie being, as every page of his volume attests, fully competent to discriminate between true yellow fever and other forms of febrile disease observed on shipboard, his statement of the occurrence in the ill-fated vessel will of itself be sufficient to scatter to the winds all the doubts entertained by fanatic contagionists as to the possibility of that generation.

Scarcely less satisfactory, in illustrating the development of the yellow

¹ Bancroft, *id.*, p. 209.

² Observations on the Diseases which prevailed on board a part of his Majesty's Squadron on the Leeward Island Station, between November, 1794, and April, 1796. London, 1800.

fever on shipboard, is the following case, communicated by Dr. Crawford in a letter to Dr. Trotter, and inserted by the latter in his *Medica Nautica*.¹ After remarking that previous to the British squadron appearing before Port au Prince, in May, 1794, none of the ships were afflicted with contagious diseases, and that though deaths had occurred, they were caused by the common remittents of the country, Dr. Crawford proceeds:—

“On the capture of the port, June 4, 1794, about forty sail of merchantmen were found in the harbour, most of them large vessels, the cargoes of which were coffee, cotton, sugar, and indigo, that had been stowed in them from one year to three, in which time many of them never had their holds opened from the disturbances that prevailed among themselves. On board of them men were sent from the whole squadron to fit them for the passage to Jamaica, which, from the state they were in, was not to be soon done. The weather was excessively warm, and some days elapsed without a breath of wind. The very first day the people proceeded to work, many were taken ill, and sent on board their respective ships with fever; several with strong convulsions that were succeeded by fever, and one or two died. I was sent to several to remove the sick, where I found the stench from the damaged coffee and sugar almost insupportable; it wanted no great degree of penetration to prognosticate the consequences in two, where there was a quantity of sugar, &c., melted in the hold. I saw the mixture in an actual state of effervescence, and bubbling up from every part. From these ships, I can vouch, the disease was first introduced to the *Penelope*. I most truly think, that the primary cause of this horrid disease originated from these ships. One thing is most certain, that, if it did not originate there, it was much increased in virulence by our connection with them. After they were fitted out, on their passage to Jamaica, they lost more than three to one in comparison with the men of war, although this passage was not more than seven days. In the *Horizon*, on board of which was Lieutenant Gaeren and Mr. Stupart, of the *Europa*, the crew had been replaced three times, and from illness got in her died thirty men. Seven out of ten died on the passage to Jamaica; another of them was picked up at sea by a Guineaman, with every soul dead on board; even a number of negroes, who afterwards cleared them, died from fevers caught on board of them.”

Dr. Chisholm is probably among the very last authors in whose writings we should expect to find statements tending to show the local origin of yellow fever in the West Indies, whatever might be the case in Africa. And yet he has furnished us with a strong instance of the kind in his account of the supposed introduction of the disease at Demerara, in August, 1800. The account was written by him at Demerara, where he then resided, and addressed in a letter to Dr. Davidson, dated August 10, of that year. It will be found entire in the *New York Med. Repository*, vol. v. p. 229, and is quoted by Dr. Baneroff, at p. 746 of his large work on the yellow fever.

“A fever of a most alarming nature,” Dr. Chisholm says, “has most fatally

¹ Vol. ii. pp. 97–8.

prevailed since the beginning of July. I have visited a few of the sick at the request of Drs. Dunkin and Lloyd in town, and of Dr. Ord on the coast; and I have no hesitation in pronouncing it a fever of infection. Its features are almost without exception precisely those of the malignant pestilential fever of Grenada of 1793 and 1794. It is fully as fatal, as rapid, and as insidious. Its origin, as far as it has been ascertained by the gentlemen I have mentioned, seems to be similar.

“A ship arrived about the beginning of July or end of June from Liverpool, after touching at Surinam. The filth on board, occasioned by a cargo of horses, and the extreme neglect of the officers and crew, was such as beggars description. Infection was the consequence. Her officers were the first sufferers; every man died. All who went on board were attacked, within thirty hours after, with a fever of infection. What a lesson this to masters of vessels! How clearly it exhibits the necessity of exertion on their part to maintain cleanliness on board their ships! And how evidently does it display their responsibility to the public for the consequences of misconduct!”

This, if all really happened as stated—and on this subject the reader must judge for himself—is as clear a case of the generation of the yellow fever poison on shipboard as could be desired. It is, indeed, admitted as such by Dr. Chisholm. Whatever he may have thought of the manner in which the disease spread, after it had fairly been established in the place, nothing is said or hinted at calculated to lead to the supposition, that the vessel had received the infection at Surinam; and it cannot be supposed that any one will entertain the idea of its having been imported from Liverpool. If, therefore, the fever broke out in the way mentioned, it must have arisen from causes located in the vessel itself. As to its contagious nature, although Dr. Chisholm, as all might have foreseen who are familiar with his decided opinion on the subject, did not hesitate to pronounce in the affirmative, it would seem that Drs. Ord and Durkin, to whom, as we have seen, he refers, and who probably had more experience in the matter than himself, as he was not then in the practice of his profession, entertained a different sentiment; for we are informed by Dr. Bancroft (p. 747), that both these physicians stated to him a few years after, that they had never seen any fever in Demerara, or on that coast, which they believed to be contagious.¹

¹ “Ships containing wine in their holds in a state of decomposition, are generally extremely sickly, and the character of the prevalent disease is that of yellow remittent fever. Several instances of this took place in Fort Royal Bay, in the years 1797, 1798; and the situation of the ships in the open bay, far from the influence of marsh effluvia, precludes a suspicion of the fever proceeding from that cause.” “The ship *Nancy*, Capt. Needs, from Fayal, with a cargo of wine for the army, arrived at Fort Royal, Martinico, in the month of October, 1798. She met with a gale of wind at sea on the 17th September, and several of the casks, from the motion of the ship, became leaky. The captain was actually taken sick at sea, and died with every symptom of the highest grade of yellow remittent fever. The mate and several of the crew were attacked with the same complaint; they recovered; but a mate, shipped at Fort Royal, fell ill on board and died. The ship lay out in the open bay; no vessel near her was sickly; and she herself became very healthy after the cargo was landed.”—*Chisholm*, vol. i. p. 379.

The case of the *Regalia*, mentioned by Dr. Ferguson,¹ and that of the *Rattlesnake*, described by Dr. Wilson,² afford most satisfactory proofs of the local origin of the yellow fever on shipboard, either from a particular condition of the vessels themselves, or from their ballast or other contents.

The first of these vessels, the *Regalia*, sailed with black recruits from the coast of Africa, for the West Indies, in 1815. The ship was good, and her crew healthy during the time she was employed on the African coast, until she took on board, a very short time before the blacks were embarked, a very large quantity of green wood, cut down and stowed away the same day. The fever prevailed with great malignity among her crew, and continued to do so up to the time of her arrival at Barbadoes; attacking all on board excepting the blacks, who were exempt from the fever when they embarked, and continued so to the last. From the healthy condition of the crew during their stay on the coast, the absence of fever among the black troops embarked, we are naturally led to look to other agencies than contagion to account for the origin of the disease. These are easily found in the quantity of green wood stowed on the African coast, and the condition of the ballast. The quantity of wood laid in appears to have been considerable, for, as Dr. Ferguson remarks: "After she had been several weeks in the West Indies, there were still as many tons left as, in the master's opinion, would serve for a voyage to Europe. The ballast, too, had never been changed or shifted from the time the

As Dr. Chisholm denominates the disease thus produced yellow remittent fever, and regards it as essentially different from what he considers to be the true malignant yellow fever—the former being of local origin; produced by malarial exhalations, and destitute of contagious properties, and the latter arising from morbid effluvia emanating from human bodies, originally imported from the western coast of Africa, and transmitted from individual to individual, or through means of fomites—it may be urged that the instance of infection on board of the ship *Nancy* cannot find a suitable application in pages, the object of which is to show the origin of yellow fever in ships from the evolution of poisonous effluvia; for what might be true of the one might not necessarily be so of the other. But if, while bearing in mind the ill success of Dr. C., in his endeavours to prove the introduction of malignant yellow fever into Grenada by the ship *Hankey*, in 1793, and hence into other places, as well as the contagious character of the disease, we turn to the description of his yellow remittent fever, contained in the first volume of his work, and of the epidemic recorded by him in Duncan's *Medical Commentaries* for 1793, and compare it with that he gives of the fever of Grenada, and those we possess of the yellow fever of this country, Europe, and tropical climates generally, we shall find just cause for suspecting that the difference between them is not as great as that distinguished physician so confidently asserted it to be; and that his yellow remittent fever, and his malignant pestilential fever, constituted at best but two forms of one and the same disease. I may add, before closing this note, that the *Nancy* is not the only vessel in which Dr. Chisholm, contagionist as he is, admits that the true yellow fever originated; for he states, as his opinion, that the disease which he thinks was introduced into Grenada in 1793 by the ship *Hankey*, "was generated on board" of that vessel "during her disastrous detention at Bulama."—Vol. i. p. 318.

¹ An Inquiry into the Origin and Nature of the Yellow Fever, as it has lately appeared in the West Indies, &c., *Medico-Chir. Trans.*, viii. 108.

² *Memoirs of West Indian Fever, &c., commonly called Yellow Fever*, p. 85. London, 1827.

vessel left England, nor for any discoverable time before. It was what is called shingle ballast, small stones, with a considerable mixture of mud and other impurities." "And it had been much spoiled by the leakage from the water-casks. The ship, in respect to leakage, was far from being a dry ship, and from that circumstance might, with better ballast (of iron or large stones), have proved a very healthy one; but the absorption of sea-water among foul ballast and green wood, could scarcely fail to prove unwholesome."

That the cause of the fever resided in the hold of the ship, is proved by the fact that the cleaning of that part proved particularly injurious to those exposed; and that while the fumigation of the ship proved of no avail in purifying it, the fever ceased when she was completely cleaned, and, with her hatchways closed, her whole hold had been exposed to the concentrated heat of many stoves; and if further proof that contagion had no part in the origin of the disease is required, it will be found in the fact, to which attention has already been invited, that though the *Regalia* communicated freely with the seaports of Barbadoes and other islands, landing the sick or dying subjects of that disease among the inhabitants, or at the hospitals, the infection was not communicated anywhere; and that, after being purified, she sailed from Guadaloupe to Europe, crowded with prisoners, having on board a case of fever which terminated fatally, without communicating the disease to those around.

The *Rattlesnake* was lying at Port Royal when, on the 8th of August, 1824, the yellow fever broke out in her. For some months previous to that date, there had been very few cases of fever at Port Royal, either in the squadron or on shore. Two came to the knowledge of Dr. Wilson, during the time the *Rattlesnake* was in harbour, but with neither of them had the individual first attacked the power of communicating. From the 8th of August till the 10th of September, there were occasional cases—in all, nineteen. But after the last date, the disease assumed an epidemic appearance, passing rapidly through the ship's company, and affecting seventy persons within the month. From the 10th of October, it gradually declined, and finally disappeared about the end of the following January. On the 27th of August, 1824, the ship went out of harbour on a cruise to leeward, and returned to Port Royal on the 27th of September with forty men ill of fever. Twenty-seven cases were sent to the hospital the same day, in different stages of progress, and for some time afterwards, three, four, or five were sent thither daily. As has been noticed in innumerable instances of the kind, the patients landed did not communicate the disease to their attendants, or to the other sick in the hospital; and when, bearing this in mind, we recollect that the fever began by occasional cases, the subjects of which had not been near people labouring under it out of the ship, and proceeded in that manner for about a month, at the end of which it assumed the character of a severe epidemic, we may conclude that it did not originate in, and was not propagated by, contagion. "When," as Dr. Wilson remarks, "we were sending so many men to the hospital daily from the *Rattlesnake*, almost every fresh man whom

we received was attacked by the fever; and it might thence have been inferred that the strangers received the disease from the men among whom they lived; but we had at the same time proof that those men could not communicate it to another when removed from the ship. We had, therefore, 'the bane and the antidote both before us,' and conviction arose from the whole, that the disease at that period could not possess contagious properties; for it were futile to say that it possessed those properties in the ship, but lost them the instant its subjects were removed a hundred yards from its source."

The conclusion, therefore, is natural, that the crew did not take the disease from each other, or from the harbour; but that, like the strangers who came aboard, they received the infection from a cause inherent to the vessel itself. To this we shall the more readily give our assent, if we take into consideration the following account of the limited range of its prevalence on board, and of its mode of progression:—

"In the *Rattlesnake*, the fever was first manifested near the main hatchway; the marines, and the midshipmen of one berth, suffered its earliest and most severe effects. Afterwards, it proceeded forward rapidly, but pretty regularly, till it had affected almost the entire ship's company; but it did not go beyond the steerage in the opposite direction, no one being attacked in the gun-room except the purser, and I have good reason to conclude that in his case it was derived from the shore." "But its local origin and limited range of action were most strikingly exemplified in the berths of the midshipmen, and other officers of that class. They were placed exactly opposite to each other, with the pumps at equal distances between them. Only one gentleman was affected in the starboard berth, while every member of the larboard berth was laid up nearly at the same time. The hatch of the pump-well is opposite to, and within three feet of the larboard berth. The members of that berth were generally the junior officers of their class, and were, therefore, it may be said, most susceptible of the disease; but when it is remembered how extensively it prevailed, and how indiscriminately it attacked persons of all ages and temperaments among the ship's company, after every allowance is made for the greater susceptibility of these young gentlemen, the exemption of the others, senior only by a few years, is too striking and complete to be accounted for by their former service, or by any accidents which can reasonably be supposed to have affected them. It can fairly be attributed only to their not having been exposed to the cause of the disease with the same concentration of power, or permanence of operation, as the others; and here the contiguity of the pump-well hatch to the larboard berth cannot be overlooked." (*Wilson*, pp. 159, 160.) That the cause was not the product of contagious germs derived from the shore, is farther inferred from the fact that other vessels lying close by were not affected. The *Primrose*, for example, was commissioned about a month after the *Rattlesnake*, and arrived at Port Royal about a month later, viz: about the end of July. "She was employed, I may say, exactly as the *Rattlesnake* was. After a very short cruise, she lay six or seven weeks, including September, within a few yards of where the *Rattlesnake* lay, and was employed in the same man-

ner. She then followed the Rattlesnake to Chagres and the coast of the Spanish Main, where she encountered similar weather, and remained about six weeks; she then returned to Port Royal healthy, and continued so, and that while the Rattlesnake was converted into a complete hospital from the epidemic prevalence of West Indian fever. On the other hand, in 1825, while the Lively, Pylades, and Ferret were half unmanned, the Rattlesnake lay close to them in Port Royal harbour, for the space of eight weeks, and did not lose a man." (*Ib.*, p. 140.)

The history of the outbreak and prevalence of the yellow fever on board of the steamer *Eclair*, in 1845—the supposed cause of the introduction of the disease into Boa Vista, has often been the subject of discussion, and must next engage our attention.

This vessel sailed from England for the west coast of Africa in November, 1844. After reaching Ascension, and being properly fitted, she proceeded to the coast, and was employed in watching for slavers off Sherbro and Scabar, from December, 1844, to the 8th of July, 1845. Up to March 2 of that year the crew continued in health. After that the boats were sent up the Sherbro and other Rivers, exposed to all the vicissitudes of the weather and to malarial effluvia; and several times the men slept ashore or in the boats; the vessel, in the meanwhile, remaining at anchor from three to six miles off the coast. From the 3d to the 10th of June, there were seventeen cases of fever, and ten deaths, all of the latter occurring in men who had been employed in the boats. On the 4th of July the *Eclair* arrived at Sierra Leone, with improving health; the last remaining cases advancing towards convalescence. Her berth was not particularly unfavourable. The men had limited leave, and several of them slept on shore. They were also employed in clearing out the *Albert*, another steamer, which, like the *Eclair*, had formed part of the Niger expedition, and which had remained untouched since.

On the 19th of July a case of fever was reported; and four days after the vessel left Sierra Leone, having the *Albert* in tow, and anchored off the coast till the 9th of August. During this time there were fifteen cases and six deaths, the latter being preceded by unequivocal black vomit. On the 9th of August, the *Eclair* arrived at Gambia, and left on the 15th, reaching Gorée on the 16th; and, being refused pratique, she proceeded to Boa Vista, where she arrived on the 21st, with five fever cases on board. The number of these now increased so rapidly, that on the 31st the crew was landed on a small island two miles from the capital (Porto del Re), the sick being placed in the rooms of a dilapidated fort; and the well in tents pitched along the walls, or in apartments separate from the sick.

Here, however, the disease continued to extend, and the mortality, far from diminishing, became more alarming. In consequence of this, the crew was reshipped on the 13th of September, and the vessel steamed for England, having been previously cleaned, and, as it was supposed, purified. But the fever, which from the 9th to the 13th had appeared to assume a less violent character, acquired additional malignancy as soon as the vessel reached the open sea. After touching at Madeira, on the 20th, where she was refused

pratique, she arrived at the Mother Bank on the 28th. During the passage from Boa Vista, forty-one cases occurred; out of these, twelve ended fatally. After the vessel had reached the Mother Bank, nine fresh cases and five deaths—all traced to the vessel—were added to the list, including the pilot, who had come on board but a few days before, and a medical officer, Dr. Rodgers, who had been sent to attend the sick.

The question of the introduction of the fever into Boa Vista, which has given rise to an animated controversy, is foreign to the object of the present inquiry, and must, therefore, be passed by unnoticed.

The disease from which the men suffered while on the coast of Africa was undoubtedly the common remittent fever of the country. All the cases, as we have seen, with the exception of two, were supposed to have been traced to exposure in boats. These two had not left the ship. Dr. Bryson¹ is of opinion that they may have derived the disease from the emanations from the shore, to which, notwithstanding the distance, they must have been more or less exposed. Dr. King, in his report, has expressed his doubts on the subject, remarking that it is not clear that the remaining seven of the seventeen who were attacked, had been employed in the boats, and inclining to the belief that the cases were due to causes located in the vessel. But however this may be, and whatever may have been the nature of the disease thus produced, and which had ceased before the vessel reached Sierra Leone, there can be no difficulty in deciding that the fever which prevailed on board *after* the vessel left that place, bore the characteristic marks of true yellow, and not of common African remittent fever. Whether, with Dr. McWilliams, we are to view the disease as having been originally of the latter kind, and as having changed so much for the worse during the passage from Sierra Leone to the Gambia and Boa Vista, and especially while the crew were at the fort, as to assume the character of yellow fever, and acquire contagious properties, or whether, with Sir W. Pym, we are to conclude there were two distinct fevers on board—marsh fever, in April, May, and June, and the true Bulam, or yellow fever, from the 23d of July (the day she left Sierra Leone) to the time of her arrival in England—are questions upon which it is unnecessary to dwell here. Certain it is that the fever could not have been the result of imported contagion. At the time the *Eclair* lay at Sierra Leone, up to the day of her leaving for the Gambia, there existed no epidemic condition of the atmosphere at that place; and, although the rainy season had commenced, there prevailed no fever from which that of the ship could have been derived. On this we have the testimony of Dr. King, who subsequently learned, when he visited the place, that there had not been anything unusual at that time, either in the nature or amount of disease in the settlement. “It is not hinted that the sailors were exposed to infections from persons actually labouring under the disease; indeed, it is not certain there was a single case of sporadic yellow fever previous to their arrival. I am at a loss, therefore, to

¹ Report on the Climate and Principal Diseases of the African Station, p. 184.

understand how its origin can be explained on the principles of contagion" (p. 11).

The cases which occurred after the departure of the vessel, could not, therefore, have been carried from the shore, and have served to disseminate the disease afterwards. Nor is it less obvious that the absence of an epidemic at Sierra Leone forbids the supposition of the vessel having carried thence a tainted atmosphere capable of affecting the crew during the rest of her melancholy history, or of the crew having imbibed a quantity of poison, which, after remaining dormant in their systems, broke out at different times, up to their arrival in England. And yet, as we have seen, from the day of the *Eclair* leaving that port, to the 9th of August, fifteen cases occurred on board; and from the 19th of July, when the first case presented itself, to the 31st of August, when the crew were landed at Boa Vista, forty-four cases and sixteen deaths were reported. These cases must have been derived from causes existing on board; for no idea was entertained, up to that period at least, that the disease was propagated by contagion; and it is admitted, by Dr. McWilliams himself, that it did not manifest such property prior to the arrival of the vessel at Boa Vista. Even admitting that the crew and officers could have imbibed the poison at Sierra Leone, the disease by which some of them were attacked during the close of the passage, and especially on their arrival at Boa Vista, could not have been the result of such an exposure, inasmuch as the time which elapsed between that period and the moment of the attack, was too long to explain the occurrence on the principle of incubation.

"Without assuming at present," as Dr. King justly remarks, "that there were any just grounds for believing that the cause or causes of the fever had a local origin in the ship it may be inferred that the idea was entertained by the superior officers, or they would never have attempted to clean the holds at Sierra Leone in the rainy season, and within one month have again commenced the same laborious work at Boa Vista; and it is equally improbable (but from some apprehensions of the kind) that they would have removed the men from the ship, and lodged them, at that hot season, in such a miserable place as the fort at Boa Vista" (p. 11).

When we take into consideration the continuance and increase of the fever on board—when we learn that after the crew—the sick as the well, had been landed at Boa Vista, a lieutenant, the paymaster, the purser, and the clerk of another vessel, the *Growler*, were ordered, on the 7th and 9th of September, to survey the infected steamer, and that of these officers, the lieutenant, purser, and clerk were severely attacked—the second when actually on board, though there was nobody there to communicate the disease;¹ when, besides, we find that the pilot, who came on board in England, took

¹ Burnett, *Official Correspond.*, p. 74, and *Edin. Med. and Surg. Journ.*, Oct. 1847, p. 495.

As there had been occasional cases of fever in the *Growler* (Steward, in *Official Correspond.*, p. 89, and *Edin. Med. and Surg. Journ.*, Oct. 1847, p. 509), those adverted to may not have really originated in the *Eclair*, though it would be strange that, while there was no disease in the *Growler*, three out of four officers who had visited the other vessel should be attacked, if the cause did not reside in the latter.

the fever, though he did not communicate with the sick, had brought his bedding, and had not slept below; that some men who were employed to clear the hold in England, after the crew had been transferred elsewhere, were attacked with the disease in a mild form; and, finally, that the sick, when removed, did not communicate the fever to any of their attendants, we may safely conclude that the latter arose from the foul state of the hold. Were it necessary to adduce additional reasons for arriving at this conclusion, we could find them in facts furnished by the subsequent history of the vessel, and which show that, notwithstanding all the efforts that had been made at Sierra Leone, Boa Vista, and England, to cleanse and purify her, she long continued in a foul state, and was again, in consequence, the seat of malignant fever. Weeks or months after the people had left the ship, and when she was recommissioned, a large collection of mud, fully three inches in depth, was found on removing the engine, under that portion of the bottom occupied by the boiler and machinery, which apparently had not been disturbed for a long time.¹ The steamer left England for the Cape of Good Hope, on the 23d of February, 1847. Soon after sailing, a man was attacked with fever, which, though at first of a mild character, assumed malignant symptoms after the ship had entered the tropics. When off the Island of St. Nicholas, and almost in sight of Boa Vista, the man died, having had, for two days previous, black vomit, and other characteristic symptoms of the yellow fever. Within a few days afterwards, the steamer arrived at Ascension, where Dr. King was then stationed, with several new cases of the same disease on board. "The patients themselves," as we are informed by Dr. King, "attributed their illness to foul air in the fore part of the ship; one of them said he suffered so much from an abominable stench in the boatswain's store-room, that he represented the circumstance, and obtained permission to cut a hole in the floor, which exposed to view a considerable quantity of soft mud; and five or six bucketfuls of it, mixed with decayed shavings, and emitting an offensive odour, were removed at the time"² (pp. 12, 13).

¹ Bryson, p. 223; King, p. 12.

² Dr. John Wilson remarks, in his Statistical Report on the Health of the West Indian and North American Squadron, that the progress of clearing, perfectly cleansing, and then restowing ships-of-war in the West Indies, with the view of guarding against invasions of fever, are common; but it is a fact, however startling or difficult of explanation, that they are very generally followed, in no long period, by a serious visitation of the disease. "What relation there is between the purifying process in question and the subsequent eruption of fever, if it be an operative relation, may never be satisfactorily known; that the one frequently—generally—follows the other, is certain" (p. 85).

This occurred on board the Blossom (surveying ship), in 1835, while off Belize. "No fever of the kind existed at Belize, or any point of the Bay of Honduras, at the time it broke out in the Blossom, nor does any appear to have occurred during its cruise" (p. 85). The same result was observed on board the Forte (frigate), in 1835 (p. 110).

"When the holds of the Growler were opened at Woolwich, after her return from the coast of Africa, two men who slept directly over the hatchway were seized with fever possessing all the characteristics of yellow fever, and in the course of a few days they both had black vomit, and died in the Marine Infirmary, where they had been taken at the commencement of the disease."—Bryson, p. 224; Burnett, *Official Correspondence on the subject of the Eclair*, p. 71.

In all these instances, the disease was traced to communication with the vessels concerned, or to exposure, at a greater or less distance, to the atmosphere contaminated by the effluvia issuing from them. In none was it due to the introduction of a contagious germ through the agency of the sick, of merchandise, or of effects; in all, the febrile poison was generated on board. Were it necessary, similar examples of this mode of origin might be adduced from the history of British vessels—the Childers,¹ the Isis,² the Ferret,³ the Scylla,⁴ the Thracian,⁵ the Iphigenie,⁶ the Wasp,⁷ the Tribune,⁸ the Farmer,⁹ the Bustard,¹⁰ the Pylades,¹¹ the Antelope,¹² the Tigris,¹³ the Scamander,¹⁴ the Brazen,¹⁵ the Busbridge,¹⁶ the Pompey,¹⁷ the Bedford,¹⁸ the Powerful,¹⁹ the Blossom,²⁰ the Kent,²¹ the Circe,²² the Trinidad,²³ the Serpent,²⁴ the Pilot,²⁵ the Scout,²⁶ the Volage,²⁷ the Vestal,²⁸ the Skipjack,²⁹ the Rainbow,³⁰ the Magnificent,³¹ and others; but enough has already been said on the subject to place the question of that origin beyond the possibility of a doubt.

Nor have French writers on the yellow fever been less explicit in the expression of their belief in the reality of the generation of the poison on ship-board, or less careful in recording facts in illustration of it.

More than a century ago, Desportes regarded the disease as arising often from the foul condition of ships, and attributed the escape of a vessel—the Jason, of 74 guns, in 1746—at St. Domingo to its extremely leaky condition, by which the sources of exhalation in the hold were submerged. Dalmas³² relates the same of the Souverain, a 70 gun ship, in which the fever broke out and prevailed extensively during the passage from Europe to the West Indies. In this case, the fever attacked more readily, and was more fatal to, the artillery men and soldiers who slept on the lower deck. Those who slept on the gun-deck were less severely treated; while those employed in the rigging, as also the officers, escaped almost to a man. These facts, when taken in connection with the circumstance that the disease commenced

¹ J. Wilson, pp. 159, 160; Birnie, Edinb. Med. and Surg. Journ., xiii. 333.

² Ibid., p. 154.

³ Ibid., p. 161.

⁴ Ibid., p. 161.

⁵ Ibid., p. 140.

⁶ Ibid., p. 142.

⁷ Ibid., p. 92.

⁸ Ibid., p. 92.

⁹ Ibid., p. 92.

¹⁰ Ibid., p. 92.

¹¹ Ibid., p. 92.

¹² Birnie, Edinb. Med. and Surg. Journ., xiii. 335.

¹³ Ibid., xiii. 335.

¹⁴ Ibid.

¹⁵ Ibid.

¹⁶ Brice on Yellow Fever, p. 19.

¹⁷ Dickson, Report in Bancroft's Sequel, p. 172, 173 (note).

¹⁸ Blane, ii. 118; Sir W. Burnett on the Bann, pp. 28, 29.

¹⁹ Wilson, Statistical Report of the Health of the Navy, p. 85.

²⁰ Arnold, p. 258.

²¹ Burnett on the Bann, pp. 32, 33.

²² Dickson on the Yellow Fever, Edinb. Journ., xiii. 48; Bancroft's Seq., p. 140.

²³ Johnson on Influence of Tropical Climates, 1st ed., p. 164; Bancroft's Seq., p. 211.

²⁴ Bryson, Statist. Report of the Health of the Navy (British), p. 101.

²⁵ Ibid., p. 116; Second Report on Quarantine, &c., p. 67.

²⁶ Burnett, Official Report on the Fever which appeared on board H. M. ship Bann, &c. (Lond. 1824), p. 42.

²⁷ Birlwhistle, London Lancet, Jan. 1846; Med.-Chir. Rev., xlv. 218.

²⁸ Arnold, p. 397.

²⁹ Ibid., p. 289.

³⁰ Ibid., p. 299.

³¹ Ibid., p. 260.

³² Recherches Historiques et Médicales sur la Fièvre Jaune, 2d ed., p. 84.

at sea, and without the vessel having communicated with contaminated ships in port, show that the cause of infection was located in the hold.

The documents collected by Dr. Chervin, contain several examples of the generation of the yellow fever at sea, before the infected vessels had reached West India ports.¹ Rochoux² refers to several interesting instances of a somewhat similar kind. The following is worth recording: "The brig *Le Messenger*, forming part of the West India squadron, under the command of M. De Menars, was sent, in 1817, to St. Martin, to collect wood. The hold of the vessel was filled with mangroves. Soon after, the effluvia arising from these trees occasioned such a degree of infection, that in a short time the greater part of the crew were attacked with fever, and many died—among them the captain."³

This case is the more interesting to us, as the relator, while properly referring the disease to the effluvia arising from the mangroves, labours hard in the volume quoted, and in other publications, to establish a distinction between the fever of tropical climates and that of Europe and this country, attributing the latter to local infection, and the former to the mere action of heat.

We read of the brig *Fabriceus*, of Marseilles, in which the yellow fever broke out at sea before it had reached Fort Royal, during the sickly season of 1818, and which arrived there on the 29th of September, with several sick on board.⁴ The distance at which this vessel was from the coast at the time of the outbreak of the disease, forbids the idea suggested by Kéraudren,⁵ that the infection was due to the wafting of the effluvia from the shore.

We read also of the *Columbia*, which arrived at Marseilles from Rhode Island in 1802, with the yellow fever on board;⁶ and of the *Nicolino*, which suffered there in 1821 from the same cause.⁷ In neither of these vessels could the disease have arisen from any but local causes. As regards the former especially, the idea of a foreign source is inadmissible; for the *Columbia* had sailed from Rhode Island, where, as we know well, the disease did not prevail at the time of her departure.

The French brig-of-war the *Euryale*, commanded by M. Villaret de Joyeuse, was attacked with the yellow fever while on a cruise, and compelled, in consequence, to seek shelter in Fort Royal, Martinique, about the close of June, 1821. Before reaching that port, the *Euryale* had already lost six men, and among them the surgeon; and at the time of her arrival the sick list was very large. The sick were sent to the hospital, and the convalescents removed to

¹ Rapport lu à l'Académie Royale de Médecine, sur les documents de M. Chervin, p. 9.

² Recherches sur les différentes Maladies qu'on appelle Fièvre Jaune, pp. 151, 152.

³ Ibid., p. 61.

⁴ Lefort, Journal Générale de Méd., Nov. 1820; Ibid., Réponse à Kéraudren, pp. 29, 31; Sedillot, Opinion de M. Lefort, p. 5.

⁵ Loc. cit., p. 31.

⁶ Robert, Guide Sanitaire, p. 708.

⁷ Ibid., pp. 244, 622, and Annales de la Med. Physiol. Oct. 1821; Robert, Observations sur la Fièvre Jaune importée à Pomegues en 1802, 1804, 1822, p. 132. See two interesting letters on the subject, by Dr. Ségaud, in Berthe's volume on the Yellow Fever of Andalusia, p. 395.

Fort Bourbon. In neither of these places was the disease communicated to the attendants or others ; but, on the other hand, a number of men who were sent to work on board were seized with the fever, and several died. In this, as in other instances, the disease did not extend beyond the focus of infection where it had originated, and where it affected those who exposed themselves to its action.¹

Dr. Lefort, to whom we are indebted for the above case, adduces (p. 16), as additional proof of the local origin of the yellow fever on shipboard, the account of three other vessels of war, the *Egerie*, the *Diligente*, and the *Silene*, which, during the sickly season of 1821, anchored at Trois Islets, a port situated at the bottom of the Bay of Fort Royal, Martinique. During their stay, the *Egérie* was attacked with fever, and lost a great many men. She was ordered to sea ; but the disease increasing, instead of being arrested, she re-entered the port, and there went through the usual process of purification. While the *Egérie* was at anchor at the Trois Islets, the intercourse between her and the *Diligente* and the *Silene* was in no way prevented ; notwithstanding which, these vessels remained perfectly free from the disease. On the 19th of October, the *Diligente* proceeded to Fort Royal, and soon after was itself attacked with the fever. On the 30th, she was ordered to sea in company with the *Silene*, and during the passage from Martinique to Porto Cabello, but more especially after a week's sojourn in that place, suffered extensively from the disease. During the whole of this time, the *Silene* continued free from the infection, although daily visited by men from the suffering vessel. It is scarcely necessary to remark that if the *Egerie* had derived the disease from the atmosphere of the Trois Islets, or from contagion, the *Diligente* and the *Silene* would, being exposed to the same causes, have shared a similar fate ; and had the *Diligente* not suffered from the action of some cause inherent to the vessel itself, it is difficult to understand how its companion, the *Silene*, could have escaped so effectually the inroads of the fever, exposed as it was to the same atmosphere, communicating in the same way with the shore, and visited frequently by the crew of its infected companion.

To the same effect, we have the case of the brig *Donostierra*, at the port of Passages, in the autumn of 1823. This port, in the province of Guipuscoa, is situated at the bottom of the Bay of Biscay, and forms a sort of appendage to St. Sebastian. The entrance to it is between precipitous rocks, and is so narrow and oblique as to be with difficulty discovered. The town, whose population is very small, consists of one street, placed on a shelf of rock, so narrow that it does not admit of the passage of carts, and scarcely of horses, while the base of the mountain of Olearso is almost in contact with the houses. These are badly ventilated, filthy, dark, and crowded.² This vessel sailed from the Havana, with a clean bill of health, in the beginning of June. The crew consisted of twenty-two men, including the captain, and there were

¹ Lefort, Réponse à M. Kéraudren, p. 13 ; Ibid., Journal Universel des Sci. Méd., Oct. 1821 ; Ibid., Obs. des Sci. Méd. de Marseilles, v. 208, vi. 311.

² Second Report on Quarantine (Yellow Fever), Lond. 1852.

besides five passengers on board. As to the cargo, it consisted of sugar, coffee, tobacco, yellow wax, honey, and preserves. Ten or twelve days after being at sea, one of the sailors died of an acute disease, which, however, the captain denied to have been the yellow fever. "After a passage of thirty-five days, the vessel touched at Corunna. The captain reported this event to the medical junto, in consequence of which the vessel was ordered to quarantine ten days. At the expiration of this time, it proceeded to St. Andars, remained some days in that port, and next directed its course to the Passages, where it arrived on the 2d of August," two months after leaving the Havana. During the whole of that time, the crew and passengers had remained in good health; and as at the period of arrival there were no sick on board, and as she had been at Corunna and St. Andars, she was not here put in quarantine.

The cargo was discharged on the 6th of August, and for several days a great many people went on board without contracting any disease. On the 19th, carpenters were employed in removing some of the planks of one of the sides, which were found greatly decayed. The first individual attacked with the fever was a custom-house officer, who had slept several nights on board. He sickened on the 15th, and died on the 17th. The carpenters were soon all affected, and the greater number of them died. On the 23d and 24th, two women who had communicated with the vessel during the week, and who occupied a house situated close by, were attacked, and died. From this moment the disease spread to all the houses in the vicinity of the vessel, which was evidently the source of the infection.

It would require more than an ordinary degree of faith in the doctrine of importation to attribute this outbreak of the fever, at the time of the occurrence, to intercourse with the passengers and crew of the ill-fated vessel. Two months and a half had elapsed since they left the Havana, and they had all arrived in good health. Independently of this, during the prevalence of the disease, nothing transpired capable of proving its contagious character. It follows, from the account we possess of this visitation, that the disease did not extend beyond the houses opposite the ship; that when individuals were attacked, whose habitations were at a distance, it was occasioned by their having remained for some time within the space to which the malaria from the ship appears to have been confined; that the heat was excessive, the thermometer reaching to 96° ; that the course of the wind favoured the conveyance of the effluvia from the ship; that many individuals, evading the sanitary regulations, passed out without certificates of health, and took with them clothes even from the houses where people had died; and that neither these individuals, nor the sick that were conveyed to different parts, nor the effects that were carried out, communicated the disease to the surrounding country or elsewhere. Nor can we attribute it to the cargo, as it had not infected any one on board who had assisted in unloading the vessel, and had, besides, been removed before the commencement of the fever. More rational is it to refer the cause to the foul condition of the vessel itself—a condition not attributable to the miasma with which the hold was impregnated, and resulting

from the cargo and crew, since the former was innocuous, and the latter, had they been infected, were not located in the hold, but in the cabin and steerage, which were not the seat of the infection, but to the state of the timber, &c.; for the disease did not show itself until the sides of the vessel had been opened and the decayed wood exposed. The custom-house officer, it is true, died before this operation was performed; but, from the nature of his duties, he was obliged to visit every part of the vessel, and was therefore exposed to the direct action of the deleterious effluvia issuing from the interior of the vessel, even before the decayed timber had been fairly uncovered.¹

The following case, the last I shall adduce, affords a strong illustration of the sudden development of the disease on board of ships while at sea. It has often been briefly referred to, but deserves a more particular notice.

In the year 1802 (12 Floreal, an. 10), a flotilla, filled with French troops, sailed from Tarentum for the island of St. Domingo.² The vessels consisted of small Neapolitan polaccas—under the escort of a frigate—each of which, though only intended for the accommodation of at most one hundred men, received one hundred and fifty (p. 7). Encountering, soon after leaving the port, a severe storm, the vessels were dispersed, and sought shelter where best they could. They reassembled at Leghorn, and thence proceeded to Cadiz, there to join another division of troops that were to form part of the expedition. Stopping again at Carthagená, to take in proper provisions and to refit, the expedition set sail for St. Domingo—the troops being now transferred to eight vessels freighted for that purpose. Of these vessels, one was set apart for the accommodation of the sick.

The spring had been cold and wet. Summer came on suddenly, and was characterized, during the months of June, July, and August by intense heat (p. 10). Soon after the departure of the vessels from Carthagená, fever broke out on board, and continued to prevail in some of the ships till their arrival at Cape Haytien—spreading more extensively and acquiring greater malignancy as they approached the tropics and were exposed to a higher temperature. The disease, without doubt, consisted of one of the forms of true yellow fever; but exhibited, especially at first, a mild character. At the time of arrival, the yellow fever, in all its purity, was prevailing among the troops at St. Domingo, and by comparing the symptoms presented by the cases on board with those noticed ashore, the surgeon of the squadron was enabled to convince himself of the identity of the two diseases (p. 21).

In this instance, there cannot be the remotest reasons for referring the disease to any other than a local origin. Nothing is said of the soldiers having imbibed the cause at Leghorn, Cadiz, or Carthagená, in neither of

¹ Arruti *Tratado de la Febre Amarilla, &c.*, pp. 1–70; *Notice Topogr. du Port du Passage où la Fièvre Jaune a régné pendant les mois d'Août et Sept. 1823*, par E. L. Jourdain, *An. de la Méd. Physiol.*, iv. 505; abstract of this in *Med. Rev. of Phila.*, i. 260; *Journal Générale*, lxxxvi. 225, 231; Audouard, *Relation Hist. de la Fièvre Jaune qui regné au Port du Passage en 1823*; *Rev. Méd.*, Août, 1824; Chervin, *Examen, &c.*, p. 31, &c.

² Béguerie, *Histoire de la Fièvre qui a régné sur la Flotille Française, sortie du Port de Tarente*, p. 5. Montpellier. 1806.

which places, indeed, it existed that year. Dr. Béguerie lays some stress on the effects of bad regimen, but especially on the excessive heat to which the men were exposed; as also on vicissitudes of temperature and exposure to night air (p. 21); but he likewise attaches much importance to the morbid exhalations arising from the accumulation of the troops and the decomposition of animal and vegetable substances contained in the vessels (pp. 21, 24, 36, 88). It may not be improper to add that the disease manifested no contagious property (p. 73).

I might, in addition, refer to the interesting account of the outbreak of the fever on board the French war vessel, the *Caravane*,¹ to the U. S. ships *Natchez* and *Vincennes*,² and others, but enough has now been said to show clearly and undeniably that the true yellow fever has often been generated on board of ships under circumstances which forbid the idea of its having been the effect of contagious germs therein introduced, or of malarial effluvia wafted from contaminated vessels lying in the vicinity, or from infected localities on shore. It is easy to understand, that such examples of development must prove not a little embarrassing and annoying to thorough contagionists. A perusal of their writings will show that, with a few honourable exceptions, they deny the production of the fever anywhere on land, through the agency of local sources of infection, and necessarily feel no disposition to make an exception in favour of such sources when located in ships; or, if they admit the possibility of such a mode of origin, they limit the sphere of its occurrence within comparatively narrow bounds, rejecting, as unfounded and even absurd, the idea of including within these any section of the temperate zone. It cannot be matter of astonishment, therefore, that, entertaining such views, they should, as already seen, have denied *in toto* the truth of the statements made on the subject before us, or, when this proved impossible, endeavoured to explain them away on principles harmonizing with their opinion respecting the etiology and mode of propagation of the disease.

Be this, however, as it may, the facts presented in the preceding pages, and which have been collected from sources entitled to full confidence, are of a nature to overcome effectually, all the objections raised against the reality of the generation of the yellow fever poison on shipboard. They furnish us the means of repelling the assertion sometimes hazarded by less exclusive partisans, that if the disease truly arises spontaneously in ships, it does so only in such parts of tropical climates where it is endemic; that there it is, perhaps, the effect of atmospheric influences—seldom, if ever, of local sources of infectious effluvia; that when perchance it appears in vessels lying in ports situated in temperate regions, those vessels have arrived from such localities, or from places infected by communication with them, and that the fever has been derived directly or indirectly from tropical climates, and not from any cause eliminated on board. All this is refuted by the fact that in some of the cases cited, the fever broke out in ports situated in temperate regions under circumstances incompatible with the idea of importation, and even in

¹ Bertulus, *Obs. et Réfl. sur l'Intoxication Miasmatique*, p. 101.

² Drake, ii. 231.

vessels coming from parts of Europe where the yellow fever has never existed; that in others the fever made its appearance at sea before the vessels had reached tropical latitudes, or in vessels proceeding from one port situated in temperate regions to another of the same kind; and that, in not a few, the connection between the appearance of the fever and the existence of materials which elsewhere give rise when in a state of decomposition to febrile effluvia, is too apparent to be doubted.

Some of the facts adduced equally disprove the assertion that the development contended for takes place only, or principally, in vessels containing individuals who had recently visited sickly ports, or had there been affected with the fever; or, as it is at times maintained, that it occurs only in vessels lying within the influence of infected ports in tropical regions, and is to be referred to such influences; for in some of the instances described or referred to, the effect took place in vessels—as the *Hornet*, *Blossom*, *Eclair*, *General Greene*, *Delaware*, *Levant*, *Mary*, *Columbia*, *Antelope*, *Kent*, *Circé*, that had sailed from, or were lying in ports where the fever, if it ever prevailed, did not exist at the time, or had not done so for some, or even many years before, and which, therefore, were not likely to contain any individual who had recently, if ever, passed through the disease, or who could in any contingency have derived the latter from morbid effluvia emanating from surrounding objects.

Not less opposed to the assertions in question, are the cases of the *Fabricius*, *Thraeian*, *Lively*, the French flotilla from Tarentum, &c., in which the fever appeared at sea, during a cruise or passage, and far from any contaminated spot; as well as those of the *Pique*, *Naney*, *Scout*, *Diligente*, *Rattlesnake*, *Ferret*, *General Greene*, *Macedonia*, *Pyramus*, *Nyaden*, *Lively*, *Pylades*, *Bedford*, *Kent*, *Pilot*, and *Seringapatam*, in which, while those vessels were severely visited by the fevers, others situated close by in port, or at sea, or on the same cruising-ground, remained uninjured. In the first of this category of cases the cause of infection could not have been derived from surrounding objects, and must have been located in some parts of the vessels themselves; in the other, the complete escape of other ships exposed to the same influences from the shore, and the restriction of the disease to one, is, to say the least, a strong reason for regarding it as an intrinsic product of the latter itself or its contents, and not of effluvia derived from an extrinsic source, at the place where it was lying, or whence it had just sailed.

The foregoing statements furnish us, moreover, with materials to disprove the idea, sometimes entertained, that the disease, when it breaks out on board of ships without its being traced to an immediate exposure to contagious effluvia, will be found to do so very usually, if not always, in vessels in which it had existed, at some antecedent period, sporadically or epidemically; and that hence it is simply the effect of a revivification of germs left there at the time of the preceding visitation. Such assertions receive no support from the history of the very large majority of instances mentioned, for in them the fever made its appearance unexpectedly in vessels in which it had never

existed before ; while in the few which had been infected at some antecedent period, the occurrence was of too ancient a date to justify the supposition that the development of the disease could in any possibility be due to the cause assigned. Such a supposition would imply the admission that the germs of that form of poison giving rise to the yellow fever can remain for months or years in a state of latency in the human system or in surrounding objects, and be brought at last into action through the agency of some exciting cause. Need I remark that, notwithstanding all that has been said on the subject by Currie, Bally, Pariset, Arejula, and others, the existence of such a power of retention has never been verified on land, even under circumstances the most favourable to such a result ? And surely, if it does not there manifest itself, it would be difficult to point out the most distant reason why it should do so on board of ships.

But even were there no other reason for disbelieving the extrinsic and contagious origin of the fever in the instances before us by the revivification of germs or otherwise, the opinion of that origin may properly be rejected on the ground that the disease produced on board has not been found to be communicated on shore. It will be found on examination that individuals affected in the Rattlesnake, the Euryale, the Regalia, the Peacock, the Pyramus, the Levant, the Vestal, the Bedford, and the Blossom, were landed and received into hospital wards in various places, and that, notwithstanding the freest intercourse was allowed, the disease was not in a single instance communicated to those around. Nor need we hesitate to affirm that when the various instances of contagious communication alleged to have proceeded from such contaminated vessels in this country and elsewhere are carefully scrutinized, they are found destitute of proof, and are more readily explained on other principles.

Even the case of the *Eclair*, upon which so much stress has been laid by the advocates of importation, loses, when examined attentively, the importance it has acquired. While those who went on board took the disease with almost unerring certainty, it remains yet to be proved that those affected communicated the infection on shore. So, as Dr. Wilson, a competent authority on such matters, remarks, "it happens, if not universally, almost universally. Nearly every man who joins a ship in such a condition has the prevalent disease sooner or later; but no number of persons taken from such a ship, labouring under the disease in any stage, or in any force, and placed in a situation where the disease does not exist, though in a mass of healthy people, can excite it in a single instance."¹

We also see that exposure to the effluvia issuing from the hold during the cleansing and purifying process in ships, heretofore uncontaminated or very long free from the fever, and after the crew had been dismissed, and no one was left on board to communicate disease, has often been, not in tropical and fever regions only, but in temperate climates also, the cause of the most concentrated and fatal form of malady. In a word, the facts that have been

¹ Wilson, Statistical Report, p. 110.

adduced establish, beyond the possibility of a denial, the reality of the development of the yellow fever on shipboard, from the operation of causes existing therein, and unconnected with any contagious or infectious germs introduced from without.

Whether the infection proceed from the effluvia issuing from the bilge-water, the timber of the ship, the filth of the hold, or the cargo, need not be made the subject of inquiry in this place. Sufficient is it to know that the existence of the cause within the precincts of the ship, particularly in the hold, is rendered evident not only by the circumstances already mentioned, but by the limited space and the particular spots to which it is confined at the outset, or throughout the whole course of the epidemic. The disease usually, or very frequently, makes its appearance, and is more severe in the vicinity of the pumps and main hatchway. This is exemplified by the occurrences on board of the *General Greene*, the *Macedonia*, the *Rattlesnake*, the *Rainbow*, the *Force*, the *Skipjack*, the *Ferret*, the *Scylla*, the *Lively*, the *Isis*,¹ and is nothing more than what might be expected; for there the keel is most dependent, the water draining from other parts is accumulated, and the heat is most intense. Thus the first cases, and the largest rate of mortality, have, in many instances, been found to occur in the berths of the midshipmen and marines, which, in English frigates, are placed on each side of the pumps and main hatchway. By Dr. Wilson we are told, that the subsequent progress of the disease depends on the trim of the vessel, and the inclination of the keel from the horizontal position; the fever spreading in the most dependent parts.

In almost all cases, the disease prevails more, and the risk of infection is greater, in the lower than in the upper deck. It is sometimes confined to one end, or one side of the vessel. "As men fall under the influence of fever they are removed from their berths on the lower deck, to cots or hammocks on the main deck, which becomes the hospital of the ship for the time; yet the origin of the disease does not thereby become diffused. It continues local and circumscribed, and is extended only as the cause beneath is generated more slowly or rapidly in different instances. Thus, a man is removed from his berth to the main deck labouring under fever one day; another is removed the day after; one, two, or three the next day, and so on; the disease beginning then to extend itself, in one or more directions, goes on till it pervades the ship generally. Yet all the while it cannot be traced from those on the main deck to any other part of the ship. The gradual and regular extension of the disease will not be equally conspicuous in all cases, from the operation of causes which I shall not stop to enumerate; but if it happen in one case it is sufficient to show its local and strictly limited origin. It is, moreover, generally observed, that when men sleep and pass most of their time on the main deck, they are seized later and more lightly than those who sleep and live chiefly below."²

From these facts, and the appearance of the disease under circumstances which preclude the possibility of referring it to the operation of external

¹ Wilson's Statistics, p. 110.

² Ibid., Mem. of West Indian Fever, p. 158.

agencies, or the introduction of contagious germs, we arrive at the conclusion, already so often mentioned, that its development on board is often due to causes existing in the vessel itself, and brought into action by the long-continued heat of tropical regions, or the equally high temperature of the summer season of temperate ones.

CHAPTER XXIII.

PROOFS OF NON-CONTAGION—CONTINUED.

IN the preceding chapters facts have been adduced to show that the yellow fever has occurred in places situated at a more or less remote distance from seaports, and having but an indirect intercourse—often no intercourse whatsoever—with the sea-coast, and especially with those regions which, in the opinion of contagionists, are the native seats and starting points of the disease. Next, we have seen that the fever has, on numerous occasions, arisen and prevailed extensively on board of ships, and proceeded evidently from the morbid agency of causes of a local character. That instances of like nature, and equally subversive of the doctrine of importation and calculated to establish the domestic origin of the fever—in which the latter has sprung up under circumstances which leave no doubt as to its local origin and to the impossibility of viewing it as derived from an exotic source—have been observed in this and other countries, I presume, from all that precedes, the reader is fully aware. At the risk, however, of being accused of indulging in unnecessary details, I must devote a few pages to some remarks and facts on that subject. In the first place, it may be stated that in seaport towns which have more or less frequently suffered from the disease, the outbreak of the latter has not always occurred in or near the shipping. This fact is pronounced true so far as concerns New Orleans, by Dr. Fenner, who has demonstrated it in several of his writings. (*Fever of 1853*, p. 73.)

Of the epidemic of 1837, at Mobile, Dr. Nott tells us that it was announced by a single case on the 10th of September. “Four more cases occurred about the 20th, and it is remarkable that all these cases occurred at points so remote from the shipping and so distant from each other as to preclude the idea of recent importation or propagation by contagion.” The next epidemic at Mobile occurred in 1839, and commenced where it should have been the least expected—half a mile from the shipping.¹ The outbreak of the fever in 1843, “was remote from the wharves and shipping, and no suspicion was entertained by anybody of its introduction from abroad.”² See how matters stood at St. Pierre (Martinique), in 1837. “The disease,” says Dr. Rufz, “broke out in the garrison, spread to the city, and lastly extended to the

¹ Nott, N. O. Journ., iv. 565–6.

² Drake, ii. 224.

harbour. It is not, therefore, from a few infected vessels, or from some salt marshes (*marécage marin*) that it proceeds. The barracks, the hospital, and the houses of the first patients were not the nearest to the sea. These houses were widely separated from each other. These facts are opposed to all ideas of importation" (p. 27). At Pernambuco, where the fever prevailed epidemically in 1850, "it invaded the town by districts, beginning in that most distant from the shipping, and ending in that nearest to it."¹

Other facts of same import might easily be collected. I might recall to mind the breaking out of the fever in this city in 1820 and 1853, in the Northern Liberties at a considerable distance from the shipping and especially from all vessels that might have brought it. But as the disease had already prevailed for some time in other parts of the city, those instances may not be deemed conclusive. More satisfactory on that score are the outbreaks of the fever in 1805 and 1819. It is well known that in the latter year when it made its appearance in the ferry tavern on the upper side of Market Street, and in the contiguous buildings, there was not, nor had there been from the commencement of quarantine, any vessel near them from the West Indies or any sickly port. In 1805, no vessel, likely to have rendered us this poor service, and exonerate our city from the dishonour of being "the parent of such a pestilence" had approached nearer than the Lazaretto; so that the contagionists, in their endeavours to explain the manner in which the fever had been introduced from the West Indies, were compelled to have recourse to an individual who went down to the Lazaretto, and even below it, and approached some vessel or vessels supposed to have had sickness on board.

All, or almost all, cases of sporadic yellow fever—which, as we have seen, are not unfrequently encountered in various parts within the fever zone—arise from causes distinct from an imported contagion, and are therefore products of domestic agencies. In a preceding chapter, allusion was made to the spontaneous origin of the disease in various parts of our southwestern and southern States during the eventful year of 1853. Any one who reads attentively the accounts given of the origin of the epidemic of that year in New Orleans—the parent, according to some physicians, and the public generally, of all those that occurred in the southwest of this country—by Drs. Fenner² and Axson,³ will have no difficulty in discarding all idea of attributing the disease to any other than indigenous causes, for therein will be found that the first case that died came from the ship *Northampton*, an emigrant vessel from Liverpool direct—that other early cases coincident as to the time of sickening, with the one referred to, occurred on the ship *Augusta*, from Bremen—that the earliest cases traceable to three distinct localities where the fever afterwards prevailed had connection with the *Northampton*—that from none of the vessels, either from Rio or Jamaica, could the commissioners find a case of fever or death of all those occurring in May, or up to the period when it prevailed as an

¹ Lilly, in N. O. Report of Sanitary Commission, &c., pp. 196, 198.

² Trans. Med. Assoc., i. 425.

³ Rept. of Sanit. Commis. of N. O. 1853, p. 478.

epidemic, either among the crew or among those who worked on these vessels.

“The boldest advocate for importation will hardly dare assert the opinion that the fever was brought by the immigrants from Liverpool. She (the Northampton) must have either originated it in her foul holds or derived it from her locality. The latter source is rendered all the more probable by the following facts which we gather from our minutes of evidence. These will show the presence of some general and wide-spread agencies operating in that locality and repeating at different and distant places similar phenomena to those transpiring on the ships Northampton and Augusta. On the 22d of May, one day earlier than either McGuigan’s case or those in the Augusta, Dr. Zehender¹ visits a man living a few squares higher up with all the well defined symptoms of yellow fever; and on the 24th, one day later than the same cases, Dr. Schuppert² visits the butcher Keltering, who, after throwing up black vomit, recovers. These cases are believed not to have visited the shipping. This is literally true of Dr. Zehender’s cases. Again, on the 3d June, the second case is seen by this latter gentleman in a locality still more remote from the position of these ships. On the 2d June, more than a mile in the rear of the shipping, Kein and his wife come under the charge of Dr. M. M. Dowler. They both died with black vomit. A few days later, in a locality still more remote, and contiguous to the swamps, Dr. Campbell attends a case which died with black vomit. In all these latter instances there was positively no communication with the river or shipping.”³

Of the epidemic of 1791, in that city, Dr. Drake says there was no suspicion of its having been imported. Those of 1817, 1819, 1822, 1824, 1827, 1828, 1829, 1841, 1843, were not less shown to have been derived from indigenous causes, and as regards the others, the importationists have not succeeded in their endeavours to prove their foreign origin. So far from this, facts sufficient have been accumulated by Dr. Fenner,⁴ to prove conclusively that, as regards the epidemics of 1847 and 1848, all idea of a foreign origin must be abandoned.

Speaking of the fever of 1853, Dr. Ashbel Smith says: “Yellow fever made its appearance in Houston and Galveston about the same time, in the latter part of August. So far as the facts can be ascertained in Houston, they preclude the notion of its importation from abroad.”⁵ The fever of Rio Janeiro, whence that of New Orleans was at one time thought to have been derived, has most conclusively been proved by Drs. Pennell, Lallemant, and Candido,⁶

¹ See Sanitary Map of the city, and Dr. Zehender’s testimony.

² See ditto and Appendix—Dr. Schuppert’s testimony.

³ Report of Sanitary Commission of New Orleans, 1853, pp. 491–92.

⁴ New Orleans Med. and Surg. Journal, v. 192; vi. 10.

⁵ Trans. of Med. Assoc., vii. 535.

⁶ Pennell, A Short Report upon Yellow Fever as it appeared in Brazil, &c., p. 7; Lallemant, Observações acerca da epidemia de Febre Amarella, &c., pp. 2, &c.; Candido, Conseils contre la propagation de la F. J., &c., p. 2; see also Rept. of Sanit. Commis. of N. O. 1853, p. 185.

to have originated in that city, and certainly to have been introduced neither from Africa, from New Orleans, from Baltimore, nor from Canada. I might, in addition, show that all the epidemics of this city—from 1699 to 1854—all those of New York, Boston, Baltimore, Charleston, Mobile, Pensacola, &c., have arisen from domestic causes—that while some are admitted on all hands to have been due to such agencies, importationists have failed, in regard to others, to prove the correctness of their views.

Until recently, scarcely a voice was heard in favour of importation and contagion throughout the whole city of Charleston. Every one there knew, what Dr. Porter¹ has recalled to the memory of the present generation, that the first two or three epidemics in that city could not have been imported from the West Indies, for there was no foreign trade, the infant colony being satisfied, according to Dr. Chalmers, during the first years of its existence, to have a precarious intercourse with the lords-proprietors in England. They knew this, and knew also, that if such was the case in regard to those early epidemics, it could scarcely be otherwise at a subsequent visitation, and that, besides, nothing had transpired to cause the manifestation of the disease to be regarded as originating in an imported cause. What Dr. Strobel,² and more recently Dr. Hume³ and a few others have done to establish a contrary view, is not calculated to carry conviction to unprejudiced minds.

It would be easy also to demonstrate the fallacy of the theory of importation as regards the epidemics of Cadiz, Barcelona, Carthagená, Málaga, and other cities of Spain, of Leghorn, &c. Some of these, if connected with circumstances which afford reasons for withholding a decided opinion as to their local origin, are certainly not proved to have been due to importation; while, as regards others, the question is decided in a manner satisfactory to all impartial inquirers. Take the following as examples: As is well known, the yellow fever prevailed extensively and fatally in Cadiz and other cities of Andalusia, in 1800. On this occasion, as in every other, the disease was referred to the importation of the poison from abroad, by Arejula, Gonzales, and other Spanish physicians, as also by Fellowes, Berthe, Blane, Pym, Bally, Palloni, Strobel, Pariset, François, Carpenter, and other English, French, Italian, and American writers. By the government, the cause of importation and contagion was also warmly espoused, and when an intelligent and meritorious Spanish writer, Don R. Armesto,⁴ boldly affirmed and undertook to prove the erroneousness of these views, the sale of his work was prohibited, and publicly burned by the hands of the hangman; while the writer was obliged to make the *amende honorable*—in other words, to retract his opinion. The disease was supposed to have been introduced by the ship *Dolphin*, of Baltimore. This vessel was chartered at the Havana, to convey to Spain the Intendant of Cuba, his family and attendants. She left the Havana on the 27th of May, touched at Charleston on the 2d of June,

¹ Am. Med. Journ., Jan. 1855, pp. 98, &c.

² On the Transmissibility of the Yellow Fever, pp. 121, &c.

³ Charleston Med. Journal, x. pp. 2, &c.

⁴ Reflexiones sobre la Epidemia padecida en Cadiz, &c.

left there on the 10th, and anchored in the Bay of Cadiz on the 6th of July, after having lost three men during the passage. The disease broke out in August. But it was proved that these men had not died of yellow fever; that the disease did not prevail at the Havana at the time of departure of the *Dolphin*; that no yellow fever prevailed in Charleston, that year, prior to the 20th, ten days after the departure of the ship; that cases of the disease had appeared at Seville and Xeres before it broke out at Cadiz, and that in the latter city itself, cases had been noticed prior to the arrival of the *Dolphin*. As regards the epidemic of Cadiz in 1819, we have the high authority of Pariset for the important fact that it came direct from Calcutta! No other vessel could be found upon which the stigma of having introduced the disease could be fixed—the disease must have been introduced from abroad—a vessel from Calcutta had arrived a short time before the outbreak of the fever; therefore, that vessel had introduced it. We have seen the shifts to which pure importationists and contagionists were put in reference to this epidemic.

The epidemic of Barcelona, in 1821, was, as a matter of course, attributed to vessels from the West Indies.¹ In opposition to this opinion, however, the facts and statements adduced by O'Halloran (p. 29), Rochoux (p. 71), Chervin (*Fev. of Spain*, p. 96), Maclean (p. 110), De Fermon (p. 56), Costa (p. 15), Porta (*Archives*, v. 282), Piguilem (*Broussais' Journ.*, i. 402), are conclusive. It is known that, in 1803, a similar, though less extensive, epidemic prevailed in that city, which could not be traced to a foreign source. Let it be added, in reference to the last named city, that it was frequently visited in ancient times by what was called the plague. In the fourteenth century, from 1333 to 1396, Barcelona suffered six times from that disease. In the fifteenth century, from 1408 to 1497, it experienced sixteen epidemics of plague, or other febrile malignant diseases. In the next century, from 1500 to 1598, those same diseases prevailed eight times.² There is reason to believe that the disease, on some of those occasions, when the American colonies, if discovered, had not as yet been settled, was the yellow fever. This, at least, may be inferred from the peculiar character of the monument of Sarria, for a description of which we are indebted to Dr. Rochoux.³ In an essay on the state of Medicine in Spain after the

¹ Rayet, p. 30; Pariset, Report, pp. 4, &c.; Audouard, pp. 3, &c.; Henry, *Malheurs de la Catalogue*, pp. 3, &c.

² Capmani, *Memories sobre la Marina Comercio*, &c., Madrid, 1792, iii. 126.

³ *Dissertation sur le Typhus Amaril ou Maladie de Barcelona*, pp. 16, 17.

“There is, in the garden of the Capuchins of Sarria, a very curious monument, the object of which is to commemorate the scourge by which that city was afflicted at the time it raged in Barcelona. It is made of painted Terra Cotta, and represents several groups of figures, the largest of which are a quarter the natural size; the others are much smaller. It represents a procession around a church. In the midst of the numerous attendants a man has fallen a prey to the plague. Around this central point there are some sick, some dying, some dead, assisted and carried by the monks. All of them have red ulcers on the sides, on the back of the neck, on the arms, or on the legs. Two of the sick are vomiting; one, who is very yellow, ejects, in large quantities, a black sub-

expulsion of the Arabs, Dr. Suelto refers for a description of the yellow fever, as it appeared in early times in that country, to the writings of Antonio a Fonseca on the plague and contagious diseases, of L. Brandaon and S. Nunez (*De Peste*), of Paul Correa (*De Causis et Curatione Pestis*), and of Emmanuel de la Corda (*Contra Pulverem Venenosum*).¹ The disease, in those early times, could not have been imported.

At Medina Sidonia, a town thirty miles from the coast, the disease broke out about the latter end of August, 1801, when all the seaport towns in the neighbourhood enjoyed a perfect state of health, and gradually proceeded in the same manner as the epidemics in Gibraltar.² Of Cadiz and Xeres, we are told, that what happened there in 1820 would induce the belief that the disease must be looked upon "as endemical, as its introduction could not be traced by the most careful investigations of the different Boards of Health, and particularly of that of Xeres."³

Of the local origin of the yellow fever in the West Indies, it is scarcely necessary to say much; for although, by some writers, the disease has been attributed to an African origin, the mass of testimony is adverse to that opinion, and to the idea of its transportability from place to place. That it did not affect the Spaniards during the first visit of Columbus to St. Domingo, may be true. But it should be recollected they were not there during the sickly season; for the account we have of this visit from Herrera, Oviedo, Fernando and Christopher Columbus, shows that they remained but three months—from the 12th of October to 16th of January. On his second visit, Columbus arrived in December, 1493, and forthwith established the town of Isabella (*Herrera*, lib. ii. cap. 10). Soon after, his companions became sorely afflicted with a disease characterized by some of the symptoms of, and progressing in a way analogous to, the yellow fever.⁴ Admitting the fever to have been truly of the nature mentioned, we have at once a proof of

stane; the other has his hand to his mouth, endeavouring to prevent his throwing up the matter which his swollen cheeks show his mouth to be filled with. His eyelids are of a copper-red colour; a monk supports his head with one hand, and, with the other, holds a vase containing a beverage which he seems to be offering him.

"There is, besides, a large number of figures, which I will not describe. I will close by speaking of a small edifice, two stories high, covered by a pall, and filled with bones. It is open on one side, and brings to view eleven dead monks, laid out, three on the second floor, four on the first, and four in the basement. At the bottom there is an inscription in the Catalan language, of which the following is a translation: 'Names of the eleven nuns who died of the plague in the year 1652, giving both spiritual and temporal assistance to the inhabitants of Sarria attacked with that malady; they rest within under this monument.'"

¹ Journal Un. des Sei. Méd., ii. 135-6; see a translation of this essay in Phil. Med. and Phys. Journ., vii. 263-4.

² Aniel, Edinb. Journ., xxxv. 267.

³ Rances, in O'Halloran, Fever of Andalusia, p. 166.

⁴ Oviedo, lib. 2, cap. 4, 13; lib. 3, cap. 4; lib. 16, cap. 3; Herrera, lib. 1, cap. 2; lib. 3, cap. 15; Benzoni, Traduction et Commentaires de Urb. Chauvalon, liv. 1, chap. 9, and 29, pp. 73, 368 (12mo.), 1579; Gomara, Hist. de las Indias, lib. 1, cap. 2 and 29; P. Martyr, Dec. 1, lib. 4; Fernand Colomb, Vie de Chr. Colomb, chap. 25, 58.

its indigenous origin in the West Indies. But even were this not the case, the proofs of that origin have been too frequently exhibited since the first known occurrence of the disease in that region, to be made now a subject of doubt and comment.

I have, however, sufficiently enlarged on this point, and after referring the reader to the appendix of this volume, where will be found an inquiry into the origin of some of the epidemics on which the contagionists and importationists have laid most stress, I pass to the consideration of a few more instances in which the fever has broken out in places in this country having no direct communication with the West Indies, and where the disease manifested itself without our being able in any way to trace it to importation. Already mention has been made, among other instances, of the epidemics of this city in 1805, and 1819, on which occasions the disease broke out under circumstances forbidding the idea of its being the offspring of importation; for no vessel, or individual, or fomites, capable of producing the mischief, had reached here. In 1853, the disease, as we shall see more fully, was attributed to a germ imported in the hold of a vessel from the West Indies. But, independently of sundry considerations for entertaining the strongest possible doubts relative to the correctness of this belief, it may be stated that some ten days prior to the arrival of that vessel, a decided case of yellow fever had occurred in an individual whose business called him to visit the localities which subsequently became infected. The next year the disease once more made its appearance; but on this occasion no suspicions attached to any source of importation. No vessel, no individual, no box of merchandise, no trunk of clothes, could be pointed out as likely to have introduced a contagious poison among us. The advocates of the exotic origin of the disease remained silent, and if pressed on the subject, contented themselves with the remark that the cases which occurred might be the result of a revivification of a germ left in the infected district the year before. It may be remarked, in addition, that after the fever had made some progress, several persons brought it with them from Savannah and Charleston, but not one case of communication could be made out.

During the summer of 1853, eight cases of what any one conversant with the disease would consider well-marked cases of yellow fever, occurred at the village of Brandywine, some twenty or thirty miles from Philadelphia. They were indubitably the result of local causes, inasmuch as no vessel and no individual had reached there likely, in any way, to introduce a contagious poison.¹

At Selma (Ala.), in 1853, "early in September, cases of sickness became more frequent, intermittents and remittents increased in number and violence, and soon the enemy marched with sure and insidious steps among us. The first case of unmistakable yellow fever occurred about the 17th of that month. The subject of it had not been to Mobile, or any other place where the disease prevailed, and at that time no case had been brought here by the boats."

¹ Bush, Accounts of a Mal. Fev. which occurred in Brandywine Village, &c., Am. Journ., xxvii. 328.

No other well-marked case was witnessed until the 7th October. On the 8th and 9th, other cases presented themselves. On the 10th, the first and only case brought to that place by the boat was landed, and died in a few hours. There "was nothing in the history of the epidemic that could, by the most favoured construction, be made to support the opinion of the contagionists, but, on the contrary, everything was presented that could be desired to confute and overthrow it."¹

The fever prevailed extensively in the city of New London in 1798. In the accounts we have of the event, we find that importation from abroad was not thought of—the different chroniclers, without exception, attributing it to domestic causes—discarding all thoughts of contagion and pointing out a condition of locality amply adequate to the production of a disease of the kind.²

We have already seen that the fever could not have been taken to Gallio-polis from the Atlantic States in 1796, for Mr. Ellicott's boat was the first that descended after the fall of the waters in the spring; and the fever, for reasons specified, could not have been carried from New Orleans. The fever of Marietta, if really the true yellow fever, may be adduced as an example of the local origin of the disease, for on this occasion there was not the most remote suspicion of its having been imported from abroad.

In 1828, when Memphis (Tenn.) was an inconsiderable village, with, as Dr. Drake remarks, scarcely any population in its rear, and so little commerce or social intercourse with New Orleans that steamboats seldom stopped, and a fortnight frequently elapsed without a single landing, the yellow fever made its appearance there. "For some time," Dr. Drake adds, "before the fever broke out, no steamer had stopped. The fever that year was epidemic in New Orleans, and sporadic in Mobile; but no case was landed at Memphis." The first cases were a man, wife, and two children. The five or six cases lodged in a house about one hundred yards from the others. The next five resided and worked at the distance of a quarter of a mile from the preceding, and had had, so far as could be ascertained, no communication with them. The disease spread before the death of the last of those noticed, and invaded every part of the village. Out of a population of from 250 to 300, forty were attacked, and one-half of these died. It was undeniably of local origin. (*Drake*, ii. 283.)

In 1806 the fever broke out spontaneously in the Richmond (Va.) penitentiary. The cases amounted to six or seven, and of these, one died. The symptoms were characteristic—pain in the head, red eye, the skin of a much darker colour than gold, black vomit, black feces, &c. In the case that ended fatally, the sensibility of the stomach was so excessive, that by gently touching with the finger the region of the stomach, it produced hiccups. "It would almost be a miracle, had the *imported germ* of the yellow fever visited

¹ Marks, Report on the Diseases of Selma (Ala.), during the year 1853; Tr. of Alabama State Med. Assoc. for 1853, p. 101.

² Holt, A Short Account of the Yellow Fever as it appeared in New London, &c., p. 24; Channing, Med. Reposit., ii. 402, 405; Coit, ib. p. 407.

the penitentiary. Here are a set of men almost completely cooped up from the rest of the world. The turnkeys, the respectable superintendent of the penitentiary, his whole family, are healthy. The guard, who traverse the outside of the building, are too far removed to communicate the contagion. Few persons have access into the building; no one can enter it without a permit from the inspectors; no one who has visited it lately has exhibited symptoms of the fever. There is not a single corner in the city where the fever has peeped forth; not one person who has fallen beneath it."¹

At the bay of St. Louis, in 1820, the fever originated among the soldiers, who did not communicate with New Orleans or with trading vessels. "When it appeared among the citizens and visitors, it broke out almost simultaneously in all parts of the settlement, among people with whom the soldiers had no intercourse, and as well among those who had not as those who had communication with New Orleans, and the vessels arriving hence, but principally among females in the higher classes of society, who lived very secluded, and really had no such exposure."²

Whatever may be thought respecting the origin of the epidemics of Natchez generally, there can be no doubt that the fever of 1823 was not, and could not in any possibility be due to importation, and that it must have arisen from the operation of causes located in the place itself. It commenced in houses remote from the river. The people of Natchez under the Hill remained free from it for a fortnight after several well-marked cases had occurred in the upper town,³ a result different from what might have been expected had the disease reached Natchez from infected places, above or below the town. But, besides this, the only place whence it could have been derived was New Orleans, for no other town in the Valley of the Mississippi experienced an attack. True to their theory, Drs. Monette and Carpenter, neither of whom were on the spot, of course trace it to that city. Now, it is worthy of remark that the fever, during this mortal visitation, in Natchez was scarcely, if at all, sporadic in New Orleans. Dr. Drake furnishes us the following information on the subject: "Dr. Thomas, who was in the city, declared to me that, positively, the fever was entirely absent that year; and, in a careful examination of the books of the Charity Hospital, I could find but two cases reported, one on the 23d of August, the other on the 11th of September. It seems impossible that the fever should have been epidemic without affording more cases to the hospital, seeing that its victims belong chiefly to the class of persons who, when ill, rely on that hospital for relief" (ii. 206). To this it may be added that Dr. Barton, in a list of the epidemics of New Orleans, says nothing of 1823.⁴ One of these cases, let it be remem-

¹ Med. Repos., x. 217, from Richmond Inquirer.

² Merrill, N. O. Journ., viii. 7.

³ Cartwright, Med. Recorder, ix. 8; *Ib.*, Address before State Med. Society of Louisiana, 1853; Merrill, Philad. Med. and Phys. Journ., ix. 202; *Ib.*, Memphis Med. Recorder, iii. 257.

⁴ Report on the Sanitary Condition of New Orleans in 1853, p. 463.

bered, occurred two, and the other four weeks after the epidemic in question commenced. The first did not originate in New Orleans, but was taken from a flatboat lying at the mouth of Red River, and brought to the city by a steamboat.

Dr. Hort, of New Orleans, states that in the town of Wilmington, North Carolina, there is no intercourse with the yellow fever region from the end of May until November, and very little at any time; yet, at the period the rice-fields are drained, cases of yellow fever occur among the unacclimatized, and he has known entire crews of vessels, from New York or some other northern ports, arriving at that season, to be cut off in seven or eight days.¹ The epidemic of that place in 1821, was clearly shown by Dr. Hill to have arisen from local causes.²

Dr. Hort has seen the yellow fever in Florida, fifty miles from the sea, where the black vomit was forcibly ejected five or six feet, and it could be traced to a local cause on the plantation. In the lower part of the house lot of one of his neighbours, a small pond was formed by the trampling of stock, into which a quantity of cotton seed had been washed by heavy rain. The negro cabins were on one side of this pond, some nearer to it than others; the dwelling-house was distant about one hundred yards. At the end of August, 1825, the yellow fever appeared, attacking both blacks and whites; and no doubt the mortality would have been great, if the gentleman had not promptly moved all hands to the pine woods, which were close at hand. Dr. Hort, having sat up one night with three of the white family, was attacked three days after with the yellow fever.³ I may mention that the doctor was well acquainted with the disease, and could not be mistaken as to its true nature.

It is stated by Dr. Strobel, and the statement has been credited by contagionists that the yellow fever which occurred in Augusta (Georgia) in 1839 was transmitted, there from Charleston. "It is true that they (Dr. Strobel's informants) admit there were some cases of fever which occurred early in the season, but it was not until several cases had been transported there from this place (Charleston) that it began to assume that particular form." (*Op. cit.*, p. 187.) On examination it will be found, however, that there were at least nine cases of yellow fever in the place before the arrival of a single one from Charleston. The cases which occurred in Augusta extend from the 5th to the 26th of July inclusive; while the first cases from Charleston did not reach the former place until the 27th of the same month. The disease commenced the same day and almost at the same hour in different houses.

"After having thus given," the reporter remarks, "the date of the commencement of the prevailing fever, the concurrent testimony of the attending physicians, and the period at which the cases of yellow fever were introduced from Charleston, and their different localities, we feel satisfied that further argument is unnecessary. That the epidemic has commenced fairly and de-

¹ New York Quarantine Report, p. 182; N. O. Med. and Surg. Journ., ii. 5.

² Med. Recorder, v. 87.

³ *Op. cit.*

cidedly before the introduction of a single case of disease from Charleston, must be evident to every unprejudiced observer.”¹

In allusion to the fever of Savannah in 1854, the Mayor, in a report to the City Council, remarks: “Various causes have been assigned by different individuals for the disease during the past season. I have felt it my duty carefully to investigate them. The first cause assigned was the introduction of the fever into our city from the brig Charlotte Hague, which put into Cockspur Roads about the 29th of June. She was a Danish brig, bound from Havana to Copenhagen. She was visited on the 30th by the port physician, Dr. Mackall, who reported two slight cases of sickness on board. It having been asserted that some of the men brought from her to the city died of the yellow fever, I have used every effort to obtain evidence to prove the fact; but I have found no proof to satisfy my mind, that there was any case of yellow fever brought up to the city from that vessel, and I herewith submit for your consideration, a letter from Dr. Mackall and one from Dr. Wragg, the attending physicians of the Savannah Infirmary, to which all the men were carried” (p. 11).

From the testimony of these medical gentlemen we find that the cases thus brought up from the aforesaid vessel were cases of remittent fever; that they both recovered; and that the yellow fever did not break out until a month after their discharge—*i. e.* on the 8th of August.²

As regards the fever of 1820, we might remark that Dr. Daniel, throughout his whole volume on the autumnal fevers of Savannah, published in 1826, makes no mention of importation; holds up the fever as the product of the soil and climate, and, after remarking that cases of yellow fever annually occur, states that the causes of the fatal epidemic of 1820, and of that of 1817, can be readily explained without resorting to contagion. But, laying this aside as the expression of opinion of an individual—though of weight on this subject—and appealing to something of a more satisfactory character, we find, in Dr. Waring’s able report on the former epidemic, facts and statements which go far to disprove the agency of any imported contagion in its production.

Dr. Waring produces evidence to the effect that it could not be traced to foreign ships, because two fatal cases had taken place, the one on the 7th, the other on the 10th of May; before the first arrival in the same port of a vessel from the West Indies on the 22d of that month. On the last mentioned day, a government vessel, commanded by Captain Newell, left the island of Ossabaw for New York, having lost a short time before one of his crew by the yellow fever. Another died during the passage, and a third in the Marine Hospital of that port, who was declared by the health officer, Dr. Bayley, to be a most malignant case. While at Ossabaw, none of the crew had communicated with the main land, or any of the shipping, so that

¹ Robertson, Report on the Origin and Cause of the late Epidemic in Augusta (Georgia), 1839, p. 4-9.

² See Mackall, “Introduction of Yellow Fever into Savannah in 1854, Charleston Med. Journ., x. 150, &c.

the disease must have been of local origin, and not the offspring of importation.¹

In the summer of 1826, some men died of yellow fever in Apalachicola Bay. They had come direct from Baltimore (where no yellow fever existed), bringing out a large stock of goods, to establish a store in the interior of the country. At that time there was no settlement in Apalachicola, and no intercourse with the West Indies or any place from which the disease could have been imported.² In 1817, there were three boats running on the Mississippi River, but they seldom visited New Orleans during the sickly season. One, however, ventured down that year; two gentlemen are now living who were on board. The yellow fever broke out among them before they reached the city. In 1819, the yellow fever carried off a whole family at Bayou Sara, from the operation of local causes. Prior to this family being attacked with the disease, and destroyed, no case of yellow fever had occurred there, nor did any other case occur, for the cause was immediately removed (p. 184).

In 1841, a family left Baton Rouge, in a flatboat, for Donaldsonville. There was no yellow fever that year in the former town, or in the intermediate one of Plaquemine. The family had been two years from France. When they reached Donaldsonville, the father and his eldest daughter were ill with the fever. Dr. Sabin Martin was called in, and found the former walking about his room, and saying he was not sick. In three days, however, he died with black vomit and jaundice. The daughter had all the characteristic symptoms of yellow fever, and died in four days; previous to which she had suppression of urine, her skin became yellow, and she threw up great quantities of black matter. Dr. Drake, from whom I borrow the fact, got it from Drs. Martin and Cotman, good judges in matters of the kind (ii. 247, 248). The history of the fever which occurred at Gallipolis, in the State of Ohio, in 1796, and to which reference has already been made, is in point; for on that occasion there could be no question as to the local origin, the non-importation, and the non-contagion of the disease.

But I have said enough on this subject for the present. I trust that the facts adduced are sufficient to prove, beyond a possibility of denial, that the yellow fever has originated, in this country and elsewhere, from the operation of domestic or local influences, and without the concurrent agency of a contagious poison; while, from a review of the various points examined, the inference is not unfair that the disease extended from the action of the general

¹ Waring, Report to City Council, p. 34, &c.

Other vessels from the West Indies and New Orleans, five in number, reached Savannah from the 2d to the 27th of June, of course long after the disease had made progress. They had, besides, healthy crews, and there was nothing in relation to them which could authorize a belief of their having either severe disease on board, or the power of propagating any fever whatsoever. By some, it was supposed to have been brought from the coast of Africa by the brig Ramirez which had on board a cargo of young negroes. But this vessel arrived with a healthy crew, and an entire freedom from any malignant disease; and did not reach Savannah until some time after patients had died of black vomit, on the 22d of July (pp. 34, 35).

² Op. citat., p. 183.

cause thus generated, and not through the instrumentality of a contagious principle communicated from the sick to those in health. My distinguished friend, the late Dr. Drake, has well said (ii. 296) that the occurrence of the fever from local conditions, independent of any foreign agency, being established in even a single instance, the controversy is, *de jure*, at an end; for if it can be thus generated in one case, it may in all, and to say that it is not is to superadd a hypothetical to a known cause, in violation of the rules of philosophizing. He, indeed, who admits its production in this manner in one instance, and denies it in others, is bound to support his denial: he brings on himself the *onus probandi*. When a phenomenon has been proved to arise from a certain cause, true logic requires us to refer it, whenever and wherever it may afterwards appear, to the same cause. If, then, the yellow fever in the village of Memphis, in Galliopolis, at Natchez, in Florida, at the bay of St. Louis, in the Richmond prison, in the village of Brandywine, on board of Mississippi steam and flatboats, in Apalachicola, and in the very many towns and villages mentioned in this and former chapters, arose from local causes only—if some of the epidemics of this city, New York, New Orleans, Mobile, &c., are shown to have been produced by similar agencies, and if there it has not been propagated by contagion—we may conclude that it has arisen in every other place, and in the same place at every other time, from like causes; in other words, that it is indigenous, and has everywhere and always spread independently of a contagious principle.

Beneficial Effects of Expurgation, Proof of Local Origin.—The local origin of the yellow fever, and its independence of a contagious poison or an imported germ, is shown not only by the outbreak and prevalence of the disease, in ships or on land, under circumstances forbidding the idea of its being the effect of any morbid agency introduced from abroad, but also by a stop being put to its progress by the removal of the materials from which the poison is eliminated, or by the neutralization or destruction of the effluvia issuing from the infected localities. It not seldom happens that vessels in which the disease has sprung up at sea or in port have infected the vicinity of the place where they were moored. By their removal, a stop is immediately put to the disease. For example, the "Sea Island" infected a portion of the town of Middletown in 1820. She was removed some miles up the Connecticut River, and moored opposite a cotton factory. The fever stopped at Middletown, while cases soon appeared at the place near where she had been sent. But satisfactory as such cases may appear to many, and however entitled they may be to the consideration of all, I shall not dwell upon them here, inasmuch as some may contend that, whatever be the manner in which the disease originated, the poison issuing from the vessel may have been the product not of the vessel itself, or its cargo, or the contents of its hold, but of the individuals on board, and that it possessed contagious qualities.

In other instances, the disease has been arrested on board of ships by hygienic measures of various kinds, but all having reference to the impure condition of the vessel or its contents, and not to the individuals aboard, sick or well, the intercourse between whom remained unchanged, while the pro-

cess of disinfection was going on. Thus we have seen that in the *Pyramus* the disease, after committing considerable ravages, was put a stop to by landing the crew, and placing them beyond the reach of the effluvia issuing from the vessel. The latter was thoroughly expurgated. After this, "the crew re-embarked on the 14th of March, in excellent health, and remained so." The reader will recollect, also, that after the unarmed transport mentioned by Dr. Hartle had undergone a general purification, the fever ceased in her. In the *Dart*, the fever was put a stop to by removing some of the tanks, "knocking down the bulwarks, and cleansing the cisterns." In the *Regalia*, the cleaning of the hold proved particularly injurious to those exposed; "and while the fumigation of the ship was of no avail in purifying it, the fever ceased when she was completely cleaned, and after, with her hatchways closed, her whole hold had been exposed to the concentrated heat of many stoves." After being purified, the *Regalia* sailed from Guadaloupe for Europe, crowded with prisoners, and having on board a case of fever, which terminated fatally, without communicating the disease to those around. We have seen that in the *Rosamond* (formerly the *Eclair*) the fever broke out a second time, and was traced to a quantity of soft mud mixed with decayed shavings, and emitting an offensive odour. On the removal of these substances, a stop appears to have been put to the infection.

In the case of the *Trinidad*, at Barbadoes, mentioned by Dr. James Johnson (p. 164, 1st ed.), the fever disappeared as soon as the hold was washed and cleaned, the dirt, filth, and stagnant water removed, and scuttles cut for better ventilation. The disease on board of the *Egérie* was put a stop to by the usual process of purification. So also in the *Diligente* and the *Hiron-delle*.¹ Some officers, by the early adoption of proper hygienic measures, particularly of those calculated to insure cleanliness and prevent the accumulation of sources of impure exhalations, have succeeded in guarding their vessels, under the most unpromising circumstances, against the development of fever. While other vessels were sorely visited by the disease, Capt. Smith, of the British navy, never had it in those under his command during long service in the Mediterranean. Like him, many others have preserved their crews by having "the holds of their vessels washed out daily by means of plugs till the water came out perfectly clear, so that an accumulation of filth could not take place."²

More than a century ago, Desportes expressed himself in favour of the opinion that yellow fever arises often from the foul condition of ships, and attributed the escape of a vessel, the *Jason*, of seventy-four guns, in 1746, at St. Domingo, to its extremely leaky condition, by which the sources of exhalations in the hold were submerged.³

The common saying of sailors, that a leaky ship is ever a healthy ship, is well known; and, in conformity with the result of experience on that point, the submerging or sinking of infected vessels has not unfrequently been resorted to as a means of purification.

¹ Lefort, Réponse à M. Kéraudren, p. 22.

² London Med. Gaz., ix. 800.

³ Mal. de St. Domingue, i. 162.

I have already referred to the history of the fever of Gallipolis in 1795. It arose from the infectious effluvia issuing from a mass of decomposing materials situated in a large pond near the cantonment. "As some decisive measures became necessary," Major Prior states, "to save the remainder of the troops, I first thought of changing my quarters; but as the station was in every respect more eligible than any other, and had been made so by much labour and expense, I determined to try the experiment of changing the condition of the pond from which the disease was believed to have arisen. A ditch was accordingly cut, what little water remained was conveyed off, and the whole surface covered with fresh earth. The effects of the scheme were soon obvious. Not a man was seized with the worst form of the fever after the work was finished, and the sick were not a little benefited."¹

In 1819, as we are told by Dr. Hort, of New Orleans, the yellow fever carried off a whole family at Bayou Sara. The disease was traced to a quantity of putrid bacon exposed in a cask in the back yard. "Prior to this family being attacked with the disease, and destroyed, no case of yellow fever had occurred there; nor did any other cases occur, for the cause was immediately removed."²

A writer of distinction, in his own country particularly, M. Kéraudren, who, from the high position he occupies in the French navy, has had ample opportunities of ascertaining the effects of expurgation in arresting the progress of the yellow fever in infected vessels, admits the fact as one well known and fully attested; but, faithful to his belief in the contagion of the disease, he regards such effects as lending support to, instead of opposing, his views. In such cases, he remarks, the causes of the yellow fever are inherent in the sickly vessels. To purify these, they are dismantled, and emptied of all their contents; then they are washed, rubbed, dried by means of heat, ventilated, fumigated, and finally whitewashed. After this operation, these vessels are again armed. They are sent out on a cruise, or to France, and the fever does not reappear in them. "Hence, as I have said," he continues, "the disease arose from causes inherent in the vessels, and they have disappeared at the same time. The success of these purifications, which are effected by the very means employed for the removal of contagious complaints, indicate sufficiently the nature of the disease they have succeeded in destroying."³ To such a conclusion, however, we can scarcely reach; for several of those means, and others of like import, are equally effective in arresting the progress or preventing the occurrence of ordinary malarial fevers, the contagious properties and exotic origin of which even M. Kéraudren himself would not venture to uphold.

It is to be remarked, also, that, whatever may be thought of the conclusion drawn from the supposed presence and retention of a contagious poison in the hold of a sickly ship, and of the assertion that the effects of the expur-

¹ Potter on Contagion, pp. 15, 16.

² New Orleans Med. and Surg. Journ., ii. 6.

³ De la Fièvre Jaune, Obs. aux Antilles, &c., pp. 45, 46.

gating agents employed to arrest the progress of the disease show that the poison thus existing is of the nature contended for, sure it is that the same will not apply to those instances in which like results have been obtained on land—as, for example, at Gallipolis; for in these the cause of the disease was traced to a locality where no contagious poison could have been deposited; where no such poison could have remained for weeks, attacking those who were placed within reach of its baneful influence; and where it was extinguished by draining the miry surface and covering the decomposing materials left with earth. The poison destroyed evidently emanated from such materials, and consisted of the effluvia resulting from their decomposition. It could in no way be ranked among those of a contagious character, yet it produced the fever in question. And if the latter can be the offspring of a poison arising from materials of the kind on land, we may readily perceive the impropriety of concluding, when the destruction of the fever is effected in a ship by the means mentioned, that the contagious character of the disease is thereby fully established.

But admitting, for the sake of argument, that the effects referred to do not lend support to the doctrine of non-contagion—which, of course, I do not—it is impossible to refuse our assent to the fact that they go far to establish the local origin of the fever, especially when we bear in mind the sameness, just alluded to, of many of the means employed with those found successful in common autumnal fever, the local origin of which is placed beyond the possibility of doubt.

On the beneficial effects of complete drainage I need not dwell. They have been demonstrated on innumerable occasions, and in another work¹ I have collected a number of interesting and important facts in point. Nor are instances of the successful results obtained from covering sickly places with water difficult to find (p. 260). We have the authority of Pringle for the fact that, during the campaign of 1748, in Brabant, the country bordering on the lower part of the Maas, was rendered more unhealthy upon letting off part of the water by which the country around the fortified towns had been submerged. It may now be proper to add, from the same author, that the “States of Holland, being made sensible of the sickness which raged at Breda, and in the neighbouring villages, gave orders to let in the water again, and to keep it up till winter” (pp. 61, 62). Indeed, experience taught Pringle that, “as to cantonments in marshy grounds, if the troops must remain there in the dangerous season, it will be better to float the fields entirely than to leave them half dry” (p. 98). The same process was resorted to some years ago, with complete success, at Paris, Bordeaux, and other parts of France, and is never neglected there when marshy surfaces cannot be otherwise reclaimed.²

Indeed, the beneficial effects obtained from the flooding of marshes or in-

¹ On Pneumonia and Malaria, p. 244, &c.

² Parent du Chatelet, *An. d'Hyg.*, xi. 310; Fleuriau de Bellevue, *Comptes Rendus de l'Acad. des Sci.*, xxv. 338, 339.

salubrious surfaces, by artificial means, by freshets, or otherwise, have been noticed from time immemorial, as well in foreign countries as in our own.¹

The practice is noticed as extremely useful, under peculiar circumstances, by Lancisi, who was too clear an observer not to have discovered that marshes are "harmless when plentifully diluted and cleansed throughout by pure and fresh supplies of water." Lancisi reminds us, too, of a fact mentioned centuries ago, by Strabo,² and which has been already briefly alluded to: "Alexandria, in Egypt, stands near the marshes; and although it ought to experience, during the scorching heat of summer, a close and suffocating air, yet, by the seasonable rise of the Nile, nothing filthy exists whence vitiated exhalations can arise, especially when the Etesian winds begin to blow from the north, and arrive after sweeping a wide tract of sea."

The following fact, already adverted to, illustrates more than one of the results here mentioned: "It has been remarked by persons who live in the vicinity of Morne Fortuné (St. Lucia), that when the military, who inhabit the Morne, suffer severely from sickness, the inhabitants of the town of Castrie (in the close vicinity) are generally in good health; and, on the contrary, that when the inhabitants of the town are sickly, the garrison on the height is comparatively healthy. The Morne is a bog in wet weather; Castrie is then an inundated swamp: in continued dry weather, the Morne has a hard and firm surface; Castrie is then a swamp advancing to exsiccation. The fact is obvious, and presents itself as a cause of what takes place."³ All these facts, as also the stoppage of febrile epidemics by a profuse fall of rain, are, besides many more referred to in preceding chapters, attested by the most reliable authorities.⁴

To the same effect may be cited the beneficial results obtained in France and elsewhere by the filling up of ditches and other excavations, remarkable examples of which are on record.⁵ I might dwell on the effects produced by the covering of the marshy margins of river shores by *sand* inundations, as observed on the borders of the Baltic, in Holland, Italy, France, Africa,⁶ &c., and particularly on the well-known case of the Goodwin Sands, in which, while from a similar cause the usefulness of the land was destroyed, the salubrity of the vicinity was firmly established.

¹ Carrière, loc. cit., p. 526; Ludlow, N. Y. Journ., ii. 84; Caldwell, Essay on Malaria, p. 75; Villermé, An. d'Hyg., ii. 349.

² Geographia, lib. xvii. p. 278.

³ R. Jackson, Sketch, ii. 358, 359.

⁴ Bally, p. 309; James Johnson, pp. 43, 320, 330; McWilliams, p. 184; Pritchett, p. 108; Boyle, pp. 3, 123; Dazille, p. 10; Desportes, i. 52, 80, 87; Firmin, pp. 3, 17; Gillespie, p. 137; H. McLean, p. 25; Rush, iv. 154; Ferguson, Recol., p. 199; Chisholm, i. 294; Ferguson, Med.-Chir. Trans., viii. 180, 181; Lempriere, i. 26; Rochoux, p. 11; Caillot, p. 121; Valentin, p. 89; Gouraud, p. 65; Arnold, p. 31; Furlong, Med-Chir. Rev., xxv. 289; Dickson, Edinb. Journ., xiii. 47; Bancroft, p. 200; Berthe, p. 156; Macculloch, pp. 204, 206; Brown, in Cyc. of Pract. Med., iii. 61; Cooke, Med. Rec., vii. 457; Pinckard, ii. 485, 486; U. Parsons, p. 204; Irvine, Dis. of Sicily, p. 6; R. Jackson, Sketch, ii. 259.

⁵ Monfalcon, p. 43; Macculloch, pp. 126, 127; Cycl. of Pract. Med., iii. 61.

⁶ Macculloch, p. 207.

With the same view I may, besides, appeal to those instances in which places heretofore insalubrious have been rendered otherwise by being thoroughly *washed*, through the agency of a freshet or an inundation, which carried off all substances susceptible of decomposition, and left in their stead a deposit of innocuous materials—as occurred in New Orleans, after the crevasses of May, 1816, and 1849,¹ and more recently in some parts of the State of Pennsylvania—nature doing on a large scale what we have seen is done with good effect on board of ship, in reference to the yellow fever. In the latter instance, prior to September, 1850, intermittent fever prevailed to a great extent along the course of the Schuylkill, and was found, in many instances, to be unmanageable, showing a tendency to a frequent recurrence. But, since the flood which took place at the time mentioned, the same localities have been remarkably free from it.²

We learn from Vitruvius that, in the lagunes of Venice, the air was extremely pure, especially around Ravenna, Altino, and Aquileia, the three principal, and doubtless the most populous, points. He himself affords us the explanation of this anomaly. At that remote period the marshes and ponds were situated to the north and east of those districts, and their bottoms were higher than the level of the sea; owing to which the latter, at every ebbing of the tide, carried away all the putrescent materials and filth, and thereby cleansed the lagunes.

In Seringapatam, materials of putrefaction, for about eight months of the year, lie all over the banks of water streams and other repositories, “until the periodical rains of Malabar begin, which, falling in the Ghauts, run down and fill the Cauvery River. The filling of this river is always very sudden, and it comes rushing along with great impetuosity, sweeps out all the filth from the ditches, clears away all the impurities so long stagnant on the island, and leaves the place for a while tolerably healthy, and the air cool and refreshing.”³

The city of Avignon was inundated on the 30th of October and the 4th of November, by a rise of the Rhone. Nine-tenths of the city were under water. No fever, however, followed, owing to the complete washing which the surface underwent, and the supervention of the cool north winds, which wafted the morbid exhalations along the great valley of the Rhone out to sea.⁴

¹ Transactions of the Pennsylvania State Medical Society, ii. 34; see also i. 24.

² Nicol, Obs. on the Nature of the Climate of Seringapatam, Edinb. Journ., xi. 290; J. Johnson on Trop. Clim., p. 101.

³ J. Johnson on Trop. Clim., p. 101; Nicol, Obs. on the Climate, &c., of Seringapatam, Edinb. Journ., xi. 290.

⁴ Gouraud, Fièvres Interm. Pernicieuses, pp. 133, 134.

CHAPTER XXIV.

PROOFS OF NON-CONTAGION—CONTINUED.

Not communicated to neighbouring Towns, Cities, or Plantations.—By those who, believing in the contagious character of the yellow fever, maintain that the disease has been transmitted to the open country around this and other places visited by an epidemic, or from one part of them to another by the sick or their effects, it is, as a matter of course, thought to have been propagated to villages, towns, and cities, at a greater or less distance. How far the accounts recorded in support of the first of these points can be trusted, has already been shown. As regards the second, it will not be difficult to discover, that though the yellow fever, if really contagious, ought to be more easily propagated in the less pure air, and amid the denser population of towns and cities, than in open country situations, facts will not bear out the advocates of the doctrine of the transmissibility of the disease, in their views respecting the result in question.

So far, indeed, from this, it is not hazarding too much to affirm, at least as regards this city, that the yellow fever has not been propagated to other towns or cities by the removal thence of persons labouring under it, or by their effects; that this has been the case not only where this removal has occurred to places where, from the nature of the localities, or other circumstances, the fever has never prevailed, and will, in all probability, never prevail; but to localities where it has, at times, spread to a greater or less extent, and with more or less malignancy; and, on the other hand, that the introduction of persons affected with the disease into this city has not been followed by its propagation, though no precautionary measures were taken at the time to prevent intercourse between the sick and the well. It may be shown, in addition, that the result observed in other cities of the United States has been of a kind to confirm fully the experience obtained here; that observations of like nature have been made in Europe and in the West Indies; and finally, that many facts and reasons may be adduced to render very doubtful the validity of the statements brought forward in support of the transmissibility in question, whether in temperate or in tropical climates.

It is conceded by contagionists themselves, that, during the epidemics which prevailed in Philadelphia at various times from 1699 to 1854, individuals who removed to neighbouring or distant villages, towns, or cities, while labouring under the fever, or who, having imbibed the seeds of the infection in the former, were attacked and went through the disease in the latter, did not communicate it to those around them, or to any of the inhabitants.¹

¹ In Carey's account of the fever of 1793, we find satisfactory proofs of this fact. This intelligent chronicler, who, as already stated, was a zealous contagionist, remarks that,

In 1798, twenty-six deaths occurred at Chew's Landing, a small village of New Jersey, about ten miles from the city. The same number took place at Cooper's Ferry, opposite the city. Many cases occurred at Trenton, Lambertton, Bristol, Frankford, Norristown, and Reading—and several of these ended fatally;¹ but in none of these instances have we any evidence of the disease having been communicated.

That the disease has been introduced into the city by the arrival from abroad of individuals labouring under it, and that this introduction has not been followed by any ill consequences, is shown by the fact that in the year 1818, two cases were admitted into our almshouse hospital from an Havana vessel. They went through the disease, and died in the crowded wards of that establishment. But, with them the disease ended. The same result obtained in September, 1841, when a patient was admitted into the fever ward of the Pennsylvania Hospital, and there died and was dissected without communicating the disease to his attendants or physicians.² Cases in good number came to us in 1854 from Savannah—some by land, others by sea. In several of them the disease broke out after their arrival—in others, during the passage. They were received and treated—they recovered or died—in private houses, in public hotels, and in the hospital; but in no one instance was the disease communicated to friends, nurses, physicians, or servants.

in defiance of all the resolutions entered into by the inhabitants of various towns, many of our infected citizens evaded their vigilance and took refuge among them; and he confesses that, in only one or two cases is it known they communicated the infection. But how far these exceptions can be relied upon we shall see. "Three persons from Philadelphia," he proceeds to say, "died with this disorder in one house at Woodbury, in New Jersey; they had been attended during their illness by the family, none of whom caught the infection. Six or seven died at Darby, as many at Germantown, and eight at Had-donfield, without communicating it to any of the inhabitants. A man from Philadelphia, of the name of Cornell, died in New York, about two days after his arrival. The place of his death was a boarding-house, in which were a number of boarders, one of whom slept in the same bed with him. Two of the family only were slightly affected; but not in such a degree as to require medical aid. Three other infected persons from our city, who, when discovered, were removed to Governor's Island, died there, and no one took the disorder from them. A man died at one of the principal taverns in Baltimore of the same disorder. Many people had visited and attended him during the whole of his illness without injury. No person was affected but his doctor, whose indisposition was not of long continuance. A great number of similar instances have occurred at Burlington, Bordentown, Lambertton, Princeton, Brunswick, Woodbridge, Newark, Lancaster, and various other places, and in no case, except those at Baltimore and New York, has the infection spread" (pp. 80, 81). The reader who is aware of the dangerous character and great malignancy of the yellow fever, will feel little disposed to regard as instances of the disease cases of so slight a kind as to require no medical treatment; nor will he recognize a fair case of the fever in the indisposition of the physician in Baltimore, which is acknowledged to have been of short continuance, and slight; and he will the more readily doubt the propagation of the disease in that case, because, had such a propagation occurred, it would follow that the contagion seized one who only occasionally visited the patient, while it spared those by whom the latter was attended.

¹ Currie, *Fever of 1798*, p. 34; Condie and Folwell, p. 97.

² Stewardson, *American Journ.*, Jan. 1842, N. S., iii. 91.

Much reliance has been placed, as we have seen, on the events which occurred in Baltimore in 1793, at Germantown in 1798, as well as at Wilmington, Marcus Hook, and Chester, to prove the propagation of the fever from Philadelphia to neighbouring towns, and, as a necessary consequence, its contagious character. With respect to the first, enough has already been said. The instance of Germantown deserves a more serious notice, from the high authority of the individual by whom it was recorded—the late Prof. Wistar—and from the fact of its having been adduced by Dr. Chisholm,¹ Dr. Hosack, Dr. Girardin, and others, as well as by the College of Physicians of this city, as an adequate example of the contagious character of the disease. In the first place, it may be mentioned that none of the cases which occurred in Germantown came under the immediate observation of Dr. Wistar.² His statement was based on facts collected seven years after the event by Dr. Heister, who, at that time, had just commenced his medical studies, and whom he sent to investigate the subject and collect information—not, as it would appear, from the physicians of the place, but from the families that had suffered. From this circumstance—the incompetency of Dr. Heister to decide, at that time, on matters of the kind—and the imperfect description we have of the cases, we would be justified in doubting the fact of their being true yellow fever, had we no other authority on which to form an opinion on the subject. But granting they were of the nature contended for, we may, from the details given by Dr. Wistar, and the information since collected from the physicians of the place, reject all idea of the propagation of the disease to that village.

The first case was that of Betsy Johnson, who had spent some days in Philadelphia. On the 27th of August, fourteen days after the death of Miss Johnson, Mrs. Duy was attacked with a disease *supposed*, as said Dr. Wistar, to have been of same kind; and, on the 30th—or seventeen days after the same event—Mrs. Hubbs was likewise seized. They both died. Mr. Duy was taken eighteen days after the death of his wife, sixteen after that of Mrs. Hubbs, and thirty-two after that of Miss Johnson. From these long intervals between the cases mentioned, we certainly find reason to doubt the communication of the disease from the first attacked to those that succeeded. But this is not all. We are told that the fever did not reappear in the Johnson family until the 17th of September, or thirty-five days after the death of Betsy; and yet the members of that family, who continued to the last to nurse and attend on her, would, in all probability, had she been the focus of contagion, have been attacked before strangers, who, if they were exposed at all, must have been so but slightly and temporarily.

In addition, it may be stated, that the only ten persons who, according to Dr. Wistar, took the disease from Miss Johnson, occupied three houses located within an area of eighty yards, in a comparatively low situation, and near which was a pond of stagnant and putrid water, which, in hot weather,

¹ Letter to Dr. Haygarth, pp. 177–8.

² This is stated by Dr. H. in a letter to Dr. Jackson, who communicated it to Dr. Chervin.

emitted towards morning a highly offensive smell. It may also be mentioned, that a number of individuals from other parts of Germantown visited the patients with perfect impunity, the fever being strictly limited to residents of the small infected spot in question; and that Germantown was at the time crowded with fugitives from Philadelphia, many of whom became affected with the fever, and died or recovered without communicating it to any one. From Condie and Folwell we learn, that the number of such cases in and about Germantown amounted that season to fifty-eight, of whom thirty-three died. In Germantown proper, there were thirty-seven cases and twenty deaths; and the late Dr. Carter, who attended a large proportion of them, testified as to the fact of the disease not having been transmitted in a single instance from the sick to those by whom they were attended. Mr. Johnson, brother to Betsy, and himself a firm believer in contagion, stated to Dr. Chervin that, of the numerous cases that came to Germantown from Philadelphia, from 1793 to 1820, none, except his sister, propagated it in that village. Dr. Bonsall, who was long engaged in the practice of medicine there, certified that he did not witness, during that period, a single case of contagion; and the late Dr. Betton states, in a document furnished to Dr. Chervin, that his father, who also practised medicine in Germantown in 1798, investigated the facts recorded by Dr. Wistar, and was perfectly satisfied that the disease, in every case, had not been communicated from Philadelphia, but had arisen from local causes.¹

When, with the facts before us, we discover that Dr. Rush, Dr. Caldwell, and others, take no notice of this supposed instance of propagation; that the former states explicitly, in his letter to Dr. Miller,² containing the avowal of his retraction, that the disease "has uniformly perished in the high and healthy village of Germantown," we will find cause to attach less importance to the outbreak of the fever in that village as an evidence of its contagiousness, than is done by the opponents of the domestic origin of the disease. Well may Dr. Bancroft³ remark, that if such cases had really occurred, and with such evidence as to render them credible, Dr. Rush, who must have heard of them, and who, at that time, believed that the disease might sometimes be contagious, would probably have been so far convinced in that belief as to have abstained from the retraction which he afterwards made of it, and especially have hazarded, in the very letter containing that retraction, the declaration I have alluded to. The fact is, the publication of Dr. Wistar proved satisfactory to none but such as were already decided contagionists; it made no convert to the declining cause of contagion, and was not instrumental in arresting or retarding the change then taking place in the views of the profession relative to the question of the origin and mode of propagation of the disease.

The visitations of fever at Wilmington (Del.), in 1798 and 1802, which, as

¹ See Chervin, *Arch. Générales*, xvii. 150; *Réponse aux Allégations de M. Girardin*, pp. 7, 8.

² *Works*, iv. 165.

³ *Essay on Yellow Fever*, p. 415.

may be seen in a former chapter, were adduced by Drs. Tilton and Monroe,¹ and the opponents of the domestic origin of the disease generally, as illustrations of the propagation of the latter from Philadelphia, where it then reigned epidemically, will be found, on close and accurate examination, as unsatisfactory as the outbreak in Germantown. Dr. Rush, who was well acquainted with the details of the events in that town at the various periods mentioned, states explicitly, in the letter just referred to, that the supposition of the disease having proved contagious there was perfectly destitute of foundation (iv. 165); and the several publications of Dr. Vaughan, supported by the certificate furnished by Dr. Didier to Dr. Chervin, will serve to convince every unprejudiced mind that the disease was not introduced from Philadelphia or elsewhere; that it was due to the operation of local causes, and that it manifested no contagious property. The contrary view rests mainly on assertion; and the accounts published by its supporters contain statements which must, to say the least, create doubts as to its correctness.

Thus, when we are told, by importationists themselves, that the first case that came from Philadelphia in 1798 communicated the fever to no one; when the only reason assigned in favour of their views is the fact that the fever became epidemic between the 15th and 20th of August—but a few days after a young man from that city had been attacked—and that shallop men, who passed from one place to the other, were the first victims; when we learn that infected household goods and furniture brought by the shallops had more influence in spreading the contagion than diseased persons; when, by the same authorities, it is affirmed that stronger exhalations arise from persons affected with this fever than in other febrile diseases—to such a degree, indeed, that many appeared to have been affected by the contagion at a distance from the sick reaching quite across the streets—but, at the same time, that in no instance did those who fled to the country communicate it to others; when, with all this before us, we bear in mind that the state of the localities where the fever appeared and prevailed was such as to favour the development of fever, and that the temperature was truly tropical; and when, besides, we recollect that Dr. Vaughan found sufficient reason, as the result of his experience, to discard the doctrine of contagion, which he had originally upheld, and took pains to refute the conclusion of the advocates of importation²—we can have but little hesitation in refusing our assent to the opinion of the few who contend that the yellow fever was carried from Philadelphia to Wilmington, and there spread by contagion.³

¹ Tilton, letter to Dr. Currie, Feb. of 1798, p. 138; Ibid., letter to Dr. Miller, *Reposit.*, iii. 128; Monroe, letter to Dr. Miller, *ib.*, iii. 136.

² Vaughan, letter to Dr. Miller on the fever of 1798 in Wilmington, *Reposit.*, iii. 368; *Ibid.*, Feb. of 1802, p. 19.

³ 1802.—1. “Ann Davidson, the only person who was or could be suspected of introducing contagion among us, came from Philadelphia, in the beginning of August, to her father’s house, in King Street, and was reported to be affected with contagious fever on the 15th. She recovered without suffering any of the more violent symptoms of malignant disease, and removed into the country. 2. The family in which she resided consisted of ten persons, all of whom remained well until the 7th of September, when her mother

The history of the yellow fever at Chester, in 1798, as given by Dr. Currie,¹ is most unsatisfactory when adduced as an example of the propagation of that disease by personal contagion; for it amounts to this, and nothing more, that a lad who had slept two nights on board a shallop employed, a short time before, in trading to Philadelphia, died with black vomit and yellow suffusion on the 31st of July. Another lad, who had likewise slept on board the same shallop, was taken ill, but recovered. Seven days after the decease of the first, his mother and brother were both attacked with fever, of which the latter died on the eleventh day. From this time other cases occurred among the people of the vicinity, some only of whom had visited the patients; and after the 1st of September the disease spread in every part of the village, and destroyed, before it ceased, all but five or six out of fifty persons that were attacked. We are not told, however, that there was any one on board the shallop ill with the fever, and capable of communicating it to the boys above mentioned. None of the crew are said to have suffered at any time from the disease; and from this circumstance, added to a consideration of the unfavourable position of Chester in point of salubrity; to the fact that the locality affected lay near the margin of the river Delaware and along the decayed wharf, and was such as generally promotes the development, under conditions of atmosphere such as existed then, of malignant fever; that the march of the epidemic was similar to that observed in diseases arising from local causes; that an epidemic influence was prevailing over a large portion of the country, and the fever sprang up spontaneously in several localities; and, finally, that the importation of the fever on that occasion was contradicted by the highest medical authorities of the time,² we can run no risk in expressing the opinion that the example is not one from which the contagionists can derive much support to their views.

Of the fever which occurred, in 1798, at Marcus Hook, a small town situated on the Delaware, about twenty miles south of Philadelphia, we know but little. It is mentioned by Currie (p. 135), Condie and Folwell (p. 95), and more fully by Dr. Sayres, in a letter to Dr. Currie;³ all of whom adduce it as an example of the propagation of the disease. Dr. Currie contents himself with stating (p. 135) that the disease *appears* to have been introduced by infected persons from Philadelphia. In another place (p. 102) he tells us

had an attack of fever, after a journey of ten miles into the country, performed partly on foot and partly in an open cart. 3. In the mean time, T. Musgrove's son, Ann Hadley, Capt. West's girl, Mr. Cloud, and Hannah Robinson, in the same square, and R. Hagin's son, J. Warner's girl, E. Dale, and others, in different directions, were attacked with malignant fever. 4. If Ann Davidson's disease had been contagious, and the only source of future disease, is it not reasonable to suppose that some one or more of the ten persons confined in a small house with her would have been the first affected by the contagion?"*

¹ Fever of 1798, pp. 134, 135.

² Rush, iv. 165.

³ See Currie, Fever of 1798, p. 136; Condie and Folwell, p. 96.

* Vaughan, Fever of 1802, pp. 15, 16. See also Med. Reposit., vi. 299, 300.

that two persons from Philadelphia were attacked with the fever and died soon after, that five of the family with whom they resided were attacked in succession, and that every one of them died. Condie and Folwell speak of that introduction as a matter of fact; but Dr. Sayres, entering more into details, gives a somewhat different version of the rise of the disease. The first case that came under his notice was on the 2d of August. This was a shallop man, who came from Philadelphia four days previous to the attack. He died on the sixth day of the disease. The next five cases were persons who were but a few days from Philadelphia—from two to eight days. The disease appeared to increase considerably by the 20th of that month. The greater part of those who were lately from Philadelphia died between the second and sixth day of their attack. The inhabitants of the place, who had the disease, and who had not been to the city, recovered in greater proportion. “The disease,” Dr. Sayres adds, “appeared in a number of cases, evidently communicated by contagion—in some cases from the sick, and several cases from the bed linen, &c. Though, in a considerable number of cases which speedily terminated fatally, I was not able to trace the smallest vestige of contagion being previously applied or received.”

In this instance, again, it is difficult to find anything to lend support to the opinion of the transmissibility of the fever. In the first place, we must bear in mind, that at that time all febrile attacks accompanied with yellow suffusion of the eyes and skin, and characterized by symptoms of malignancy were regarded as cases of true yellow fever. Let the reader recollect, also, that Marcus Hook, like many other places along the margin of the river is placed in the midst of localities which, under the influence of particular meteorological conditions, are prolific of fever; and let him recollect, besides, that among those who had not been to Philadelphia, the disease was much less violent in character; and he will, perhaps, be disposed to doubt, that such individuals laboured under the disease then prevailing in that city. But be this as it may, the fact, that shallop-men who had recently visited the city, and fugitives from the same were among the first attacked, cannot be regarded, without positive proof to that effect, as having communicated the fever to the rest of the community; while the transmissibility of the disease is rendered more than doubtful by the admission that in a considerable number of the most formidable cases, no vestige of contagion could be discovered. If, in such cases—probably the most numerous—the disease could arise from causes distinct from contagion, we cannot, in fair logic, attribute the others to the latter, merely because they communicated directly or indirectly with the sick, inasmuch as they were exposed to the influence of the same agencies which produced the disease in those in whom no “vestige of contagion” was traced, and would, in all probability, like these have sickened, had they not communicated with the sick.

Dr. Miller (*Works*, p. 108), informs us that during the epidemic of New York in 1805, many persons who had contracted the disease in that city, died of it in Boston, Albany, and other cities at a distance; many likewise at Greenwich, Brooklyn, and other villages in the neighbourhood. In no in-

stance, did these victims of the epidemic communicate contagion. Eight cases occurred at Newport in 1798, all of whom took it at New York. Five of them ended fatally; but the disease did not extend.¹

Dr. J. B. Beck (in the Appendix to his edition of Gregory's *Practice of Medicine*, i. 653), informs us that, during the prevalence of the epidemic of New York in 1822, six persons lay sick with it at Greenwich, a village about a mile from the city. All of these had contracted the disease in the infected district; five of the six died there. In no instance did they communicate the disease to physicians, nurses, attendants, or friends.—To give additional weight to this fact, Dr. Beck remarks, it should be recollected that to this place, had been transferred the seat of business; that in consequence of this, almost all the merchants of the city had retired to it, and that to accommodate them, hundreds of temporary buildings had been erected. From this influx of inhabitants, it may naturally be inferred that the village was crowded to excess, from which circumstance, as well as the want of comfortable accommodations, everything seemed favourable to the propagation of the disease. Still, although a number of very decided cases of the fever were carried to this place, lay sick and died, it was not, in a single instance, communicated.

Dr. Beck further says, that in addition to the cases already recorded, there were a large number of persons who, after having contracted the seeds of the disease in the city, sickened after their removal into different and distant parts of the country. There occurred about thirty-six cases of this kind—three in Jersey City; one at Tappan (N. Y.); six at Bloomingdale; one at Albany; three at Middletown Point; four in different parts of New Jersey; one at Newton (L. I.); one at New Canaan (Conn.); two at Amboy (N. J.); one at Hempstead (L. I.); one at Westchester; one at Bloomfield (N. J.); one at Woodbridge (N. J.); one at Saugatuck (Conn.); two at Bushwick (L. I.); two at Elizabethtown (N. J.), and one at Boston. “Of these, there were twenty-seven deaths, being three-fourths of the whole number, or seventy-five per cent., proving the disease to have been of a very decided and malignant character; and yet, not in a single instance, was the disease communicated.”

As a counterpart to this, it may be mentioned that when the fever prevailed at Brooklyn in 1809,² it was strictly limited to that place, and was not propagated even to New York,³ which is situated on the opposite side of the river, and between which and Brooklyn, a constant intercourse is kept up; where patients from the infected spot were received,⁴ and where we may presume, notwithstanding all that Dr. Hosack has said, to account for the immunity consistently with his particular views, that the contagion, had it existed, would have spread more readily than it is said to have occasionally done in the purer air of the country.

¹ Currie, *Fever of 1798*, p. 79.

² Gillespie, *Med. and Philos. Reg.*, i. 101.

³ *Reposit.*, xiii. 199.

⁴ *Regist.*, i. 279; ii. 94; Hosack on *Contag.*, Works, i. 306.

In 1853, several cases reached New York from some places south. Two, in particular, died in the hospital, and, after death, were thoroughly dissected, but the disease was not communicated to any one. The same thing occurred later in the season. We know that, during our revolutionary war, fever prevailed to an unprecedented extent in the West Indies (as any one may learn from Dr. Hunter, &c.). Sir G. Blane informs us, that during that period a very large number of sick were landed at New York from the British fleets. Eleven sail of the line arrived there early in September, 1780, from the West Indies, and twenty-six sail of the line in the same month, two years after. From each of these fleets—no quarantine existing—a great number of sick, afflicted with malignant fevers, were sent to the hospitals. The same was done in the autumn of 1781; and yet, notwithstanding, the fever was not communicated to any one.

To examine in detail all the instances adduced in proof of the propagation of the yellow fever from the cities of this country to other cities, towns, or villages, more or less distant, would lead us much too far. Suffice it to say that not one of those instances has been satisfactorily made out, but that, on the other hand, numerous cases might be collected showing the perfect immunity attending the introduction, even in those very same places, of persons labouring under the disease. Witness the unsatisfactory nature of the reputed transmission of the fever from New Orleans to Madisonville, in 1818, as recorded by Dr. New in a document furnished by him to Dr. Chervin. The disease occurred in a tavern crowded with persons of the lower classes, and where several individuals labouring under the disease had been received from New Orleans. Some of the patients died; others recovered. *One month after*, when the town was entirely free from sick and convalescents, the keeper's family took the disease, and several of them died. It may be remarked that, in another tavern, frequented by the better classes and less crowded than the former, the disease was not propagated, though patients from New Orleans were there, in like manner, received.¹ The rest of the population remained perfectly free from the disease.

If Dr. Monette affirms, and Drs. Strobel, Carpenter, &c.,² repeat, that the fever was, at different times, introduced in all the towns or villages of the south and southwest referred to in a preceding chapter, the writings of Perlee, Merrill, Cartwright, Thomas, Barton, Dowler, Stone, Drake, and many others; the various reports of the Medical Society of New Orleans; the certificates furnished by Drs. Kissam and Osborn to Dr. Chervin,³ and numerous articles inserted in the volumes of the *New York Medical Repository*, and other professional periodicals, from the earliest times to the present, contain a mass of facts sufficient to overturn the fabric erected by the contagionists out of the instances of communication they adduce, or at least

¹ Report of the Acad., p. 15.

² Monette, pp. 64, 65, 66, 67; Strobel, pp. 117, 123, 130, 187; Carpenter, pp. 17, 18, 22, 23, 25, 26, 27, 28, 29.

³ Rapport de l'Académie, p. 25 (note).

to create a strong suspieion as to the accuracy of these, or of the deductions drawn from the circumstances eonnected with their occurrenee.

The following statement I derive from Dr. Dowler's aecount of the fever of New Orleans in 1853 (p. 53). The late Mexican war furnishes the most eomplete refutation of the eontagiousness of yellow fever in the absenee of quarantine, so far as negative evidence can go. If the United States Government had tried to devise an experiment, on a vast scale, to ascertain whether yellow fever eould be propagated by ships and armies, it eould not have achieved its purpose more effectually. In 1846-47-48, this malady existed in Tampico and Vera Cruz, and was very severe in New Orleans in 1847. The troops and the material of the army leaving New Orleans for Vera Cruz, and Vera Cruz for the interior of Mexico, did not suffer themselves from yellow fever, nor spread contagion through the towns and eountry. In 1848, thousands of the returning soldiers passed through Vera Cruz in June, where yellow fever existed, and on reaching New Orleans in July and August, a few died out of fifteen thousand who remained in the eity and its environs some time, without communicating any disease to the city by means of their goods, army materials, and selves. Thousands thus, without having been quarantined, remained in the city for a time and quitted it for their homes in other towns and places, without having communicated the disease to any one. After the reduction of Vera Cruz, yellow fever appeared, and many invalids and sick persons were sent to New Orleans and other places for treatment, in the transports which carried out the troops, yet they did not propagate the disease anywhere. Thus, at least fifty thousand experiments made in Tampico, Vera Cruz, and New Orleans, not to name other places, produced no proofs of personal or other kind of contagion, though in both the first-named plaees yellow fever prevailed moderately among residents not acclimatized.

Few plaees are more exposed to the influenee of yellow fever ships than the Balize, at the mouth of the Mississippi River. Dr. Drake well remarks, that whatever may be the origin of the disease, we should, *à priori*, expect to find it a great scourge of this little population; and yet it appears to be a rare visitor, and is not regarded with the least concern (ii. 212). It does not appear to have ever been epidemic there, while every one to whom Dr. Drake spoke there "favourably contrasted the Balize with New Orleans in reference to the fever." When introduced there, the disease has never spread; while, on several oeeasions, it has originated in the place. Plaquemine has often been visited by the yellow fever. But, on several occasions, when introduced there, the disease has afforded proofs of its non-transmissibility. Several examples of this are mentioned by Dr. Drake who eollected them on the spot. (*Ib.*, p. 249.)

A large portion of the fugitives from Natchez in 1823, sought an asylum in the town of Washington, six miles to the east, where they sickened and died in large numbers; but no one took the disease. This is stated positively by Dr. Cartwright, and Dr. Drake assured himself, while at Washington, of the truth of the fact. It must be remarked, that though there have often been

sporadic cases at Washington, the disease has only been once epidemic there. On that occasion, it was thought by Dr. Monette to have been introduced from Natchez. But the truth of this is far from having been proved, as every one may know, who will peruse Dr. Cartwright's essay in the ninth volume of the *Recorder*. Let it not be forgotten, that while Natchez has suffered several times, Washington has suffered but once, though persons fled from the former to the latter for protection.

At Memphis, which is within the yellow fever zone, the disease has prevailed, and has often been brought to that city, and proved fatal in the hotels and boarding-houses; but not propagated. This, Dr. Drake was informed of by a physician of the place, Dr. Wyatt Christian. In 1842, the steamer Louisiana stopped at the wharf for twenty-four hours, having sixteen yellow fever patients on board; and though the people of Memphis visited her throughout the whole time, no one contracted the disease.¹ In 1853, when the fever is said to have been introduced in one hundred places along the Mississippi and its tributaries around Mobile, &c., no quarantine regulations were adopted in Memphis; no special sanitary measures were instituted with the view of preventing an incursion of the epidemic. Boats freighted with merchandise saturated with the atmosphere of New Orleans, and other infected towns, landed at the wharf almost every day; a number of persons, labouring under the disease were carried through the streets to the hospital—many dying there, and some in private families, and yet no sickness was communicated to nurses, friends, or physicians, or to the public at large.²

In 1837, when the city of Natchez was laid waste by the devastating influence of the disease, numerous cases in every stage of the disease—from the initiatory symptoms to the black vomit—were landed at Vicksburg from boats, and ushered into crowded, filthy boarding-houses, with ten or a dozen in a room, and in not a single instance, as we are informed by Dr. Hicks, did a nurse or attendant become affected with the fever, though many of those landed died with black vomit. Such was the case in 1839. Dr. Hicks attended numerous cases that were landed from boats from Natchez, the city being then crowded with paupers, who were flying from disease and contagion, as they supposed, as is usual in our southern cities when an epidemic breaks out; but in no instance did he witness any disposition to contagion. In 1841, many citizens fled from Vicksburg when the epidemic was declared to exist. A large portion of them sickened at different points, and many died, all of whom had numerous attendants, and in no instance did any of their friends or nurses “sicken with the disease, notwithstanding they all died with the black vomit—the ripe stage of contagion—if contagion could be.”³

“Numerous cases have occurred,” says Dr. A. Smith, “of persons exposed in Houston and Galveston, who afterwards sickened and died of black vomit on the farms and plantations. In no instance did these give rise to yellow

¹ Drake, ii. 283.

² Tuck., N. O. Journ., x. 665; xi. 187; Merrill, Memphis Recorder, Sept. 1854, p. 95; Smith, N. O. Journ., x. 662.

³ New Orleans Med. and Surg. Journ., v. 224-5.

fever. My own observation has fully satisfied me that in Texas, yellow fever is in no degree communicable from one person to another, but that exposure in an infected district is an essential preliminary to the development of the disease."¹

The old Spanish and French village of Natchitoches is situated on Red River. "In the month of August, 1839, several persons from New Orleans had the fever in that town, but no case occurred in its own population, which amounted to about five hundred."² Dr. Lebby, in a letter to Dr. Strobel, states another strong fact. The mate of a lighter, an Englishman by birth, about eighteen years of age, who had been in the harbour about seven or eight months, slept one night in Charleston, in September, 1838. On the fourth day he sickened, and although he was among children at Sullivan's Island, and others who were liable to contract the disease, no instance of it occurred in the family; he recovered.³

In 1854, fever cases were received at Mobile without hinderance from Montgomery and other places, and yet the disease was not communicated by one of them. The disease, that season, existed to a slight extent, the number of deaths amounting to 38, and that of cases, it is supposed, to about 300; but all these arose independently of any possible imported contagion. Why was not Mobile scourged?⁴

At Portersville, on the lake between New Orleans and Mobile, several hundred people were assembled in the summer of 1854. Of these, about one hundred and fifty were collected in one inclosure. Among these, no case of yellow fever occurred, though five imported ones were brought in, nursed by different persons, and two died with black vomit. These facts Dr. Nott, from whom I quote, obtained from Dr. J. W. Moore, a very intelligent gentleman who lives at Portersville, and saw every case of sickness that occurred there. Dr. Nott attributes the exemption of the place from an epidemic extension of the disease to the circumstance of the New Orleans boats not touching there.⁵ Similar occurrences have been noted during the summer and autumn of 1853 at Concordia Parish, at Clinton, at Centreville, at Lake Providence, and at Biloxi Bay, Bladon Springs, Cahawba (Ala.), Black River, Gainesville, Portersville, and Trinity;⁶ in Thibodeauxville, Little Rock, Columbia, Sabine River, &c.;⁷ in all of which places individuals labouring under the fever have completely failed to propagate it.⁸

These facts should be sufficient to render very problematical the argument in favour of the contagiousness and transmissibility, or the latter alone, of the yellow fever, founded on the supposed propagation of the latter by the sick or otherwise, in places communicating with New Orleans or Mobile, or other infected cities and towns; for inasmuch as the disease was not propa-

¹ Trans. of Med. Assoc., vii. 535.

² Drake, ii. 245.

³ Strobel, p. 192.

⁴ Ketchum, Trans. of State of Alabama for 1855, p. 111.

⁵ New Orleans Journal, x. 579.

⁶ Barton's Rept., pp. 277-8.

⁷ Fenner, Trans. of Am. Med. Assoc., vii. 502-528.

⁸ Report of the Sanitary Commission of New Orleans, pp. 39-65.

gated in many localities within the fever zone, it is not likely to have been thus carried elsewhere. The fact of this propagation is the more to be doubted, and the inference drawn from it is the more evidently vitiated from the circumstance that during the very season when those proofs of transmissibility are said to have presented themselves, the disease is proved to have originated spontaneously in many towns, villages, and even plantations situated at no great distance from the former—at Fort Adams, New River, Bayou Sarah, St. Francisville, Lake Providence, Centreville, Natchitoches, Woodlawn, Selma, and Hollywood.¹ It had done so before at Plaquemine in 1845, Port Hudson in 1841, at Natchez in 1823—to say nothing of other periods of epidemics in that city²—at Galliopolis in 1796, &c., and on several plantations.³

Here, then, we have a failure of the propagation of the disease in many places through the instrumentality of the sick or merchandise. On the other hand, we have the spontaneous origin of the same disease in very many localities, and independently of the arrival of the sick or of infected boats. Where, then, let me ask, is the propriety of attributing the occurrence of the fever anywhere to circumstances which have failed completely to produce the effect in many instances, and which were as completely dispensed with in many others? The absence of connection, as cause and effect, between such arrivals and the appearance of the fever will be the more apparent to the careful inquirer when he bears in mind the kind of evidence on which most of such cases of communication rest, the nature of the circumstances adduced in proof of the after propagation of the disease, and the character and calling of some of the individuals whose testimony is appealed to. A steamboat, a railroad car, or stage arrives with an individual—sick or well—from some infected place. Some days, or even weeks after, another individual is taken ill with the disease, and dies or recovers—it matters not whether he has seen or approached the former, or kept aloof. It may be that in the first instance, the disease has shown itself in individuals who had a short time before opened boxes of goods, or had approached such goods. It spreads among those who have been near the sick, but as frequently among persons at a distance, or who have passed, or who have resided on the opposite side of the street; and forthwith a febrile complaint, which, in a thousand places, and under every variety of circumstance calculated to favour the diffusion of contagious diseases, has been proved over and over again to be incapable of communication, even to those who surround the sick, and eat, drink, and sleep with them, is supposed to have been introduced and propagated in the way mentioned. In some instances, the believer in such extraordinary instances of transmission can only adduce in support of his opinion the popular rumors of the place, which he not unfrequently gets at second hand. Sometimes that opinion is based on the *supposition* that the individual first attacked may

¹ Report of Sanit. Com. of New Orleans, pp. 16, 30, 33, 36, 38, 40, 47, 56, 61, 68, 109, 113, 129.

² Drake, ii. 249, 252, 267.

³ Fenner, pp. 509–13.

have visited a steamboat—sometimes on the *on dit* or recollection of a casual observer, male or female. In not a few instances, the chroniclers of such events are clergymen, shopkeepers, and village meehanics, whose avocations and mental trainings do not fit them for conducting investigations of this nature, and who, like people at large, almost invariably connect contagion with the spread of the disease; or physieians, who, judging from their reports, are not exactly the sort of men to whom we should look for correct conclusions in matters of the kind, or who are blinded by strong predilections in favour of eontagion. Would that such views were entertained only by individuals of the class mentioned. Unfortunately, they receive the sanetion of some professional men of respectable standing, from whom better things could have been expected, and who, as already remarked, are not over-fastidious as to the nature of the facts by which they have allowed themselves to be subjugated.

It is an admitted fact that, at each visitation of the fever at Gibraltar, many individuals took refuge at Tangiers and other towns of the opposite coast of Africa; and yet we do not find that in a single instance the disease was eommunicated to the inhabitants. Sir James Fellowes states that, during the height of the epidemie of 1800, Mr. Cardoza, the highly respected ehief of the Jews at Gibraltar, embarked with his wife and twelve other persons in a small vessel which sailed for the coast of Barbary. Having a foul bill of health, they were not permitted to land at Tetuan. After beating about for a considerable time (during which seven of the passengers died), the vessel passed the straits, and anchored in the Bay of Oran. The remaining passengers were landed, and the disorder, so far from spreading, disappeared entirely (p. 109, note). Berthe acknowledges that the disease could never take root at Aleala in 1800, though brought there on several oocations. Not less than twenty-four eases occurred there, and of these eighteen proved fatal; and yet the fever was not eommunicated to any one.¹ Bally and Arejula inform us that, in 1804, *notwithstanding its visibly pestilential character*, the fever spared several small villages in the neighbourhood of Malaga—such as Churiana, Alhaurinejo, Alhaurin, and Torre-del-Mar—where there were no other patients than the fugitives, or those who went to Malaga and were imprudent enough to sleep there.² The fact of this non-transmissibility is acknowledged by the French eommission sent to Spain in 1805; and the reader who investigates the subjeet will find that the many reports of the propagation of the fever from Bareelona to the neighbouring towns and villages have, without one single exception, been disproved by Chervin,³ O'Halloran (p. 70, &c.), C. Maelean,⁴ Costa (p. 24, &c.), Fermon (p. 19, &c.), and Piguilem.⁵

Dr. Doughty relates the following fact: In eonsequence of the blockade of Cadiz by the French, in 1810, it was neecessary to convey all supplies for the sustenanec of the combined forees stationed at the Isla, amounting to more than

¹ See Berthe, pp. 157, 158; Deveze, p. 226; Bancroft, p. 314.

² Bally, p. 87; Arejula, p. 215; Fellowes, p. 181.

³ Rep. of the Acad., p. 80; Fièvre Jaune d'Espagne, p. 96, &c.

⁴ Evils of Quar., p. 122.

⁵ Ann. de la Méd. Physiol., i. 104, &c.

15,000 men, from this city ; also those for the inhabitants, consisting of near 10,000 people. This required a daily and constant communication between the two places. Not only the muleteers and others employed in the transport of provisions, &c., but soldiers and inhabitants were constantly passing and repassing. No cordon of troops was stationed to cut off the communication, and for a very obvious reason, because such an interruption of intercourse at that time would have led to starvation of the Isla ; and its being of necessity allowed to continue was attended with no bad consequences to this latter place, as to the dissemination of the principles of contagion. This Dr. D. regards as an additional proof that the fever of Cadiz was divested of that power, as it was, notwithstanding the constant and daily intercourse, never propagated to the Isla during the entire duration of the epidemic (p. 196).

Pariset, the great apostle of contagion in France, himself gives us proofs of this non-transmissibility of the fever through personal contagion. A number of persons labouring under the disease having left Seville, where the fever was, as it was thought, highly contagious, went over to Cordova, Carmona, Ecija, and Alcala ; but, though many of them there died, nothing was heard of contagion. Pariset was told by Dr. Ramon that while, in 1804, the yellow fever was spreading extensively, *by contagion*, at Carthagen and Alicant, a number of persons from those places arrived at Murcia with the disease ; but here again, though the city was filthy, they did not communicate the fever to the inhabitants, with whom they kept up a constant intercourse. The same thing occurred at Alberga, a village situated near Murcia. Alicant was ravaged by the *contagion* in 1804 ; but though, in 1811, patients were admitted there, and died with the black vomit, no one was infected by them.¹

We are informed that although, in 1804, the commerce of Leghorn continued uninterrupted, and the French garrison (several of whom had contracted the disease) removed to Pisa when the epidemic was at its height, and eight thousand of the inhabitants emigrated to that city and the neighbouring country, and some there died of the disease, yet the latter was in no instance communicated to those around.² The same fact, as to the non-transmissibility of the fever, not only at Pisa, but at Calci and other towns, is attested in a report furnished to the Queen of Etruria by all the consuls residing in Leghorn.³

In the autumn of 1853, the steamer Plata arrived in Southampton (Eng.) from the West Indies, having had cases of yellow fever on board. Mr. Napier, an engineer, was landed from the vessel, and eight days after the Plata was released from quarantine had a decided attack of yellow fever, which did not spread. Mr. Wiblin, in his account of the cases which occurred in the ship, states his belief that a solitary case of this kind not causing a spread carries no weight ; and that "if a cannon-ball had been fired against the town, and done no harm to life or property, it might as well be affirmed that

¹ Observ. sur la F. J. de Cadiz en 1819, pp. 79, 80.

² See Rev. of Palloni, in Edinb. Journ., ii. 90.

³ Inserted in full in Devezze, p. 231.

the common belief in the destructiveness of such projectiles is fallacious, as to say that a case of a disease occurring and not spreading proves that it is not contagious."¹

In his discourse on the plague (p. 71), Dr. Pye states, on the authority of a Mr. Fox, a merchant of London, that during his residence in the island of Teneriffe, a plague (which we may presume to have been the yellow fever) raged with great violence at Santa Cruz, in that island. "The place he resided at was the City de la Laguna, about three or four miles from Santa Cruz; and though a great many diseased persons were brought thither, some of whom, too, died there, and though the people of La Laguna visited and conversed with those sick persons, yet none were hurt thereby, nor was there a single instance of any person having the disease who had not been at Santa Cruz, the place where the sickness raged."² The same immunity from the disease was experienced at La Laguna during the epidemic which prevailed at Santa Cruz, Puerto de la Orotava, and Teneriffe, in 1811. On this, as on the former occasion, the sick from those places were received and nursed at the former with perfect impunity.³

To notice in detail all the examples that could be collected of the non-transportation of the yellow fever of tropical climates, even in towns and villages subject at times to its severe visitations, and where everything would conspire to spread the contagion, if it existed, would be an endless work. Let the following suffice: In 1838, the governor of Martinique proceeded from the town of Fort Royal to that of St. Pierre, where the yellow fever prevailed, and took with him the musicians of the regiment stationed there. These, to the number of seven, were lodged in the barracks. Eight or ten days after their return to Fort Royal, they were all attacked with the disease, of which five died. But though no measures whatever were taken, either in the barracks or in the hospital, to prevent those men from communicating with others, the fever did not extend, and did not make its appearance until the following March, when it broke out in the city and among the troops.⁴ In alluding to this fact, Mr. Luzeau, chief physician of the Hospital of Fort Royal, states, in a report to government, that the number of cases received into that establishment, including the above-mentioned musicians, amounted to twenty-seven—all of whom had taken the disease at St. Pierre.⁵

The *Tigris* and *Childers*, ships-of-war, imported at St. Vincent distinct and well-marked cases of yellow fever from Point à Pitre, on the evacuation of Guadaloupe, without communicating the disease to any one.⁶ The *Regalia*, transport, arrived at Barbadoes in August, 1815, with the yellow fever on board. She was put under no restraint or quarantine, but communicated freely with the seaports of Barbadoes, the Saints, Antigua, and Guadaloupe, landing

¹ Cummins, *Lancet*, July, 1853, p. 51.

² Quoted from Maclean, i. 349.

⁴ Rufz, pp. 28, 29.

⁶ Ferguson, *Med.-Chir. Trans.*, viii. 143; Bancroft, *Seq.*, pp. 218, 219, 228.

³ Valentin, *Journ. Univ.*, xi. 138, 139.

⁵ Cited by Chervin, *ib.*, p. 98.

the severally ill or dying subjects of that disease amongst the inhabitants and at the hospitals at Barbadoes and Antigua, without communicating any infection at either of those places.¹ While, as we have seen, almost every fresh man received on board of the Rattlesnake was attacked by the fever, the sick that were landed at Port Royal did not communicate it to any one there. We have seen, also, that the same absence of communication of the disease attended the landing of the sick from the Dasher and Pyramus at Antigua.

During the epidemic which prevailed in Rio Janeiro in 1850, several of the inhabitants left that city and went to Petropolis and Tijuca, hoping thus to escape the fever. However, in a few days after arrival, many of them were attacked in both places; but there was no spread, by contagion or otherwise, in either place.² Dr. Croker Pennell, in his account of that epidemic, states that of the number attacked at Petropolis eleven died. Similar facts were observed in Nova Friburgo, Constancia, and Paqueta. The fugitives in these places stayed and died in the hotels, full of people, who all had the greatest proclivity for the yellow fever. "The disease thus existed at Petropolis with sufficient violence to destroy a large proportional number of lives; for it is believed that the eleven deaths formed a high rate of mortality, compared with the number attacked. It is probable that at least seven persons held communication with each individual who died, either as attendants, nurses, or in washing and burying him; so that we are thus presented with an instance of from seventy to eighty persons exposed to a virulent infection, if such it be, and, singular to relate, not one contracted the disease. Not a single resident of Petropolis caught the fever. The number (eighty) who communicated with the sick is evidently too small, inasmuch as there were many more ill, who escaped, and who also had attendants, &c. If the disease only spread by its infectious properties, it is unaccountable why almost every one exposed to this infection in Rio should catch it, and not one, out of eighty or more, at Petropolis."³

Two or three persons died in Mahaica, a village twenty-five miles from Georgetown (Demerara), at different periods of the epidemic of 1837-'45. All those persons, it was proved, had visited Water Street, and transacted business there. But the disease did not spread in Mahaica by this contact,

¹ Ferguson, viii. 114.

² Cummins, London Lancet, July, 1853, p. 51, Am. ed.

³ A short Report upon Yellow Fever, as it appeared in Brazil during the Summer of 1849-1850, p. 14; Rep. of Sanit. Commiss. of New Orleans in 1853, p. 157; Ib., Lallemant, Observacoes acerca da Epidemica, &c., p. 35.

"A febre amarella por varias vezes foi introduzida na colonia de Petropolis pelos fugitivos da cidade; estes fugitivos moravão e morrêrão no meio de hoteis cheios de gente que todos tinham a maior propensão para a febre amarella, mas nenhum caso houve de transplantação da febre para estes hoteis. E os colonos convalescentes chegados da cidade, e até com a roupa da cidade e a febre ainda no corpo, vivendo no meio das casas cujos habitantes todos tinham a maior inclinação para a febre, todos estes colonos exercêrão uma influencia absolutamente nulla sobre a colonia. Nenhum Petropolitano que nunca chegasse à capital adoeceu; a doença, julgada no Rio summamente contagiosa, não era contagiosa no meio de mil homens, que todos estavam summamente predispostos para o contagio."—*Lallemant*, p. 36.

“nor did it make its appearance in any of the villages or plantations throughout the colony, although a free intercourse between them and the infected locality was constantly kept up, excepting in persons who had visited Water Street, and carried the seeds of the disease home with them.”¹

Mr. Imray, in his account of the yellow fever of Dominica in 1841,² relates the following case: On the 13th of December, 1844, the detachment of the 46th regiment in garrison was replaced by a company of the 71st, from Grenada, that island at the time of their departure being healthy. On the 27th of December, another company of the 71st was disembarked from the mail steamer, and marched to Morne Bruce—the passage by steam being about thirty hours. Shortly after the first company left Grenada, yellow fever broke out in the garrison there, and was prevailing at the time the second company left. Two men were taken away ill, who, on landing, were carried to the garrison hospital. Both died with black vomit and all the symptoms of the yellow fever. This event created great alarm. “After several days had elapsed, new cases were admitted to the hospital, and the disease continued to prevail with great mortality, being, however, confined, with two exceptions,³ to the company by whom it was imported. During this time there was no interruption to the usual intercourse between the town and the garrison. The non-contagious nature of the fever was rendered still more certain by the removal of the troops from Mount Bruce to a level spot of ground in the environs, or it may rather be said within the town of Roseau, and no precaution whatever was taken to prevent constant communication with the inhabitants.”

On the subject of the more recent case of Boa Vista, on which so much reliance has been placed to prove the transmissibility of the yellow fever from the original focus of infection to other localities more or less distant, and which has caused the conversion of some writers,⁴ and is regarded by others as alone sufficient to establish beyond the possibility of doubt the contagious nature of the disease, I cannot and need not enter at large. Of the origin of the fever on that occasion I shall have occasion to speak soon. The question at present is, whether, when once developed, its appearance in various localities of the island was due to propagation by the sick—in a word, through the effect of contagion. That the facts stated by Dr. McWilliams, and so complacently repeated by others, are of a striking character, and have the appearance of truth, and that, if true and incapable of refutation, they would, more perhaps than any others, prove a stumbling-block to the advocate of non-contagion, I shall not attempt to deny. But are they really entitled to the

¹ Report of the Guiana Med. Soc., in Blair, p. 155.

² Edinb. Med. and Surg. Journ., lxiv. 338.

³ “They belonged to the company first landed. One of these men had been in close confinement from the time of his arrival, and therefore could not have taken the disease from the sick of the second company, with whom he had no communication. They both had imbibed the seeds of the disease which, though not manifested, must have been floating at the time of their departure.”

⁴ British and For. Med.-Chir. Rev., Vol. i. No. 1.

confidence accorded to them? On this point we may, for the following reasons, be allowed to demur. In the first place, they are in direct opposition to numberless facts which go to show that the fever is not so introduced by ships or otherwise, and it is difficult to suppose that it would follow a different course at Boa Vista. In the second place, they prove too much; for if Dr. McWilliams's statements are correct, the disease must have proved as highly contagious as any other known disease, if not more so. In no instance, as it would appear, did the removal of a patient to any locality fail to be followed by the development of the disease in that locality. Every case was distinctly and easily traced to exposure, direct or indirect, to an individual already affected. Now, medical readers must know that such a facility of communication has in no other instance been exhibited, whenever the investigation has been conducted with care; contagionists having, to say the most, been able to make out a probable case in a few instances only. In all others, their statements have been disproved by counter statements, or rendered doubtful by facts of an opposite character. In this country, such has been the result. Importationists may have occasionally brought forward a few facts which lend some slight support to the opinion that the disease, under particular circumstances, becomes endowed with transmissible properties, but nothing more; and certainly, heretofore, experience in tropical climates has not tended to lead to a different sentiment. Not so at Boa Vista. There, as it would seem, everything was as clear as the sun at noonday. The fever, which, according to Dr. McWilliams and his fellows, was not originally contagious, became so by the effects of contingent circumstances, and was then clearly and indisputably propagated wherever the sick appeared. This facility of tracing the cases, without mistake, from one to another, so greatly at variance with what occurs everywhere else, must, in accordance with the old saying, *quod probat nimis probat nihil*, create some suspicion as to the correctness of the statements in question.

It may be remarked, also, that until recently we were in possession of but one side of the question, our information respecting the rise and progress of the epidemic of Boa Vista being derived only from the report of Dr. McWilliams. Those who have examined the records of the yellow fever in various parts of the world, need not be told with what caution reports, apparently the most satisfactory and convincing, as to the contagious properties of the disease, should be received, and of the propriety of deferring a conclusion on the subject until such reports have been tested by the investigation of other observers. How different would have been the state of opinion of the profession, as regards the fever of this country, of Spain, and of the West Indies, had implicit confidence been placed in the statements and conclusions of Currie, Hosack, Pariset, Arejula, Sir William Pym, Sir J. Fellowes, Warren, Moreau de St. Méry, &c.; and had we not possessed the counter statements of Deveze, Chervin, Jackson, Wilson, and many others! And that the same caution must be observed in regard to the report of McWilliams, may be inferred, not only from the result of further inquiries elsewhere, but from the

fact that the very next physician who, after him, has investigated the history of the fever of Boa Vista on the spot, has arrived at conclusions different from those of that clever writer.¹

CHAPTER XXV.

PROOFS OF NON-CONTAGION—CONTINUED.

Not communicated in Hospitals, Barracks, &c.—If the disease possessed a contagious character, we might expect to find that character more readily displayed in hospitals where patients, in all stages of the disease, are collected together—where, very generally, the effluvia from the sick taint the atmosphere in a way sensible to the senses, and where the scenes of suffering presented to those around are of a kind to shake the nerves of the most resolute, and predispose them to the more energetic action of the efficient cause. Such has always been admitted to be the case in all diseases reported—correctly or not—to be contagious; and no one can deny that in respect to typhus fever and to smallpox, the statement is founded in truth, for, of the latter disease, the large majority of those unprotected by a former attack, or by vaccination, will, with certainty, be attacked; and of the former, Dr. Haygarth, who has investigated the question with great care, found that five only, out of 168 exposed to infection, escaped—or less than 1 in 33.²

But while contagious diseases are thus apt to be readily communicated in the wards of receptacles for the sick, the result of experience in this city and elsewhere shows, in the most conclusive manner, and contrary to the assertion of contagionists,³ that no such tendency has been exhibited by the yellow fever, whether during warm weather, when, the windows being open, the freest ventilation is maintained, or during the cool weather of autumn, when the wards are kept comparatively close.

The early epidemic of 1793 afforded ample means of testing this fact. During the prevalence of the fever, the general hospital for the accommodation of the sick was established at Bush Hill, about two miles from the seat of the epidemic. To the wards of that asylum were conveyed an immense number of diseased individuals. It is hardly necessary to remark, that into such a receptacle the very worst forms of the complaint would necessarily find their way. Although everything practicable was done to effect it, cleanliness in the hospital could not be preserved. The establishment had been very hastily and defectively prepared, in the midst of great confusion and a want of means. The nurses, in the beginning, were inexperienced and unfaithful, and even of that description a competent number could not be

¹ See Dr. King's Report on the Fever of Boa Vista, ordered by the House of Commons to be printed, 10th March, 1848.

² Letter of Dr. Haygarth to Dr. Percival, p. 32.

³ Hosack, i. 259.

procured. The consequence of such a state of things must be readily perceived. By exhalations from the sick, the dying, and the dead, the atmosphere of the building was thoroughly impregnated. From the neglect of those in attendance, portions of the floor were often covered with the matter of black vomit. Owing to the same cause, the natural excretions of the patients were not, at all times, very promptly removed. For want of more suitable apartments, the nurses and attendants ate, drank, and slept in the wards of the sick. Had contagion existed, a more suitable arrangement for the propagation of fever by it could scarcely have been imagined. The closing of the doors and windows of the edifice would have rendered it complete. Yet nurses nursed, attendants waited, and physicians visited, without the production of a case of disease. The sick introduced from the pestilential section of the city excepted, the establishment presented, throughout the season, uninterrupted health.

This statement, which I transcribe in the language of Dr. Caldwell,¹ is fully confirmed by that of Deveze, the physician of that establishment, by Carey² (p. 61), and other writers and chroniclers of the events of that epidemic. Deveze, in illustration of this absence of communication of the disease on the occasion alluded to, mentions the following fact: During the height of the epidemic, a vessel filled with patients from the hospital of the Cape (St. Domingo), arrived at Philadelphia. The inhabitants, fearing lest these individuals might, by their presence, increase the intensity of the disease, induced the authorities to order the removal of the vessel from the port. Through the influence of the French ambassador and of Dr. Deveze himself, permission was obtained to convey the sick to Bush Hill. The vessel was ordered up the Schuylkill, the sick were landed and placed in huts erected around the hospital—the wards, entries, and even the cellars of that establishment being already crowded with yellow fever patients. Many of the new arrivals had been wounded, and among these some required important operations, which were performed generally with success. As vacancies occurred in the main building, patients from the huts were placed in it. Gradually, these patients all found admittance into the wards of the hospital (p. 222). But neither these nor individuals admitted from the city, labouring under intercurrent diseases—and they were not few—took the yellow fever (1st ed., p. 35). Dr. Deveze admits, in his first edition, that Mrs. Saville, the matron of the house, and two male nurses, had the disease; but there is every reason to believe they had all imbibed the germs of it in the infected districts.

The hospital was opened for the reception of patients in subsequent epidemics, and the results at each period differ in nothing from those recorded in relation to the eventful year of 1793; thereby fully justifying the remarks of Dr. Caldwell, that “notwithstanding every possible exposure of nurses and attendants, the records of that institution do not, since its establishment, present a single instance in which yellow fever was communicated from the sick to the well.” This exemption, which was early noticed by some physi-

¹ Prize Essay, pp. 121–2.

² First ed., pp. 31, 35, &c., last ed., p. 221.

eians, and pointed out in 1794 by Dr. Deveze, was generally admitted even by contagionists themselves during the epidemic of '98; one of whom remarks that it was often eustomary for the nurses to sleep on the same beds with the siek, and that some were known to swallow a portion of the juiees (?) of the sick, without experieneing any injury. They were seen to help a patient to a spoonful of food, and to take another themselves, or eat the fruit that had been gnawed by the sick. Instanees occurred of wives nursing their husbands in the hospital, and mothers their children, none of whom took the disease, though lying on the same bed,¹ and being surrounded by other individuals labouring under the disease in its various stages.

Condie and Folwell affirm, it is true, that some of the nurses took the disease; and these, as well as the death of one of the physieians of the establishment, Dr. Cooper, which took place there in the early part of September, and a few cases of slight indisposition which occurred among the nurses in 1799, were eagerly seized upon, as proofs of their views by the eontagionists, who affirmed that Dr. Cooper must have taken the disease from the sick of the hospital, "as he had not been in any other part of the eity for more than three weeks before his attack."² But, in answer, it may be remarked that the authenticieity of the eases said to have occurred in 1798 remains yet to be proved—that the indisposition of the nurses in 1798 could scarcely be referred to the reigning fever, inasmuch as the disease in all terminated favourably; and as regards Dr. Cooper, no conclusion in favour of eontagion can be drawn from his case; because, so far from having been confined to the hospital for three weeks, it was satisfactorily shown that he had gone to the foot of Market Street near the wharf—the very focus of infection—for the purpose of bottling atmospheric air with a view to submit it to analysis.

At the City Hospital, which was a receptacle of the most malignant cases of fever, in 1805, and to which upwards of three hundred patients were conveyed during the season, no marks of contagion were discovered. (*Caldwell*, p. 71.)

To this may be added that, in 1820, a number of cases were as usual admitted into one wing of the City Hospital, while the other wing was appropriated to the accommodation of such of the inhabitants of the infected districts as were ejected from their homes, and could not find shelter elsewhere. Free intercourse was permitted between the inmates of the two wings of the establishment. The rooms, which are small, were often crowded, and towards the close of the sickly season close and confined. The patients were nursed assiduously, visited twice daily by the physieians, and surrounded by medical students—yet not a ease of eontagious communication was heard of. In 1853, twenty-four cases were admitted into one of the wards of the Pennsylvania Hospital; eighteen were reeeived at the Blockley Hospital; seven at the Bush Hill or City Hospital, and three at the St. Joseph's Hospital. Dr. Jewell—himself an importationist—informs us that in no instance can it be shown that the disease spread from those labouring under the fever.

¹ Condie and Folwell, p. 84.

² Currie, 1798, p. 43.

At the Pennsylvania Hospital, the yellow fever cases were intermixed "with numerous other patients; some ill, and others convalescing from disease, but not an individual—either among the patients, nurses, or visitors—contracted the fever. The like immunity was observed with the cases treated in the Blockley, St. Joseph's, and Bush Hill Hospitals."¹

Dr. Miller, of New York, also remarks that no exception to the fact of the non-communication of the yellow fever in hospitals situated at a small distance from the cities to which they belong, had ever occurred up to his time in the establishment at Bellevue or the Marine Hospital at Staten Island, provided the malignant air of the city had been avoided. "The nurses at Bellevue Hospital became," he says, "so entirely free from all apprehension of the contagiousness of this disease, that they often slept on the same bed with the sick; and it happened more than once, in the course of the season, that a nurse overcome with fatigue and want of sleep, threw herself in the night, for a little repose, on the bed of a dying patient, and remained there asleep till the patient was dead, and it became necessary to remove the corpse" (pp. 103, 104).

This was found to be the case in 1795; for we learn from Dr. Seamen that "there was not a single attendant who (at the Bellevue Hospital) took the complaint, excepting such as had been but a few days from town, notwithstanding they were constantly involved in the midst of the effluvia arising from the numerous sick and dead surrounding them."²

During the sickly season of 1798, three hundred and eighty-nine persons were admitted into the Bellevue Hospital; and of this number two hundred and five died, and yet the attendants of the hospital, the physicians, nurses, and washerwomen; although literally surrounded by pestilence, did not catch the infection. And the boatmen belonging to the health office, who entered the hospital at all times, and were not only employed in bringing the sick from the city and shipping, but in removing them from place to place, enjoyed a uniform state of good health. Dr. Douglass, one of the assistant physicians, was seized with fever, but three days previous to his being taken sick, he slept in a house in the infected district, and his sickness was attributed to that cause.³

In 1799, during the months of July, August, and October, there were about two hundred and fifty persons ill of yellow fever in the Marine Hospital, a great part of whom were from the shipping, the rest from the city. At that time, there were thirty-seven persons employed or residing in the establishment, "not one of whom, although daily and hourly amongst the sick, experienced the least indisposition."⁴ The same complete exemption from contagious communication was exhibited during the fever of 1803⁵ and 1805.⁶

¹ Tr. Coll. of Phys., ii., N. S., 119.

² Webster's Collection, p. 17.

³ Report on N. Y. Quarantine Laws, 1846, p. 8; Hardie, Fever of 1798, pp. 52, 53.

⁴ Hardie's Account of Fever of 1822, pp. 8, 9.

⁵ Med. Reposit., iii. 184.

⁶ Miller, p. 104; Hardie, op. cit., p. 17; Doc. relating to the Board of Health, 1806, pp. 55, 56.

From the official returns of Dr. Joseph Bayley, health officer of the port of New York, it appears that during the prevalence of the yellow fever there in 1822, seventy persons sick with that disease were sent down to the Marine Hospital on Staten Island. Of this number thirty-eight died, eighteen of whom had black vomit. The first of these patients was admitted on the 13th of August, and died two days after. From that period until the cessation of the epidemic, patients were almost daily carried down to the hospital. They were regularly attended by the physicians and nurses of the establishment, not one of whom was taken with the disease, nor did a single case occur of any person being affected who was engaged in transporting the sick from the city to the hospital.¹

Baltimore has afforded several instances of this kind, as may be found in the writings of Macaulay,² Reese (p. 71), &c. Of the occurrences of 1819, Dr. Revere says: "The hospital is situated about one mile from the city, on a very elevated, delightful spot, and, in the extent and arrangement of the building, is inferior to no institution of the kind in the United States. It generally contains from 130 to 150 patients—lunatics, and sick of the United States navy. During the whole of the disease there were from 30 to 50 patients, in every stage of the yellow fever, from its mildest to its most malignant form. The wards where these persons were placed communicated by entries with the rest of the house, and there was an uninterrupted intercourse kept up. There were nurses constantly in attendance, employed in removing the excretions, and yet there was not a single circumstance which could lead one to suppose that the disease was contagious. Such was the conviction of its non-contagious nature, not only in the minds of the faculty, but of the other citizens, that the hospital was thronged, particularly on Sundays, by people, some of whom came to visit their friends, and many who came merely to witness the effects of a disease which had excited so much horror."

During the fever which prevailed at Pensacola in 1841 among the crew of the ship *Levant*, 138 patients were received into the hospital; and we are told by Dr. Hulse, that not one case occurred among the officers and attendants of that institution, which is located on a bluff, one mile west from the navy-yard, and about a mile and a half from the sea.³ In other cities of the Union—Boston, Norfolk, Providence, Newport, New London, &c.—the same exemption from contagion has been noticed in receptacles for the sick, so uniformly and obviously as to lead the profession and such of the public as have devoted attention to the subject to regard it in the light of an axiom.⁴

¹ See Beck, *N. Y. Med. and Phys. Journ.*, i. 473; Bayley, *Hist. of the Proceedings of the Board of Health*, pp. 132, 150.

² Letters, &c., on *Epid. of 1819*, p. 32.

³ Hulse, *Monograph on the Yellow Fever*, *Maryland Med. and Surg. Journ.*, April, 1841, pp. 392–394.

⁴ Ramsay, *Rep.*, iv. 220; Documents furnished to Dr. Chervin, by Dr. Archer, of Norfolk, Wheaton, of Providence, Turner and Waring, of Newport, Townsend, of Boston, and Lee, of New London, see *Rapport de l'Acad. de Méd., sur les Documens de M. Chervin*, p. 26; *Med. Reposit.*, iv. 5.

Dr. Stone, who was a resident physician in the Charity Hospital of New Orleans in 1833, informed Dr. Drake that nearly all the officers of the house were, like himself, unacclimatized. "The fever commenced near the river, and in the latter part of autumn prevailed among the inhabitants in the neighbourhood of that establishment. It was crowded with yellow fever patients throughout the season; yet the officers, who did not go out into the city, remained exempt till the people near the hospital were invaded, when it fell upon the officers of the establishment."¹ That is, so long as the cause of the disease did not reach the neighbourhood of the hospital, the unacclimatized officers were exempt, unless they visited the sickly localities; when it reached that neighbourhood, they suffered like the rest of the population. Dr. Arnold, of Savannah, saw, in 1839, two cases of yellow fever ending in black vomit, occurring under circumstances which ought to have favoured its spreading. Yellow fever prevailed epidemically in that year both in Charleston and Augusta. About the latter end of August, a man who had just left Charleston entered the hospital, the wards of which were then filled with cases of bilious remittent fever. He died with black vomit. About ten days afterwards, another man entered who had just left Augusta. He also died with black vomit. Not a single patient contracted the disease.²

As we have already seen, Dr. Nott contends for the introduction of the fever into Mobile in 1854. Well, the same physician tells us: "On the record of our '*City Hospital*,' the following entries are made of yellow fever cases: July 23d, one; 25th, two; 26th, three—all of whom were labourers that had fled from the epidemic in New Orleans, and were either sick on arrival or taken soon after. It may be worthy of remark, *en passant*, that I was informed by the Sisters of Charity that the disease did not spread among the inmates of this hospital until some time after, when it had become epidemic throughout the city."³

Dr. Lebby, of Charleston, in a letter addressed to Dr. Strobel, and published by the latter in his work on the transmissibility of the yellow fever (p. 190), states the following: In August, 1827, three men belonging to a company of United States artillery went from Fort Johnson, where they were stationed, to Charleston, during the existence of yellow fever, and remained over night. On the ninth day after, they were attacked with the disease, within a few hours of each other. They were not removed from their quarters (the hospital at the time being out of repair), and, during the period of their sickness, ten or twelve roomed with them, who were alike strangers to the climate; yet there were no other cases of disease in the garrison.

During the epidemic at Fort Moultrie (Charleston) in 1852, "the surgeon, steward, and four nurses, all continuously exposed for weeks together, had no kind of fever; three nurses and one cook had remittent fever in September, but nothing like yellow fever afterwards; and three nurses and one cook had

¹ Drake, ii. 208.

² The Dengue or Break-bone Fever, Charleston Journ., vi. 338.

³ New Orleans Med. and Surg. Journ., x. 572.

yellow fever. Thus we have fourteen persons on duty in the hospital, of whom six had no kind of fever, four had remittent, and four had yellow fever. Throwing out the cooks, who were not so much exposed among the sick as others, but were more exposed to heat in the exercise of their vocation, and we have six who had no fever, three with remittent, and three with yellow fever; and these twelve persons were exposed with the sick in an especial manner. We have, then, only three persons in twelve sick in a yellow fever hospital, a circumstance which seems incredible in a contagious disease. All these persons were on hard duty, which would predispose to the disease; yet three-fourths of them escaped."¹

Palloni, contagionist as he was, acknowledged that in the hospitals of Leghorn, in 1804, no attendant perished, and that the fever was not communicated in them to patients labouring under intercurrent complaints.² This statement is confirmed in a report to the Queen of Etruria, signed by all the consuls residing at Leghorn at the time.³ At the Nazareth Lazaretto of Barcelona, in 1821, seventy-nine patients labouring under yellow fever were admitted. Of these, fifty-five died. Of thirty-two individuals employed in the establishment, not one had the slightest indisposition. The same result was obtained in the Hospital of the Vice-Queen of Peru, where none of those who had intercourse with the sick—physicians, assistants, or nurses—had the disease.⁴

Dr. Doughty, in his work on the yellow fever, remarks (p. 192): "In the season of 1810, to which my own particular observations relate, there were doing duty within the walls of Cadiz, and at the Aguada (Hospital), two physicians to the forces, two surgeons to the forces, one deputy purveyor, an apothecary, five or six clerks, and twelve or fourteen regimental and general hospital assistants, not one of whom had ever been in the West Indies, or where the fever had prevailed; yet not one of them was attacked, although several attended patients labouring under the disease, as well as assisted me in the dissections I have given." And, a few pages beyond, he mentions that not one of the orderlies in the Hospicio (hospital), where several cases of this fever were admitted, and not one of those in the Aguada, where the greater number of the British troops affected with the disease were placed and treated, and where many of them died, fell sick of the same order of fever (p. 195).

Besides several instances of like nature, already referred to, I may mention the following: In 1802, as we are informed by Pariset, on the authority of Dr. Gonzales, Admiral Gravina caused 500 sailors, affected with yellow fever, to be disembarked in the last-mentioned city. They were conveyed to the

¹ Porter, *Amer. Journ. Med. Sci.*, January, 1855, p. 87.

² *Osserv. Mediche*, pp. 39–41. See also *Edinb. Journ.*, ii. 90.

³ Deveze, p. 231.

⁴ Chervin, *Fev. of Spain*, pp. 172, 173; Answer of the Physicians of the Superior Junta to the Political Chief of the Province, in Chervin, p. 101; Manifesto in Maclean on Quarantines, p. 134; Piguilem, Letter to Lassus, in O'Halloran, p. 79; *Ibid.*, *Ann. de la Méd. Physiol.*, i. 409.

Hospital San Juan de Dios, and treated therein, without communicating their malady to any person.¹

The members of the Sanitary Committee of Cadiz—Arejula, Ameller, and Coll—inform us that in the summer of 1805, the fleet of the same admiral, on its return from the West Indies, sent to the hospital of that town two hundred sick, most of them yellow, and many with the black vomit, and other symptoms of the yellow fever—*los mas de ellos amarillos y muchos con el vomito negro y otros symptomas de la fièvre amarilla*; that in 1807, the French fleet lying in that harbour landed several patients at the Hospital of the Aguada with the same symptoms, where several of them died; and that although the vessels from which they proceeded kept up a free and unrestricted communication with the town, the disease did not spread at all. “*La enfermedad no se estendio, ni comunico de modo alguno.*”²

On a spontaneous outbreak of yellow fever on board her Majesty's ship Bedford, when in Gibraltar Bay, there being at that time no fever in the garrison, 130 sick were landed and sent to the hospital; eleven died, and others were left dangerously ill on the departure of the ship. The disease did not extend beyond the crew.³

In allusion to occurrences in Gibraltar, during the epidemic of 1828, Dr. Amiel says: “It is a well-known fact that the fever spread to the *south* two or three weeks later than in the town. From the 2d of September to the 1st of October, several cases of the epidemic were admitted into the regimental hospital, three of which died with the black vomit; but the disease never attacked any of the other patients (more than twenty in number) treated during the above period for other complaints, nor any of the orderlies, who had, as usual, an incessant and universal intercourse with the dying, and slept in the same wards. It was only on the 25th of September, when the disease had spread in the south, and when the epidemic influence had extended to the district where the hospital is situated, that the cook of the establishment, who never had occasion to approach the sick, contracted the disease; and in the month of October, when the atmospheric causes had acquired more intensity, the hospital sergeant and twelve orderlies, successively sent from the camp, were taken ill but a few days after entering the precincts of the hospital, and several of them fell victims to the disease.”⁴

To the same import is the testimony of tropical physicians. Dr. John Hunter, in his excellent work on the diseases of the army in Jamaica, states that in the military hospitals the sick admitted with fevers were above three-quarters of the whole; and they were often much crowded together, yet there was no reason to believe that a man, with any other complaint, ever caught a fever in the hospital (p. 83). The same exemption in the hospitals of Jamaica

¹ Fièvre Jaune de Cadiz en 1819, p. 79.

² Dictamen de los tres profesores Medicos Commissionadis pro la Suprema Junta Sanidad para indagar el origen de los calentura reynante, 1810, p. 2.

³ Second Report on Quarantine, p. 32; Burnett, Rep. on the Bann Fever, pp. 28, 29.

⁴ Amiel, Edinb. Journ., xxxv. 272, 273.

is pointed out by Dr. Walker.¹ "At Barbadoes," says Dr. Ferguson, "our hospitals of late years have been in a regular course of importation of the yellow fever from the navy; but not even inoculation has been able to produce the disease upon any member of the hospital corps, by whom, I may truly say, that the sick have been received with open arms; for the anti-social doctrines of ideal contagion are not preached among us here to the prejudice of duty and humanity."²

Another respectable authority, Dr. Magrath, principal medical officer of the Public Hospital in Kingston, Jamaica, remarks in relation to this subject: "Yellow fever has not for several years prevailed as an epidemic in Kingston, but occasionally we receive patients suffering from it into the Public Hospital. In 1848, the crew of a vessel that had a few months previously been employed in carrying guano suffered most severely; and in 1849, the persons on board two coal ships, which, after discharging their cargo took in some impure ballast, were attacked with yellow fever in its most virulent form, while all the other vessels in the harbour remained nearly free from the disease. The sick from those ships were placed amongst the other patients in the hospital."

The same physician informed Dr. Milroy, "that nearly twenty cases of aggravated yellow fever were received from those two vessels (the holds of which were found to be in the foulest state imaginable) into the hospital, and a very large proportion of them proved fatal. None of the other inmates or of the attendants of the hospital were affected. Moreover, there was no prevalence of the disease on shore at the time."³ In this hospital, yellow fever patients are put into the same wards with the others, and there is not the smallest care taken to prevent the freest intercourse from being carried on with all around, for Dr. Magrath and Dr. Scott, the physicians of that institution, and men of the best ability, long accustomed to yellow fever, are non-contagionists; nor was their opinion proved false by its results (of the admission of cases from steamers Medway, Conway, Orinoco, Magnolina, and Parana, in the fall of 1852), for not a single case occurred amongst the attendants and patients, or any of the inhabitants of Kingston. Such also was the result at Nassau, N. P. "On the 9th of November, 1852, the *Esk* arrived there, and sent several cases of yellow fever ashore, which were distributed to three separate houses. Two, at least, had black vomit, and two died; but the disease never spread through the island, nor indeed did a single new case occur, although cholera was, at the time, committing fearful ravages on all sides."⁴

Dr. H. McLean remarks that "no circumstance attending an infectious fever occurred in the remittent of St. Domingo. When a soldier was seized

¹ Some Account of a Pestilential Fever which prevailed in the Island of Jamaica in the years 1793, '4, 5. See extract from it in *Med. Reposit.*, i. 497.

² Second Report on Quarantine (London, 1852), p. 182.

³ Second Report of the General Board of Health on Quarantine in 1852, p. 33.

⁴ Cummins on Yellow Fever, *Lancet*, July, 1853, Am. ed., p. 50. See also Second Quarantine Report, p. 33.

in the barracks, it was not observed to spread in that particular quarter, and sometimes only one was attacked; nor could we remark, when they came into the hospitals, that in one case whatever the contagion was evident. The medical gentlemen could not have possibly escaped if there had been any infection" (pp. 76, 78). "Those most exposed did not suffer. A few had the fever, and died; but they had been for a long time exposed to the causes of the remittent before they were attacked" (p. 77).

The following case is worthy of insertion, as showing that, in a hospital situated in the very midst of the epidemic influence, and crowded to excess, the attendants, if not secure from the disease, were not more affected than they would have been had they not entered within the precincts of the establishment. The occurrence took place at Bermuda, in 1843, and is recorded by Dr. King in his report on the Boa Vista fever (p. 14). From the commencement to the termination of the epidemic fever, 1,210 patients labouring under the disease were admitted, and of these, 110 cases terminated fatally. Owing to the urgent demands for hospital accommodation during the time, the wards were much crowded—and, in fact, there was hardly room to pass between the beds. "A large store-room on the ground floor was converted into (what I called by way of distinction) the foul ward, where we placed the patients who were received in an almost hopeless condition. The two large lobbies in the floors above were filled with patients we considered to be dangerously ill, and to each ward and lobby two male nurses were appointed, who remained there day and night. The medical staff of the hospital comprised one deputy inspector and three assistant surgeons; but when the latter were attacked with fever one after the other, I applied to Sir Charles Adam, the commander-in-chief, who immediately, and also at a subsequent period, sent medical officers from Halifax to assist in our labours. In the daily routine of duty, we made three—generally four—regular visits to the hospital, where we took notes at least twice a day, at the bedside of each patient; and the senior assistant went through the wards every night. Our first visit was to the foul ward, and commenced at seven o'clock in the morning; we next proceeded to the lobbies, and then visited the other wards, where the patients were either convalescent or had the disease in a mild form. After a hurried breakfast, we again met in the operating room, and inspected the bodies of those who had died in the night; and altogether we made seventy-two minute post-mortem examinations. Is it possible, sir," continues Dr. King, "after what I have stated, to conceive any combination of circumstances more unfavourable, or any situation where the attendants on the sick could have been more inevitably exposed to the operation of a specific contagion? And yet only three of the eleven medical men suffered from the disease, in a mild form; one half of the servants, I believe, had not the disease in any form whatever; and it is a remarkable, but highly important fact, that not one person of the seventy-seven attendants I have alluded to died of fever on that occasion."

A statement, very recently published, cannot well be omitted in this place. "One would naturally be led to believe that there, where a focus of infection

existed, in a hospital containing all the sick, could not be found a place of safety; it, nevertheless, is that which, in all probability, offers more of it than is supposed. Physicians are never seen to suffer there from the disease, more than in the just proportion of their numbers; and unacclimated servants, more immediately in communication with the sick, the orderlies, and others—if the ventilation and discipline are good—suffer less than the soldiers of the barracks who never come into proximity.” “It is a fact which I observed in the military and civil hospital at Vera Cruz; and it is a fact, too, that the supposed contagion never communicated itself to the patients in the surgical departments or to the convalescent, although they occupied contiguous beds in the same hospital.”¹

We are informed by Dr. R. Lallemand, in his interesting account of the fever of Janeiro in 1850, that proofs of non-contagion were amply furnished by the occurrences in the hospitals of that city. “Who are they,” asks Dr. L., “that were most exposed to the epidemic—this most contagious epidemic? Ask Dr. José Mariano da Silva, who lived two months in the convent on the island of Bours-Jesus, if he was attacked! Ask Dr. A. J. Peixotto, who lived so long in the midst of so many patients in the Gamboa Hospital! Do you know why they were not attacked? Because the disease was not contagious, and those gentlemen were not afraid! And a great portion of the gentlemen boarders, devoted with so much courage to their duty, perhaps they were attacked?”² Dr. L. himself affords a satisfactory illustration of the innocuousness of hospitals, for his duties in the convent of Bours-Jesus and the hospital of La Misericordia were arduous; he was much confined in the wards of those crowded establishments, and yet passed safely through the epidemic (p. 180).

Dr. Lefort states as a fact of not unfrequent occurrence at Martinique, that individuals admitted into the hospital for various complaints, are placed, inadvertently or knowingly, in beds still warm, in which persons have died of the yellow fever; but that in no instance has the disease been communicated in this way. The same distinguished writer informs us, that from the 1st of July, 1818, to the 31st December, 1822, 1,982 yellow fever patients were under treatment in the hospital of Port Royal, and that nearly three hundred post-mortem examinations were made. Nevertheless, not one case of transmission of the disease to the medical assistants, to the nurses or employés occurred.³ Dr. McArthur, in a report to the British Government, communicated the following fact, which deserves insertion in this place. “No place could be better adapted to propagate and disseminate contagion than the building which we occupied as an hospital in Bridgetown (Barbadoes), until May, 1807. It was situated about a quarter of a mile from the wharf where the patients were landed, and on the leeseide of the town, con-

¹ Dr. W. Humboldt, Yellow Fever of Mexico in Report of the Sanitary Commission on Yellow Fever of New Orleans in 1853, p. 130.

² Fever of Rio Janeiro in 1850, Rept. of San. Com. of N. O. 1853, p. 179.

³ De la Non-contagion de la Fièvre Jaune, p. 36.

finned by miserable huts, huddled one upon another. The patients, and their bedding, were carried through the town to this place by such labourers as we could hastily employ for the occasion. They were obliged to rest and take shelter from the sun in any house that would receive them; and, to the credit of the poor white and coloured inhabitants, they never were forced from their doors. The nurses, washerwomen, and sempstresses, were black or mulatto women, who daily visited their families in our neighbourhood; and as there were no accommodations for any description of officers in the hospital, lodgings were procured for them in various parts of the town, and they were received without hesitation, labouring under the most malignant form of the disease, even in *articulo mortis*. Yet, with this free communication with the inhabitants in every part of the town, the fever did not spread generally nor partially, neither in its aggravated nor its milder form. If it be contagious, in what way can we account for the native inhabitants of Bridgetown escaping every degree of it during a period of five or six years?"¹

During the eight years' epidemic at Demerara, described by Dr. Blair, the directors of the hospital, twenty-three in number, who daily visited it, and had constant intercourse with the sick in the wards, all escaped the disease. Dr. B. states that he does not recollect of any others who suffered from an attack who were near the sick; "in fact, it seemed as if those who were closest to the diseased enjoyed the most perfect immunity." All the resident surgeons of the hospitals, whose term of service varied from six months to two years and upwards, escaped. The chief medical officer, Dr. Smith, escaped; as also all the dispensers (three in number), who did duty as such. One died of the yellow fever, but he never did duty, and died the same evening he was gazetted to his office. All the stewards, eight in number—all the nurses, who were very numerous and frequently changed (averaging in the two hospitals—exclusive of other servants in constant attendance—22, seven in the Seamen's Hospital and fifteen in the Colonial), escaped.²

The same writer informs us, in his volume on this epidemic, that the way to give a yellow fever nurse the disease was, not by bringing him in close contact with the sick, but by discharging him or her from the hospital. "After knocking about town for a few weeks, and getting into the malarial districts, they would, it is likely, be brought to the hospital as yellow fever patients." Several cases of the kind occurred. "During the epidemic," continues Dr. Blair, "the yellow fever cases, in their worst forms, were never separated from other patients in our hospital wards. Such a thing was not deemed necessary, and never thought of" (pp. 54-5). In the Report of the British Guiana Medical Society, we are told that out of upwards of two thousand cases of yellow fever treated in the Seamen's Hospital, not thirty cases are recorded as having been attacked whilst suffering from other diseases in the hospital; and most of those who were so attacked had only been under treatment one or two days."³

¹ Bancroft, Sequel, p. 147.

² Blair, Letter in Brit. and For. Med.-Chir. Rev., vii. 556.

³ Blair, p. 153.

Were it necessary, it would be easy to corroborate what has thus been stated as to the innocuity of hospitals as sources of transmission of the yellow fever, by a reference to other facts and statements of the highest value recorded by Ferguson,¹ Menzies,² Pinckard (ii. 284), Ralph,³ Musgrave,⁴ Wilson (pp. 85, 6), Savarésy (p. 151), Dalmas (p. 67), Bone (p. 21), Ranpack, Griffins, Crosbie, Maclerty, Young, Hugh Bone, Smith, McDermott, Dummit, Court, Hernandez,⁵ Burnett,⁶ Barrington,⁷ Hulse.⁸ But enough has been said on the subject to establish the reality of the fact in question.

That cases have occurred of the yellow fever spreading more or less extensively in hospital establishments is a fact which I am neither prepared or willing to deny. I am aware that, at Barcelona, in 1821, the Hospital of the Seminaria, into which 1767 patients were admitted during the epidemic (of whom 1,293 died), presented an occurrence of the kind—three out of the ninety individuals in attendance contracting the disease. I know that cases of the same kind were observed in the General Hospital of that city.⁹ Instances of a similar character have been noticed at Gibraltar;¹⁰ in Martinique, and other West India Islands,¹¹ as well as in New Orleans. But it must be remembered that the establishments in which occurrences of the kind have been observed, are situated within the sphere of activity of the efficient causes of the disease, a defect difficult, if not impossible, to obviate, particularly in tropical climates and in New Orleans, so long as it is not deemed proper to remove the hospital far beyond the limits of the towns. Such being the case, it would be improper to ascribe the occurrence of the disease in those establishments to the intercourse of the persons attacked with the sick, inasmuch as they were exposed to the same agencies as infected other individuals placed beyond the reach of any contagious effluvium arising from the inmates of the hospital.

In corroboration of this, it may be further remarked that the medical officers of those establishments, the attendants and patients under treatment for other complaints are never found to suffer more, in proportion to their numbers, from the fever than individuals at large. Indeed, in tropical climates and elsewhere, facts may be adduced to prove that the more immediate white attendants, orderlies, and others, uniformly suffer less, if the ventilation and discipline be good, than soldiers in barracks and labourers who never go near the place;¹² whether because while so employed, they are saved from exposure to the solar

¹ Reports, see *Med.-Chir. Trans.*, viii. 147; also Bancroft's *Seq.*, pp. 186, 201.

² *Ib.*, p. 187, note, p. 201; *Med.-Chir. Trans.*, vii. 171.

³ *Edin. Med.-Chir. Trans.*, ii. 57; Bancroft's *Seq.*, p. 449.

⁴ *Med.-Chir. Trans.*, ix. 113; *Med.-Chir. Rev. and Journ.*, App. B., iv. 994.

⁵ Chervin, *Rapport*, pp. 26-7.

⁶ Report on Bann Fev., pp. 32-3.

⁷ *Am. Journ.*, xii. 308.

⁸ *Maryland Med. and Surg. Journ.* 1842, p. 392.

⁹ Maclean, pp. 134-35.

¹⁰ Bancroft, *Sequel*, p. 393.

¹¹ Kéraudren, pp. 25, 26; Lefort, *Repons*, p. 16; Pinckard, ii. 484; Ferguson, p. 143; Chervin, *Rept.*, p. 26; Rufz, p. 45.

¹² Ferguson, p. 143; Pinckard, ii. 484; Lefort, *Réponse à M. Kéraudren*, p. 16; *Manifesto*, Maclean, p. 135; Savarésy, p. 152; Gillkrest, 2d *Quarantine Report*, pp. 166, 167.

heat, to night guards, and drunkenness, or from other causes, matters not in this place. But, let this be as it may, it is sufficient to know that in hospitals situated beyond the sphere of activity of the cause of infection; the inmates of which are not exposed to the influence of the same agencies as affect individuals who have no communication with them, the disease is never transmitted from the sick to the well.

Nor am I prepared to affirm, that attendance at a yellow fever hospital as nurse, physician, or otherwise, would have no influence on the production of the disease. So far from this, I believe that it may act as an exciting cause; or when the establishment is crowded, ill ventilated, and badly kept, it may tend to aggravate the attack. But such an effect can only be produced in one who has imbibed the seeds of the disease elsewhere; and may result equally, whether the attendance be on yellow fever, or on any other disease, capable of vitiating the atmosphere.

To weaken the proof of non-contagion derived from the exemption to which attention has thus been called, it has been said, on different occasions, that patients are usually removed to such hospitals successively, and at distant intervals, and are, therefore, seldom if ever crowded together; that the wards are usually spacious, well aired, and kept clean; that, in general, there are few, if any persons, sick in them of other diseases, and on whom the contagion could operate; that the attendants are few in number, and, as it were, habituated to the disease, and thereby less liable to take it; and that from the airy and healthful situation of those establishments, the poison is diluted and rendered comparatively harmless. Such are the arguments and facts adduced by Dr. Townsend (p. 64) in reference to the non-communicability of the disease at the Marine Hospital on Staten Island; and such will be found a fair specimen of those employed by other writers in reference to this subject in other localities. Dr. Beck, in his admirable review of Dr. Townsend's volume, has demonstrated in the most convincing manner, the fallacy of the attempts so far as regards the establishment in question—showing that there must have been frequently from ten to twelve persons sick at one and the same time—that this number ought to have been more than sufficient to communicate the disease, if, as is maintained by contagionists, cases of contagion result often from intercourse with a single sick person—that no argument can be derived from the pure air of the hospital, seeing that it is maintained that the disease “to all appearances, pursues its course, independent of what in common language is understood by a pure or impure air”—that if the cleanliness and airiness of the hospital prevented the operation of the contagion, the same causes must have produced the same effect in all other situations similarly circumstanced; whereas, many other localities where the disease is represented as having proved highly contagious, are said to have been pre-eminently distinguished for their beauty, cleanliness, and salubrity—that the assertion as to the want of subjects upon whom the contagion could have operated, is not correct on the score of fact; and that there is no proof that the physicians and nurses were less susceptible of taking the disease than

other persons; for, however true this may have been at the close, it was certainly not so at the commencement of the epidemic.¹

The failure of all such endeavours to weaken or destroy the force of the argument, and impugn the importance of the facts favourable to the doctrine of the non-contagiousness of yellow fever founded on the non-transmissibility of the disease in hospitals, will be still more apparent when we bear in mind that this exemption has been noticed, not in one establishment only, but in all, situated beyond the limits of the infection, and that among these, some, at least, were far from being in a situation and in a condition capable of insuring the advantages mentioned by Dr. Townsend. They were crowded with the sick, in a filthy state, and contained in some instances a considerable number of individuals labouring under a variety of diseases—medical and surgical—they were amply provided with attendants, &c., on whom the contagion, had it existed, must necessarily have operated. The account I have given of the state of Bush Hill Hospital in 1793, and of that of the Bermuda Hospital, leaves no room for doubt on that score; and as a proof that the exemption of the medical attendants of hospitals does not arise from their being inured to the yellow fever poison, I may recall the cases of Drs. Campmany and Vicente Vila, of Barcelona; the former of whom was physician of the Lazaretto of Nazareth, and the other of the Hospital of the Vice-Queen of Peru, in that city. Both these physicians remained perfectly free from the fever during their attendance at those establishments, and until their suppression, when Dr. Campmany ventured on professional business at Barcelonnette, and Dr. Vila at Barcelona, and both shortly after took the disease, which proved fatal to the latter.²

The multiplicity of these instances of exemption—noticed in all the cities of the Union, as, indeed, in Europe and all parts of the world, where the yellow fever prevails—must be conclusive, and are well calculated to create great doubts as to the accuracy of the few cases of an opposite kind we read of occasionally in the writings of contagionists.

The Fever not conveyed by Individuals or otherwise to Ships in the Vicinity of Infected Localities.—The same effects are produced when the receptacles are, instead of hospitals, ships. Experience shows that so long as these remain at even a short distance from an infected town on shore, they may continue exempt from the disease—provided, always, they do not contain in their holds or their cargoes the materials from which the yellow fever cause is evolved. This subject has already been referred to, while treating of the effects of winds. It was then stated that while some ships take the fever when placed near such infected localities, others escaped—the results being due to the influence of the aerial currents. It may be here added that the effect is obtained, although the people on board have had no communication with the shore.³ Sure it is that on the coast of the West Indies and of Africa, as well, indeed, as in this

¹ N. Y. Med. and Phys. Journ., ii. 474–480.

² Periodico de la Sociedad de Salud publica di Catalonia, p. 362.

³ Burnett, p. 313; Cummins, London Lancet, July, 1853, p. 50, Am. ed.

country and Europe, vessels remain healthy so long as they keep at a distance from the land; but woe to them if during the sickly season they approach the shore. While, in the autumn of 1852, many British steamers and vessels of war had the yellow fever on going into port at St. Thomas, "another of her Majesty's ships, the *Devastation*, was at St. Thomas, but did not come into the harbour—keeping, however, only about a mile off the town—and remained intact."¹ Dr. Rush says of the epidemic of 1793: "I have heard of some seafaring people, who lived on board their vessels, who escaped the disease" (iii. 83). Dr. Caldwell remarks in reference to this matter: "Similar facts may be collected from the history of the fever in our own country. During the prevalence of that disease in Philadelphia, many individuals, and several whole families, are known to have retreated to vessels lying not more than from two hundred to two hundred and fifty yards from the wharves, and to have remained healthy. In New York and Baltimore, like instances have occurred."²

Dr. Lind states, in his well-known work on *Hot Climates*, that many persons escaped the yellow fever which prevailed in Pensacola in 1765, by retiring to the ships which lay in the harbour (p. 179). In another place, the author remarks: "When the violent sickness raged at Cadiz in 1764, it did not extend its influence to any ship which lay at a distance from the city" (p. 178). A similar occurrence has been noticed in Charleston (S. C.), where vessels at anchor in the stream, at a distance from the wharves, have been preserved. In 1807, when the fever prevailed extensively in that city, Danish and Hamburg vessels were kept healthy in that way, and by confining the crews on board. "Also, in 1824, in a Dutch ship which was at anchor in the middle of Cooper River, about three hundred yards from the wharves. Her crew were never permitted to come on shore while yellow fever prevailed; an awning was kept over her, and not one of them took sick, while every sailor that was liable on board of vessels lying at the wharves was taken with the fever."³ Dr. Ferguson calls attention to the circumstance that while the British troops were decimated at Cape Nicholas Mole (St. Domingo), the cruising squadron, comprehending eight sail of the line, with many smaller ships, was healthy; none suffered from yellow fever but those that were obliged to lie as guardships at the unwholesome anchorages of Port au Prince, Port Royal, Jamaica, and such like. And when, in the great naval campaign between Rodney and De Grasse, at the close of the revolutionary war, the decisive action of the 12th of April took place, the contending fleets were healthy, and yellow fever did not exist among them. Fleets have often remained healthy for years (at least free from yellow fever), unless when they get it at some unwholesome port in the West Indies; armies never.⁴

That this exemption is not the result of the avoidance of a contagious poison, we infer from the circumstance that a similar preservation is obtained in

¹ Wibbelen and Harvey on Yellow Fever, *Lancet*, April, 1853, p. 322, Am. ed.

² Prize Dissertation, *Boston Journal*, iii. 510.

³ Johnson on Yellow Fever, *Charleston Journal*, iv. 160.

⁴ *Recollections*, p. 151. See also Hector McLean, p. 26.

regard to other forms of fever, more or less allied to the one under consideration, and the non-contagiousness of which is unanimously admitted; and that other vessels become infected, as stated in a former chapter, by being placed within reach of wind blowing over the sickly locality, or by a change of position; and again becoming exempt by an alteration in the course of the former, or the interposition of some barrier. If vessels are preserved by these precautionary means from non-contagious malarial fevers, no proof of the contagiousness of the yellow fever can be deduced from the exemption of vessels which are kept at a short distance from infected localities, and the crews of which are prevented from going ashore. But, while such is the case, the absence of such a principle is fully made out by the fact that, at the very time that the disease is kept out of those vessels by placing them at a distance, and forbidding the crew to visit the shore, no injury accrues from the freest intercourse on board with persons from the infected place; that the crew take the fever by landing,¹ although they may not see, touch, or hold the most distant communication with the sick; and that, when so taken, and when they go through the disease on board, or when the sick from the infected place are carried there, no harm accrues to the rest of the ship's company, the fever not spreading, and ending with the death or recovery of the persons so affected.

Burnett adduces instances which show that, at Cadiz, Gibraltar, and other ports of Spain, individuals labouring under the yellow fever have been received with perfect safety on board of vessels, although the sick, on those occasions, were not separated from the rest of the crew. In the *Leyden*, a crowded troop ship lying in the Bay of Carthage, fifty-seven soldiers of the 67th regiment were attacked with the disease. They mostly remained on board. Only such men as, by going on shore, were exposed to the cause, were taken ill, and they did not communicate the disease to a single person (pp. 188, 224, 228, 276, 436, 438). Lind, in a passage already cited in reference to the fever of Cadiz in 1764, tells us that his Majesty's ship, the *Tweed*, which was then at anchor there, like others, escaped. All that were sent on board recovered, no bad symptoms appearing in the fever" (p. 178).

Similar statements are made by Dr. Chervin, in relation to the fever of Gibraltar in 1828, though then there were more than 2,500 men on board of vessels in the bay, sailors or refugees, and the non-intercourse between the infected place and these was but nominal.² This exemption is referred to by Dr. Smith, in his observations on the answers of Sir W. Pym to the queries of the Royal Medico-Chirurgical Society of Cadiz:³ "Several hundreds embarked on board of vessels in the bay, and as many, I believe, had free communication between the shore and shipping during the epidemic. No

¹ Trotter, i. 456, ii. 86; Bally, p. 455; Rouppe, p. 65; Caillot, p. 200; Valentin, pp. 75, 77; Hunter, p. 17; Gillespie, p. 20; J. Wilson, p. 66; Bancroft, p. 172; *Ib.*, Sequel, p. 166; Fontana, p. 12; Blane on Seamen, pp. 92, 392; Lind, pp. 106, 108, 134, 162, 195; *Ib.* on Seamen, pp. 73, 77; Bryan, p. 151; Burnett, pp. 188, 225, 268; Allon, *Edinburgh Monthly Journal*, Aug. 1847; Smith, *Edinb. Journ.*, xxxv. 13, 47, 50; Wilson, *Stat. of Brit. Navy*, p. 85; Bryson, *do.*, pp. 215, 220, 230.

² Letter of M. Monfalcon, p. 12.

³ *Edinb. Journ.*, xxxv. 13, 47, 48, 50.

precautionary measures whatever were adopted by the captain of the port. I myself had almost daily communication with a ship in the bay, for the purpose of visiting my commander's wife, who removed thither at the breaking out of the epidemic, yet the disease never spread among the shipping." "If any individuals sickened in consequence of communication with the shore, they never affected anybody else on board the same ship with them."¹ At Gibraltar, says Mr. Donnet, individuals labouring under marked symptoms of that disease, and conveyed on board of ships in the bay, go through it without infecting any of their attendants or neighbours. "This fact, I believe, was well ascertained during the autumnal fevers of 1813 and 1814."²

In 1808, while the ship *Gironde* was at anchor at the Saints near Guadeloupe, several of her crew went on board the *Eglantine* which had arrived from Fort Royal with the yellow fever on board. The next day, the *Gironde* sailed for the United States, and in the course of a very short time six of the men who had visited the *Eglantine* were attacked with the yellow fever, five of whom died from the fourth to the fifth day. Nevertheless, the disease was not communicated to any one on board.³ Again, from January to June, 1838, the *Dolphin*, a British vessel of war, was employed cruising in the Bight of Benin, and in refitting at Ascension in May; during that time there occurred altogether eight cases of fever, two of which terminated fatally. The most severe of the latter was contracted on the island when the epidemic was prevalent there; "and it is proper to observe, that the patient did not transmit the disease to any other person in the ship. The fever was of the nature of 'typhus icterodes,' and proved fatal on the fourteenth day."⁴

To this, let me add, that in all seaports within the yellow fever zone, individuals labouring under the disease have been landed without communicating it to any one—be it in private houses or public crowded establishments. Examples of the kind have been mentioned in a preceding section of the present chapter. They have occurred in all our cities. During the very last summer, vessels from Savannah and other southern cities have, as we have seen, furnished us with cases in this city and at our Lazaretto, and we are yet to learn that any evidence of contagion has resulted from the intercourse of these with persons around. When the *Growler*, English war steamer, after suffering much from the yellow fever on the coast of Africa, arrived at the Bermudas, she was placed in quarantine; the crew landed and kept apart—the sick from the well—and the vessel thoroughly cleansed and fumigated. Those from the shore, who were engaged on board in conducting the cleansing of the vessel, contracted the disease—for reasons already referred to; but it was not communicated to a family of five persons associated with the sick on shore.⁵

With facts of this kind, taken especially in connection with those that precede, the etiological inquirer may well think himself justified in having some misgivings in relation to the contagionsness of yellow fever. We have, on the one hand, exemption of ships from the disease so long as they remain

¹ Op. cit., p. 49.

³ Chervin, Rapport de l'Acad. de Méd., p. 10.

⁵ Davy, Edinb. Med. and Surg. Journ., lxxii. 282.

² Bancroft, Sequel, p. 166.

⁴ Bryson, p. 123.

at a certain distance from a sickly place—we have often disease on board when the wind reaches them after passing over said place. On the other hand, we have sickness among such of the crews of those vessels as visit the infected locality—whatever be the nature of the latter—and whether they visit the sick or remain completely aloof from them. Again, we find those vessels exempt from the disease, though daily visited by individuals from the infected districts. We find the sick carried ashore from vessels arriving from diseased ports, or at anchor near any place, proving harmless to all around—even when admitted in crowded hospitals. Finally, we find persons labouring under the fever received on board with perfect impunity, and going through the disease, to death or recovery, without infecting any of the ship's company, or their physicians or attendants. What more can we desire? Can such things be expected of a really contagious disease?

Not produced by Accumulation of Patients.—We have seen, in preceding chapters, that the disease when formed is very apt to assume a more severe and malignant character, when patients affected by it are kept in crowded rooms; that the cause, when evolved in localities containing an accumulation of population, is generally more virulent; but that accumulations of the kind cannot by themselves, and independently of other morbid agencies, produce the disease.

It has been seen also that the yellow fever is not communicated from the sick to the well in the wards of hospitals—whatever be the conditions in which such establishments are kept—that when these are situated beyond the reach of the local causes of infection which extend their baneful influence over all around, individuals who approach the sick there assembled, are in no greater danger than those who remain aloof; that cases which occur in hospitals placed within the area of the infected districts where the cause of the disease extends its baneful influence to those who are not exposed to the effluvium from the sick cannot, in justice, be referred to intercourse with the latter, but are due to the agency of the general cause pervading the atmosphere; and that, at furthest, attendance on, or residence in such hospitals can only act injuriously by exciting the disease in one who has already imbibed the seeds of it, or by aggravating its character. We may now go further and affirm that no accumulation of individuals labouring under the yellow fever, both in its early or closing stages, whatever be the nature of the circumstances under which they may be collected together, whatever the nature of the buildings where this accumulation takes place—be it a hospital, an asylum, a prison, a ship, or barrack, or what not—and whatever the condition of the establishment, in point of ventilation and cleanliness, has been known to promote the development of the disease in individuals exposed to the effluvia thereby created; in other words, to *generate* the efficient cause of the fever.

We have seen this exemplified in the hospital of Bush Hill, during the epidemics of 1793 and of subsequent years, as well as in kindred establishments in other places, both of tropical and temperate regions. Other instances of similar nature might be easily gathered from the histories of various

epidemics (*Ralph*, p. 59; *Savarésy*, p. 51), and especially of those which in 1795 and 1802 destroyed a great part of the English and French armies of St. Domingo, as recorded in the writings of Jackson,¹ Maclean (p. 20), Beaujeu, Mabit, Repey, Chervin (*Rep. in Ruzz*, p. 45), Vincent (p. 19), François (p. 31), Dalmas (p. 67), &c. As already stated, Dr. Lallemand, of Rio Janeiro, in his account of the epidemic of that city in 1850, shows the innocuousness of mere accumulation of fever cases in the Gamboa Hospital and the convent of Bours-Jesus (pp. 36, 37). Instances in large number are cited,² of vessels while in port being infected with the yellow fever, and in which the patients were accumulated together in narrow and but imperfectly ventilated localities, surrounded by their shipmates, nursed by their attendants, and visited by their officers. While thus conditioned, these vessels leave the infected place, and the fever, instead of continuing to show itself among the crowded company, diminishes, or even ceases. Now, as Dr. Ferguson³ remarks, here is the supposed contagion fairly impounded without escape, and one would suppose that the disease would there do its worst; yet it does not. It uniformly stops, the sick recover every day she sails to the northward, it even often stops when the vessel remains at sea within the tropics, and those who are in health at the time of sailing remain free to the last, though exposed to the accumulated exhalations.

Facts, it is true, show that from such an exposure fever may result, and has resulted; but experience shows also that the fever arising under such a condition of things is not the one forming the subject of this inquiry, but that form of febrile disease which is generally due to animal effluvia exhaled under particular conditions from living bodies in health or disease. Every tyro in medicine knows full well that crowding persons in health, without regard to the preservation of cleanliness and to free ventilation, will almost inevitably give rise to that disease, as is so often exemplified by its generation in ships, prisons, &c. And we know, also, that the effects will be more certainly produced if individuals so circumstanced are labouring under disease of whatever kind. Such being the result of the accumulation of individuals in health and disease, there can be no reason, *à priori*, to doubt that the crowding of yellow fever patients in wards defective in point of ventilation and cleanliness, may occasion similar effects. But we are not left to conjecture on the subject. The reality of the effect has been noticed in this country, and pointed out by Dr. Chervin,⁴ Dr. Dickinson,⁵ and others, and is particularly insisted upon by Dr. Jackson, in his admirable work on the yellow fever of Spain, as may be seen by the following passage, which, from its apposite-ness, and coming, as it does, from one so experienced in the matter, I shall make no apology for transcribing in this place. "It is necessary to mention

¹ Sketch, i. 27; Ibid., An Outline of the Hist. and Cure of Fever, p. 49, &c.

² Lind on Hot Climates, pp. 89, 120, 133; Ib., on Seamen, p. 85; Bryson, p. 228; Savarésy, p. 153; Trotter, i. 358, ii. 100.

³ Recollections, p. 143.

⁴ Examen des Nouvelles Opinions de M. le Dr. Lassis, pp. 5, 6 (note).

⁵ Observations on the Inflammatory Endemic, &c., pp. 102.

what may be: if a vessel, for instance, should happen to have a great number of persons ill of the yellow fever in its passage from the West Indies, it is possible, and even probable that, by throwing these persons together in the mass, in ill-ventilated apartments—as the 'tween decks of a ship, even of a ship of war, must necessarily be—the contagion, which belongs to an atmosphere corrupted by accumulation of living subjects in narrow space, can scarcely fail to be generated; a contagious fever will thus be produced and carried to whatever port the vessel may be destined to go. This is within the circle of possibility; and it has probably sometimes occurred. The imported fever may be contagious; but it is not the yellow fever, though the yellow fever may be the source from whence it originally springs" (pp. 8, 9).

What is here said of the accumulation in a ship is applicable to that in any other building. If fever is at all generated, it will be that form of it resulting from the undue assemblage of human beings labouring under any disease, or under no disease. But it will not be yellow fever. If the latter disease prevails under such circumstances in localities of the kind—if it attacks those who enter or reside there, and who mix with, or attend on, or lay sick among patients labouring under it—the effect is not to be ascribed to the morbid influence of the latter; but to the circumstance that the place where the occurrence is noted is situated within range of the yellow fever cause—affecting as well those who keep aloof from the place of the accumulation as those who are in the midst of it; for, by removing those sources of concentration to some uninfected locality, or by destroying the febrile cause existing and exercising its morbid effects around it—the accumulation remaining the same—a stop is often put to the fever. In most epidemics, there is a period at which a greater or less accumulation of cases occurs, either in receptacles of the sick or even in private dwellings; and yet, nevertheless, we do not find that the disease proves more contagious than it did before, or than it does when this state of things decreases. Indeed, it often happens in temperate latitudes, when the autumnal months are cool and even cold, that this accumulation takes place under circumstances which could render the contagion, did it exist, more virulent, and tend to its diffusion. "If the yellow fever be a contagious disease," says Dr. Caldwell, "whence is it that five hundred persons labouring under it in November cannot communicate it, in a single instance, to any of their attendants, while in the month of August fifty or even ten individuals affected by it diffused it through a whole community?"¹ These five hundred, too, being placed in closed rooms heated to a summer or mild autumnal temperature by means of fires.

Dr. Ferguson, while acknowledging that there may be a peculiar aptitude in the moist soil and climate of the British Isles for the generation and nurture of the typhoid poison, remarks that this property of generating the latter is not exclusively peculiar to the insular soil. It is common to the human race whenever the circumstances requisite for its formation can be brought into full operation and combination. Experience taught him that even the

¹ Med. and Phys. Mem. (1826), p. 124.

high temperature and peculiar atmospheric condition of the tropics, however soon and certainly it may dissipate the principle, is not altogether proof against its generation; and in illustration says: "The fever on board of an infected vessel from Trinidad to Barbadoes, from crowding below decks at sea, ceased to be yellow fever, and became as truly typhoid as any I ever saw."¹

If no accumulation of yellow fever patients can occasion the disease in those who are exposed to the atmosphere of places where such accumulations exist, unless such places are situated in infected localities, and if in some instances the effects are due not to the influence of the effluvia issuing from those patients, but of the febrile poison evolved or existing in the atmosphere of the locality, it is not to be inferred that no deleterious agency is exercised by such accumulations, when they occur in infected districts. So far is this from being the case, there can be no doubt that individuals living in the midst of the deleterious atmosphere of such places, or exposed to it more or less frequently, run greater risk of infection than if placed beyond the reach of that tainted atmosphere. But in such cases the disease has merely been localized in places where the accumulations exist, whereby the system of those thus crowded together, or breathing the tainted atmosphere, is rendered more susceptible to the impression of the efficient febrile cause, which otherwise would possibly have failed to strike them. The whole effect would be produced, in precisely the same way, were the individuals so accumulated labouring under other diseases, or in the enjoyment of health.

Yellow fever is, in that respect, on an equal footing with other zymotic diseases arising from special poisons. Whether the latter float in the atmosphere, and are independent, for their development, of any organic process; whether they are formed in the system, and are transmissible from one individual to another, through means of contact or the medium of the atmosphere; or whether they proceed from external sources of animal or vegetable decomposition, matters not: they are all rendered more prolific of their special and specific effects, and exhibit greater virulence, in individuals exposed to the influence of a tainted atmosphere resulting from the accumulation of human beings, in health or disease. On this principle it is that the disease is always more rife and of worse character in crowded districts, or houses, or ships, and especially when the accumulation is associated with insufficient ventilation, filth, &c. On this principle, too, it is that in tropical climates the appearance of yellow fever has often coincided with the arrival of large bodies of soldiers and seamen from northern latitudes, and that, in such contingencies, the disease spreads more extensively among them than under different circumstances; for, from the fact of their unacclimatization, and the state of predisposition produced by their habits and mode of living, those men become doubly susceptible to the fever through the effect of the impure air of their crowded camps and ships.

In all such cases, I repeat, the disease is not the product of the trans-

¹ See *Recollection*, p. 168; *Bancroft's Sequel*, p. 228, &c.

mission of a contagious poison from one man to another. They all take it from the place where they land, and would not be affected had the disembarkation taken place in a salubrious locality. But on this subject I have already sufficiently enlarged, and need not, therefore, pursue it here; and conclude with the remark that no accumulation of persons, in health or disease, can *generate* the yellow fever among those thus crowded together, or individuals by whom they may be more or less frequently visited; and that the occurrence of cases under those circumstances indicates the existence of a cause independent of the effluvia resulting from the mere effect of the accumulation.

CHAPTER XXVI.

PROOFS OF NON-CONTAGION—CONTINUED.

Not communicated by Fomites.—At an early period in the history of the epidemics of Philadelphia, the greater number of our physicians, and the public at large, were of opinion that the bedding, clothes, and furniture of individuals who laboured under the yellow fever, or who had died of it, constituted a frequent medium of communication of the disease to those exposed to the exhalations arising therefrom; and we have already seen that some of the early outbreaks of the fever were ascribed to the importation of chests of clothes or the like from the West Indies. In conformity with this opinion, which prevailed everywhere, and continues to be entertained by many in this and other countries, it was customary to destroy every article that had been made use of by the sick, to scour or varnish the furniture, scrape and fumigate the rooms, or to close these up until the accession of frost.

That contagious diseases may be, and are often communicated through these means, is a fact I shall not attempt to disprove; and there can be no doubt that the transmissibility of the yellow fever in this way, if well authenticated, would constitute a strong presumptive evidence in favour of its contagious character. But it is equally certain, that the number of diseases so communicated is more limited than was formerly thought; and experience has amply shown that, however this may be as regards other febrile complaints reported by some to be contagious, the transmission of the yellow fever through the medium of clothing, bedding, &c., if not disproved in a way satisfactory to all etiological inquirers, is at least far from being sufficiently well made out to be adduced in proof of the contagious character of the disease. The result of observations on the oriental plague leaves little room to doubt the non-transmissibility of the disease in that way—facts innumerable being adduced to show, contrary to the statements of Russel, Astruc, Diemerbroeck, &c., that the clothing, bedding, and furniture of the sick, have, even when not purified, been used and handled with perfect impu-

nity; while the strongest cases offered in modern times in support of that mode of communication, are open to so many objections as to lose all importance in the minds of strict inquirers.¹

In relation to the yellow fever, we find so many instances establishing the fact of the non-transmissibility of the disease through the agency of articles of the kind mentioned, and of merchandise generally, that we cannot but discredit the accounts of a contrary character assigned in medical writings, and still more so those presented on the strength of popular report solely. For if, in a large number of well authenticated cases, such articles have been handled and used with perfect impunity—and that, too, often under circumstances best calculated to insure the effect in question—we have every reason to conclude, that a contrary result will not be obtained in other instances of a similar kind; and that, consequently, the effect said to have been produced by exposure to those articles, must—unless established beyond the possibility of doubt—be referred to some other agency. Now, that they have not been so established, and that no injurious effect has resulted from exposure to contaminated objects in this and other countries, it is easy to show.

Referring to the writings of Chervin, Bancroft, and others, for ample proofs of the little credit to be attached to many statements of transmission, in this way, adduced by the contagionists of Europe and the West Indies—Pariset, Audouard, Pym, Robert, &c.—and postponing, for the present, those mentioned by American writers—I will limit myself to the remark, that in this city, in this neighbourhood, no case of propagation of the fever by the means alluded to, has been satisfactorily made out. In every instance cited, we have mere assertions or surmises, while the attack may invariably be traced to the influence of the general cause floating in the atmosphere, and affecting individuals who have not approached the sick, or their clothes or bedding, or of particular sources of infection amply adequate by themselves to produce the effect in question.

On the other hand, the harmlessness of the clothing, bedding, &c., of individuals who have had the yellow fever, has been demonstrated in hundreds of cases, and recorded by high medical authorities. In speaking of the fever of Carthagena, Burnett remarks: “I have before mentioned that the disease ceased on the 23d of January, 1805, on which day the last patient was received into the Royal Hospital. On the 5th of February following, a French frigate, having on board the wounded of the Arrow sloop, which she had captured a few days before, after a gallant resistance, arrived in the harbour; the wounded were immediately landed, and placed in the same bed and bedding in which several patients, who had died of the prevailing fever, had lain, without the bed or bedding having either been washed or aired; yet not one of these men was attacked with the disease in consequence” (p. 273).

It is recorded in the manifesto of a free association of fifteen native and

¹ Clot Bey, pp. 309, &c.; Report of the Acad. of Med., pp. 103, 160, &c.; Aubert Roche, *De la Pest ou Typhus d'Orient*, pp. 67, &c.; *Ib.*, *De la Réforme des Quarantines*; Brayer, *Neuf aux à Constantinople*, ii. 354, &c.; Schnurrer, *Faits recueillis pour servir à l'Histoire des Mal. Epid.*, translated by Gasc, pp. 61, &c.

foreign physicians at Barcelona, in 1821, that "the daily traffic of carriages, which had conveyed sick persons furtively introduced, or mattresses, linen, clothes, and furniture, taken from the very focus of infection, did not transport the malady beyond the limits which had been assigned to it."¹ "Many persons," continues the manifesto, "have inhabited apartments in which pestiferous patients have perished, without their having been whitewashed; others have slept in beds in which pestiferous patients had perished, without causing the mattresses to be washed or cleaned in any way; and others have used their clothes and linen, without purification; and there is not a single example of a disease being communicated in this way, which disappears at a certain period."²

The clothes of more than twelve hundred persons who died in the *Seminarium* were almost all stolen from the store-room in which they had been deposited, but the thieves do not appear to have been infected by these means.³ From a document furnished to Dr. Chervin by a number of the upholsterers of Barcelona, we learn that, during the epidemic, the latter washed, or caused to be washed, at the Lazaretto, from eight to ten thousand mattresses, and a proportionate number of pillows, sheets, curtains, and other articles used by individuals who had passed through the disease; that these articles were often soiled with blood, black vomit, or excrement; that frequently the *carmen* who carried these articles to the Lazaretto took them from under bodies still warm; that the individuals who washed them lay, slept, and ate on these contaminated mattresses, and handled them in every way possible; and that, nevertheless, they in no instance experienced any injury from so doing—that none of the women who washed the sheets, curtains, linen, &c., none of the before-mentioned *carmen*, and no members of the families of the upholsterers had the disease.⁴

Other cases, equally conclusive, are mentioned in sundry documents cited in the work from which the above is taken. Palloni, though a decided contagionist, states that "there cannot be adduced a single instance in which the infection was conveyed by substances which had been in contact with the sick; and there were many examples of individuals and whole families having continued to inhabit the room or to sleep in the very bed where a patient had died." Palloni adds that the "contagion" was never conveyed by goods of any kind.⁵

At Villa Franquesa—also called Palamo—where all the clothes of Alicante are washed, no injury resulted from the operation, which was uninterrupted during the continuance of the epidemic of that city.⁶

In the West Indies, cases of the same kind have often presented themselves. Take the following as an example. Mr. Ralph, in his account of the fever among the troops at Barbadoes in 1816, states that, "on the 10th of November, it was determined to have the whole of the married families re-

¹ Maclean, p. 133.

² *Ibid.*, p. 136.

³ Rochoux, p. 159.

⁴ Report of the Academy of Med., p. 78.

⁵ Osservazioni, &c., p. 30. See also *Edinb. Med. and Surg. Journ.*, ii. 90.

⁶ Bally, p. 449.

moved to a certain building situated on a dry, sandy soil." "The beneficial effects which were the consequence of the adoption of this measure were speedily rendered evident by the disorder ceasing in a class of individuals who had previously been particularly prone to its attacks. They carried with them their bedding, and every article of apparel belonging to many who had fallen victims to the disease; and surely, had these articles been infected, the disease would have continued its destructive career; but as the cause of it was purely *local*, removal to another situation, differently circumstanced as to soil, &c., afforded a release from this terrific scourge. Unfortunately for those of the troops who lived in the barracks, they remained in the focus of the pestiferous emanations, and continued there to suffer most severely."¹

M. Joubert, who has given an interesting account of the fever which prevailed on board the French war steamer *Gomer*, informs us that eight men of the crew were continually employed in preparing the baths, washing the sheets and linen of the sick, and doing up the mattresses of those who had died of the disease. No precautionary measures were employed in regard to them, because, as M. Joubert remarks, they were not regarded as being in greater danger than the rest of the ship's company. Well, while two-thirds of the crew were attacked with the disease, only one of those men was affected, and in him the fever was evidently brought on by excess of drink and a prolonged exposure to the action of the sun. To dissipate the panic which had seized upon the men, one of the surgeons of the vessel—the only one who was untouched by the fever—deliberately lay in the bed of an individual who had just died of the disease, and slept there all night.

In Bermuda, during the epidemic of 1843, "the shirts, flannels, sheets, pillow-cases, coverlets, and occasionally the sacking and hair of the beds, were all washed by females belonging to the establishment." Not one article was destroyed, and yet no injury resulted.²

Dr. Blair states, in regard to the epidemic of Demerara: "Washing is, and always has been, done for the Colonial Hospital by contract. The contractor is Mr. Fr. Hopkinson. Before 1840, Mr. H. contracted for the clothes washing of both hospitals, and, besides the assistance of his own family, required eight people constantly at work. After 1840, the women were employed separately for the Seamen's Hospital, and then only six assistants, besides his own family, were required by him; and the same numbers respectively continue till now. The individual washers were often changed during the period of the epidemic, but none of them were ever affected by the prevalent disease."³ In his work on the same epidemic, Dr. Blair relates the following case: In December, 1843, the mate of the *Matilda Luckie* was admitted (into the hospital) with the *gravior* form of the disease, and of a low type, of which he died. His bed was in a sheltered corner of Ward No. 2, and had mosquito netting all around. Into this bed a seaman, who was admitted for disease other than yellow fever—slight indisposition—was

¹ Report, contained in Bancroft, Seq., p. 450.

² King's Report on the Fever of Boa Vista, p. 14.

³ Brit. and For. Med.-Chir. Rev., vii. 556.

put for several days, without any infection of any kind following. Neither was such an experiment deemed hazardous to the subject, nor objectionable, except on the score of cleanliness (p. 55).

A great deal has been said about the agency of the clothes of the officers and men of the *Eclair* in introducing the yellow fever into the island of Boa Vista in 1845. Mr. Jamieson, as it would appear, brought ashore in his boat at least a dozen bags of soiled linen belonging to the officers of the ship, which he deposited in the consul's store-rooms for the night, and gave to the washerwomen of the town next morning. The number of these is differently stated. According to Dr. McWilliams's list, they amounted to seventeen; while in Dr. King's report the list contains nineteen names (p. 16). Dr. King, on this subject, says: "The women who washed the officers' clothes were islanders, black and coloured. The soiled clothes—linen, cotton, and flannel—which had accumulated in the cabins from the time of departure from Sierra Leone, were contained in twelve bags. Now," continues Dr. King, "if the disease possesses the power of reproduction, its poison must have been as certainly communicated through the medium of fomites as by direct contact with the sick on board or at the fort." But what was the result? All these women, with the exception of an old negress, were attacked with fever, of which four died. Dr. McWilliams, at page 82 of his report, informs us that he examined the whole of the survivors, and found that two were attacked late in October, five in November, two in December, three in January, and one not until some time in February. The *Eclair*, it should be remarked, arrived on the 21st of August, and the first well-marked cases on shore occurred on the 17th of September. The epidemic, therefore, cannot be laid to the score of the foul linen, as an interval of some seventy days elapsed from the time of exposure to the presumed contagion to the period of attack. It may be added that all the other members of their families recovered.

This absence of connection between the appearance of the fever and the foul clothes would seem, indeed, to be admitted by Dr. McWilliams himself; for he says: "None of the deaths took place until fever was general in Porto Sal Rey; so that in none of these cases can the occurrence of the fever be fairly attributed to infectious matter conveyed by the linen" (*ib.*). Dr. King, in his more recent work, remarks on this passage: "Yet, notwithstanding Dr. McWilliams's late attempt to throw overboard the foul linen, I affirm that these women not only washed the linen, &c., of persons who had been ill with fever on board the *Eclair*, but also of those who were then actually labouring under the disease at Boa Vista. Will the contagionists account for this anomaly in the laws of contagion? They cannot. The disease, then, was not communicated through fomites; and I point to the fact (which is manifested from the evidence) that among the washerwomen and their families, amounting to eighty-four persons, four deaths only occurred, and those not until November, December, and January."¹

¹ The Fever of Boa Vista in 1845-'6, &c., p. 17.

No less positive and satisfactory, on the same subject, is the testimony of Drs. Belloc, Vatable, Allenet, Bouquin, Chopitre, at Guadaloupe, Castro, at the Havana,¹ Lefort (p. 33), Savarésy (pp. 151, 152), Dariste, at Martinique (pp. 42, 43), Stewart,² at Grenada, and Pennell, at Rio Janeiro.³

If now we turn our inquiries to the result of experience in this city, we shall find it, as regards the innocuousness of objects in question, similar to that we have noted elsewhere. Dr. Mease communicated the following fact to Dr. Rush. While he resided at the Lazaretto, as inspector of sickly vessels, between May, 1794, and the same month in 1798, the clothing contained in the chests and trunks of all the seamen and others belonging to Philadelphia, who had died of yellow fever in the West Indies or on their passage home, and the linen of all the persons who had been sent from the city to the Lazaretto with that disease, amounting in all to more than one hundred, were opened, exposed to the air, and washed by the family of the steward of the hospital, and yet not one of these persons contracted the least indisposition from them.⁴ Dr. Lehman, who for several years held the same appointment, testified similarly in a document furnished to Dr. Chervin.⁵ The late Dr. Monges, of this city, who saw undoubtedly as much of yellow fever as any physician among us, remarks that he constantly reprobated the practice of burning the clothes and bedding of the dead, and never found any bad effects to follow to those who observed his advice.⁶ Dr. Ffirth states that he saw friends and attendants sleep in the very beds with patients suffering under yellow fever. This, he says, occurred to him not once or twice, but a number of times. After the death and burial of the patient, the bed was used by the family without being cleansed; yet none of them took the disease.⁷ The same results were noticed in 1805 and subsequent years, including 1853 and 1854.⁸ Dr. Deveze, in his treatise on the yellow fever, communicates a fact which, corroborating in the most striking manner the correctness of the views under consideration, deserves a particular notice in this place. We have already seen that during the height of the disastrous epidemic of 1793 a vessel arrived from St. Domingo with a considerable number of sick and wounded on board, and that these were conveyed to Bush Hill Hospital and quartered in huts erected around the main building. We have also seen that as a bed became vacant in the wards it was filled by one of those individuals, and that nevertheless the disease was not thereby communicated. But this was not all. At the close of the epidemic, the Committee of Health of Philadelphia sold to the administration of the French government the beds, effects, utensils—in a word, all that had been used, during the course of the epidemic, by the sick. The French being already in possession of them, it was found impossible to have them washed in the ordinary way; and yet the

¹ Report of the Acad., pp. 30, 31.

² Letter to Dr. Hosack, Reg., iii. 188.

³ New Orleans Rep. for 1853, p. 157.

⁴ Rush, iv. 146.

⁵ Report, p. 30.

⁶ North Am. Med. and Surg. Journ., ii. 63.

⁷ A Treatise on Malignant Fever, &c., p. 51.

⁸ Caldwell, Fever of 1805, p. 68; Essays (1826), p. 131.

yellow fever did not reappear. It did not break out again the following years, though the disease prevailed in Philadelphia, and the French soldiers continued to use the same articles with perfect impunity (p. 222).¹

“When the sick were brought from the Peacock to the hospital near Pensacola,” says Dr. Barrington, “their mattresses, blankets, hammocks, and bags, as they came from the infected ship, were regularly taken by the attendants to be aired, and their clothes to be washed. The beds and bedding of those who died were immediately removed by the same persons, and not in a single instance was any one connected with the hospital attacked with the disease.”²

Other instances of a similar kind can easily be found in the writings of Valentin (p. 93), Miller (pp. 108, 109), Vaughan,³ Wheaton,⁴ and others;⁵ but the preceding must suffice. Taken in connection with the results of experiments instituted purposely to ascertain the point under consideration,⁶ they are certainly well calculated to throw some light on the question of the non-contagiousness of the yellow fever, and to create

¹ This fact seems to have annoyed Sir William Pym, for, in commenting on Dr. Bancroft—who derived it from Dr. Valentin—for having adduced it as a proof of non-contagion, he remarks that, if such an occurrence really did take place, it was certainly a disgrace to civilized society; and adds that, in palliation of Dr. Deveze, he is more inclined to suppose that, in the heat of argument against contagion, that physician mentioned the circumstance to Dr. Valentin as possible (if put to the test), than to suppose that he or any other medical man could be guilty of so disgraceful and disgusting an act towards any class of individuals intrusted to his care (p. 197). Sir William Pym cannot, however, find, even in supposing the fact to be true, that it militates against his favourite doctrine, inasmuch as the disease had actually ceased, as usual, at the approach of winter, before the bedding in question was sold; and there surely can be nothing very singular in this contagion or disease not attacking Frenchmen after having ceased its ravages upon Americans, or, in fine, after its existence was at an end (p. 196). It would certainly have been much more wonderful and inexplicable if it had.

In relation to these surmises and attempts at explanations it may be remarked that nothing in the language of Valentin justifies the supposition that the occurrence in question was mentioned to him by Deveze, in the heat of argument, as possible (if put to the test). So far from this, Valentin (p. 93) refers to it as a fact publicly stated by Deveze, who, in reality, adduced it in the second edition of his essay (p. 51) on the epidemic in 1793, published ten years before the appearance of Sir W. Pym’s work. The occurrence, therefore, so far as we can rely on the testimony of Deveze—and no one has as yet impeached it—was real; and whether or not disgraceful and disgusting, as it is pronounced to be by Sir W., must be viewed as serving the purpose for which it was intended, and cannot be rejected by that author and other contagionists, inasmuch as, if at the time of the sale the epidemic was over, there is no reason why the contagious virus with which these articles were contaminated should not have produced an injurious effect at the period of the recurrence of the fever, since, according to them and others, the disease often remains dormant during a whole winter, to revive and break out afresh the next summer.*

² Barrington, *Am. Journ.*, xii. 310.

³ *Med. Reposit.*, iv. 243.

⁴ *Ibid.*, x. 336.

⁵ *Ibid.*, ii. 299.

⁶ Guyon, in Lefort, pp. 125, 126; Ffirth, p. 50; Caldwell, *Essays* (1826), p. 128.

* See Pym, p. 64; Fellowes, p. 177; Bancroft, *Seq.*, p. 115.

some degree of suspicion as to the value to be attached to instances adduced in proof of the instrumentality of clothes, bedding, and other personal effects, merchandise, &c., in transmitting the disease, and to the inferences drawn from them. The experience furnished by those facts is on a large scale. In some instances we find clothes, bedding, &c., in quantities sufficient to infect a whole city, did they possess the power of doing so, foul to excess, and stained with the matter of perspiration and the ejections of the sick, washed, handled, and slept on, with perfect impunity, out of the infected district; often under circumstances best calculated to insure the transmission of a contagious poison. In others, we find similar articles, in similar condition, similarly treated in infected localities, without giving rise to a greater amount of sickness among individuals who wash or use them than is experienced by others in the neighbourhood, who have nothing to do with those articles. In some, indeed, we find that individuals who have washed or used such articles have suffered in less proportion than the rest of the community. These facts have been observed in various latitudes and places—in public establishments, in private houses, and in ships. Everywhere the same results have been obtained. Surely if, in such instances and under the circumstances mentioned, no evidence of communicability has been observed—if nothing like a show of contagion has been noticed—we may well infer that in the records of those cases of a contrary kind, which are referred to by contagionists, some error has crept in—something has been omitted or overlooked—and that the production of the disease was really due to some other agency than the one contended for.

When we find that hundreds of sheets, blankets, and other personal effects that have been in use by the sick and dying—when we find that loads of merchandise in bales or boxes, have so frequently proved innocuous, we can scarcely feel disposed to admit that the disease has been transmitted to a healthy locality by the clothes of a single individual who has laboured under it, by a peddler's pack, or a box of hardware or crockery. Still less can we admit the possibility of the occurrence when such clothes have not been worn or used by the sick, and have merely arrived from an infected locality. Nor is it possible to be convinced of the reality of the transmission, when the result in question has been obtained in the midst of the epidemic influence. Most of the cases cited by Pym, Pariset, Fellowes, and Arejula, in Europe, and by many of our own writers, are embraced in the latter category; while such as are said to have occurred beyond the limits of the infected localities will not bear examination, or are so improbable as to excite our astonishment at their being adduced by writers of respectable standing in the profession.

The case of Dr. Valli, cited in a preceding chapter, and on which much stress has been laid, will not be thought to go far in proof of contagion when it is borne in mind that the enthusiastic Italian made the experiment—which, it is maintained, cost him his life—in the Hospital of the Havana; that this establishment is situated within the limits of the epidemic influence; that he was unacclimatized; and that he had, besides, committed imprudences which greatly increased his susceptibility to the impression of the efficient cause.

“Contagionists,” says Dr. Dowler, “have, during this as well as during all former epidemics, collected facts to prove their theory. A peddler, from an infected district, arrives in a town—his pack is opened—he, the family, and many of the villagers die of the yellow fever. Exactly the same occurrences (which are mere coincidences) take place a hundred times, where there has been no peddler, no box of goods opened, no traveller from the infected districts. In one town, a crate of crockery from New Orleans is said to have been the medium of transmitting the contagion to the village. In another a bale of oakum, a box of axes, a box of children’s shoes, and a paper package containing India rubber coats, carried from a village where they had remained but three days—without being unpacked—were seriously thought to have been the source of mischief. But at that very time, nearly all the other towns for five hundred miles around were falling under the malignant influence of the epidemic. It would be most extraordinary if crates, boxes, passengers, and pestilence should never happen to get together—not as causes and effects—but as coincidences, necessary in the ordinary course of business. If the pestilence got into towns before the arrival of a bale of goods, the former did not cause the arrival of the latter. If the man who opens the goods dies of black vomit, together with all his family, a hundred other families take the disease without any such apparent exposure, and die in like manner.”¹ And, as Dr. Dowler might have added, thousands open boxes of goods, or trunks of clothes, in this country and elsewhere, without experiencing any injurious consequences.

Not communicated by handling the sick or the dead, and by dissection of the latter.—From what has been stated relative to the harmlessness of intercourse with the sick in hospitals and elsewhere, the reader must be in some measure prepared for the statement, that the danger is not enhanced by direct contact, by handling the sick or dead, or by post-mortem examinations. In places situated beyond the epidemic influences, or in the very centre of infected localities, friends or attendants who touch the bodies of persons labouring under the disease—physicians who examine them in every possible way, dress their blisters, &c., and dissect them after death—run no more risk than those who keep aloof, and abstain from all exposures of the kind.

In his answer to Mr. Frazer, Mr. Martindale says: “If the fever were contagious, it is reasonable to suppose that orderlies in the hospital, who constantly attend the men attacked with it, touching their bodies, their bed-clothes, &c., removing their excrements, and breathing the same atmosphere day and night, would, by being thus exposed to the worst source of the contagion, be liable to take a fever of the highest degree of malignity; but, out of seven attacked with the fever, not one died, and only one had it severely. Medical officers, also, are constantly exposed to this contagion, if it may be so called, not only during the lifetime, but after the death of the patients—but no one took the disease from opening a dead body. I myself opened several, and once cut my finger in doing so, but no consequence followed

¹ Tableau of the Yellow Fever of 1853, &c., pp. 53-4.

and of the medical officers taken ill, not one died. Both of these descriptions of persons are exposed to the local causes of this fever, and it is much more probable that they took it in this manner than by contagion."¹

Nowhere do we find that physicians, nurses, and attendants are particularly liable to the disease. If the patients are placed beyond the limits of the epidemic influence, none around—however closely in contact they may be—are attacked. If the sufferer remains in the infected district, his medical and other attendants are not more affected than if they kept aloof. The same immunity is experienced by those who dissect the dead. The number of physicians in Georgetown (Demerara) in 1843 was sixty-five. Only two of them suffered. "The number, of course, fluctuated during the eight years of the epidemic, some dying on leaving the colony and being replaced by others; and sixty-five, therefore, represent the number of only one year who were exposed, while three represent the number who suffered during the whole period."² Dr. Frazer, in a communication commented upon by Dr. Blair, remarks: "The mortality has been great amongst the clergy." To this Dr. Blair adds: "This is quite true. But the inference he would draw is not. Here, again, those *most* exposed, suffered *least* from the malady; and *vice versa*. Those of the metropolitan clergy whose avocation was among the stricken, and the susceptible, passed through the ordeal least harmed; and those whose congregations were chiefly of the negro race—such as London missionaries and those of the rural parishes of the Scotch church—suffered severely."³

We have already seen that three only of the eleven men attending the sick in Bermuda in 1843 suffered from the disease, and they had it in a mild form; that one-half of the servants escaped, and that only *one* out of seventy-seven attendants died. Let it not be forgotten, also, that after visiting the sick—handling, feeling, examining them in all possible ways—the medical gentlemen assembled in the operating room, and inspected the bodies of those who had died in the night, and that altogether they made seventy-two minute post-mortem examinations. (*King*, p. 14.) The occurrences at Leghorn were much of the same character. Of the priests who attended the sick, only one died; of the numerous medical practitioners, only two or three. There were many examples of wives sleeping with their sick husbands without being infected; of numerous families in which only one suffered; of children sucking their mothers till within a few minutes of their death—the whole with perfect impunity. (*Palloni*, p. 31.)

Similar results, from direct and frequent contact with the sick and dead, and from dissection, have been noticed by other physicians in Europe, in tropical regions, and are notorious in various parts of this country from Maine to Louisiana.⁴ With five or six exceptions, all the physicians of the United

¹ Bancroft, Sequel, p. 393.

² Blair, Brit. and For. Med.-Chir. Rev., vii. 557.

³ Ibid., p. 558.

⁴ François, p. 9; Rochoux, F. J. des Antilles, p. 315; Chervin, Rapport, pp. 24, 27-8-30, 72-3, 6; O'Halloran, pp. 35, 88, 95; R. Jackson, Y. F. of Spain, p. 38; Chervin, F. J. d'Espagne, pp. 65, 66; Mem. addressed to the Queen of Etruria on the Fever of Leghorn, in Devezé, p. 231; Doughty, pp. 49, 196; Ferguson, Med.-Chir. Tr., viii. 143;

States who furnished certificates to Dr. Chervin—those of Portland, Portsmouth, Newport, New London, Hartford, Middletown, New Haven, New York, Newark, Germantown, Philadelphia, Wilmington (Del.), Washington, Alexandria, Baltimore, Norfolk, Wilmington (N. C.), Smithfield, Charleston, Savannah, and New Orleans—agree on this point. (*Rapport*, p. 24.)

Dr. Bone was in charge of the quarantine hospital of St. Andero, in Spain, in 1813. Fifty of the patients had yellow fever. Of these, eleven died, and were all carefully dissected by him or his assistants; yet none of them caught the disease, nor any of the other patients in the quarantine hospital, nor any of the hospital servants or washerwomen, nor any of the patients or servants in the hospital from which the yellow fever patients were taken. He calculated that 700 persons had been exposed to the influence of the disease—yet none caught yellow fever. The cordon of troops did their duty—the British strictly, the Spanish with ferocity—but could not prevent all intercourse with the quarantine hospital. One evening, when one of his assistants, Mr. Williams, made his visit to the female ward, he discovered that one of the female patients had her sweetheart, a sergeant from the depot, in bed with her. She was then yellow as an orange, and the disease was running its course, but the sergeant did not catch yellow fever. There was in the same ward another female who contrived to see her sweetheart while she was affected with the disease, and he did not catch yellow fever.¹

Everywhere, I repeat, persons of both sexes and all ages have been placed in the closest approximation—have slept several successive nights with patients labouring under the yellow fever, yet without contracting the latter. What renders the fact still more striking is, that it has been principally observed in necessitous families, in which, consequently, the apartments were small, foul, and ill-ventilated. The bodies of the sick have been touched, turned, and carried from place to place—the dead have been washed, inhumed, without communicating the disease. Physicians, surgeons, and others, have received the matter of the black vomit on their clothes, hands, and face, without suffering from it. Physicians and surgeons have prosecuted their autopsic researches on a large scale, with impunity—immersing their hands in the fluids, and breathing the fetid exhalations issuing from the stomach and intestinal tube. Even exhumation has been practised without injurious results. In inspecting the bodies of the dead, they have cut or otherwise injured themselves; and even immersed their hands, so injured, in the different fluids, without bad effects. Some have gone further, and inoculated themselves with the blood, the serum, and the black vomit; or, sacrificing their feelings, have swallowed portions of this fluid.²

Dariste, p. xxvi.; W. Humboldt, in *N. O. Sanit. Com.* 1853, p. 130; Savarésy, pp. 151, 153; Dalmas, p. 67; Lefort, pp. 19, 31, 35, 36; Bally, p. 241; Joubert, p. 963; *Med. Reposit.*, ii. 299, iv. 5, vii. 183; Valentin, p. 92; Barcelona Manifesto, in Maclean on *Evils of Quarantines*, p. 135; *Edinb. J.*, xxxv. 378; Potter, p. 52; E. Miller, p. 109; Lallemand, *op. cit.*, pp. 36, 37.

¹ Bone, *Diss. on Yellow Fever*, pp. 19, 20.

² Guyot, in Lefort, pp. 126, &c.; Doughty, p. 103; Blair, p. 55; Bryson, p. 55; *Rapport de l'Acad.*, pp. 29, 30; *Edinb. Journ.*, xxxv. 378.

What has occurred in Europe, in the West Indies, and in various parts of the United States, has been observed in this city from the earliest to the last of our epidemics. Nurses have attended on their patients—parents have attended on their children—children on their parents—friends on their friends, partaking often of the same bed, and washing and dressing them—physicians have, without resorting to precautionary measures, examined, touched, and handled those intrusted to their care, dressed their blisters, and been soiled with blood or the matter of black vomit—and while, beyond the infected districts, all this has been unattended with the most remote appearance of the propagation of the disease, it has been found that *in* the very focus of the infection, those exposed to this direct contact with the sick have not been more liable to be attacked than other individuals not so exposed, but who, like them, breathed the infected atmosphere, and were under the influence of the same exciting, predisposing, and efficient causes. The remarks on this subject made long ago by Dr. Ffirth, will apply not only to the preceding epidemics, but to all that have followed. “Of several thousand nurses employed in taking care of the sick in this city during the prevalence of an epidemic, it seldom happens that any of them have the disease. Not a single instance has occurred of a grave digger falling a victim to it; none of those who cart the sick and the dead experience any bad effects therefrom; the washerwomen, and those that lay out corpses, follow their several occupations with the same indifference, and without any worse effects during the existence of the disease than at any other time” (p. 50).

In this city, as elsewhere, autopsic researches have been prosecuted with zeal, and extensively, at each return of the disease; and in no instance, that I can discover, has any injurious effects resulted to the operator or his assistants. Neither has it been discovered that inoculation, whether accidental or intentional, has propagated the disease. These facts, which have often been verified in our different epidemics, were noted in a striking manner in that of 1793. “In Philadelphia,” says Dr. Deveze, “I made frequent autopsic examinations. In this operation, I twice wounded myself with impunity. The first time, I cut the middle finger of the left hand with the scalpel in attempting to detach the gall-bladder, which was of an extraordinary weight. The student who was writing my observations, pointed out to me that blood flowed from the part, which was already covered with the blood from the dead body. I wrapped it up with linen, and continued the dissection. When this was over, I washed my hand, and discovered on the first phalanx a wound half an inch long. I covered it with court-plaster, and it healed up easily without accident. The second wound was caused by one of the ribs which I broke in order to examine the extent and origin of a sanguineous abscess situated under the pleura, on the side of the vertebral column. One of the ends of the rib tore the back of my hand. The wound was two inches long, of unequal depth, and from several parts of it there issued a considerable quantity of blood. After washing it, I dressed it with court-plaster. It inflamed, and occasioned some degree of redness; but, in the course of a few days, healed up” (pp. 239, 240).

Nor were the physicians who investigated the nature and mode of propagation of the yellow fever, slow in devising and resorting to experiments with a view of testing the contagious character of the disease.

Dr. Cathrall has left us, in his memoir on the analysis of the black vomit, published in 1800, an account of experiments he made, so early as 1794, to ascertain the effect of that liquid on the living system. He applied it to his tongue and skin—fed cats, dogs, and fowls with food mixed with the matter—exposed himself and other individuals to the fumes obtained by the evaporation of it—but all with perfect impunity (pp. 19–23). Dr. Cathrall's example was soon followed by Dr. Ffirth, who, in a dissertation on malignant fever, published in 1804, relates a number of experiments he made upon the fluid ejected from the stomachs of individuals labouring under the disease, as also with the serum of the blood, the saliva, and the matter of perspiration. Like Dr. C., he fed dogs and cats with the first mentioned fluid during days and weeks—inoculated dogs with the fresh matter—subjected himself to the same operation—applied the fluid to the surface of a cut made on his arm, and secured it there for ten days by means of sticking plaster—repeated the experiment above twenty times on various parts of his body—inserted the matter into his eyes—exposed himself to the evaporating fumes from the same substance—swallowed pills made of the inspissated substance obtained from that evaporation—swallowed a mixture composed of half an ounce of the fluid and an ounce and a half of water—increased the dose till it reached to two ounces—and finally swallowed the black vomit in like quantity, without any dilution at all. But neither from these various experiments, nor from inoculation with the other substances mentioned, did any injurious consequences or serious inconveniences arise.¹

Let it not be supposed that I would attach much importance to the experiments on the black vomit and other fluids referred to by Cathrall, Ffirth, and other writers in this country and elsewhere. So far as the settlement of the question of the contagiousness of the yellow fever is concerned, they can at best afford but little, if any aid; for the ingestion of these fluids, their inoculation in parts endowed with the greatest power of absorption, &c., may safely be admitted to prove their perfect innocuousness without refusing to recognize the contagious character of this disease, which may be effected through other channels and agencies not yet discovered. Such experiments only show the harmlessness of these fluids, whether used in one way or another, and nothing more. They prove no more than would the ingestion of, or the inoculation with, the serum of the blood, the gastric secretion, the saliva, the perspirable fluid of patients labouring under the whooping-cough, scarlatina, typhus fever, oriental plague, smallpox, and other affections really or reputed contagious.

The contagionist is entitled to the use of these objections. He may make the best of them he can, and place them side by side with the

¹ A Treatise on Malignant Fever, with an attempt to prove its non-contagious nature, pp. 53–57.

fact long known to every medical inquirer, that animal and vegetable substances of the most poisonous character, even the virus of hydrophobia, smallpox, &c., may be swallowed with impunity. But, although willing to refrain from appealing to such experiments for the settlement of the question at issue, we cannot but view the other facts referred to above—the innocuity of direct and close contact with patients labouring under the fever, or with their remains after death, as entitled to our regard, inasmuch as were the disease really endowed with a contagious property, this property would necessarily manifest itself under the circumstances mentioned; while the total absence of such a manifestation in localities free from the general sources of infection, and the no greater risk of attack on the part of those exposed to such contact in infected districts, is a strong evidence in favour of the non-contagiousness of the fever. Apply this test to any disease indubitably endowed with the character contended for, and see what will be the result.¹

As already seen, it has been advanced that the innocuity of such close contact and of dissections should not be adduced as proof of non-contagion, on the ground that a like immunity often attends a similar exposure to diseases whose contagiousness is not disputed—much of this depending on the absence of a state of predisposition on the part of those so exposed, or on a variety of contingencies which annul the virulence and diffusiveness of the contagious agent. Nor is this all. Contagionists, also, contend that the appeal to the immunity from the disease in the instances in question may be met by a counter appeal to facts of an opposite kind—cases having occurred in which the fever resulted from close approximation to the sick, from the dissection of the dead, and from the inoculation of the fluid during such operation. And finally, it is stated that in this and other instances of kindred nature, the practice of concluding from the exemption of a few, or even of many individuals who have been in contact with persons labouring under fever, that the latter is not contagious, has long been branded as unphilosophical and mischievous by respectable authorities.²

In answer to this it will be sufficient to remark, that exemptions from attack in individuals exposed to contagious diseases constitute exceptions to a general rule, and cannot, therefore, be placed on a parallel with the instances of exemption noticed in yellow fever, which, judging from all the data before us, are universally encountered, and form the rule; and that a reference to proper authorities will prove beyond a doubt, that the greater number of cases of the disease cited as occurring in consequence of, or after exposure to, direct contact and dissection, were observed not beyond the sickly district, but in the very focus of the infection, which, exercising its influence on those who kept aloof from the sick or their remains, may very reasonably be presumed to have produced the same effect on those who followed a different course; while the few instances in which such attacks are

¹ Kéraudren, pp. 25–27; Bally, pp. 405, &c.

² Lind on Infection, p. 239.

said to have occurred in healthy localities, and far from the reach of the epidemic influence, have not come to us clothed with that degree of authenticity that would induce us to regard them as calculated to reverse the inference to which the others naturally lead.

The argument in favour of contagion founded on occurrences of the kind—approach and care of patients labouring under the yellow fever—may convince Dr. Copland, and others of the same school; but, unaided by something of a more satisfactory kind, it cannot be admitted as going very far towards the settlement of the question in the minds of unbiassed inquirers. Dr. Copland says that, in his simplicity, he believes that when one subject becomes infected with smallpox, or measles, or scarlet fever, a short time after having been near to one or more persons already affected by either of those maladies, the infection has been communicated by those persons, whether the disease in question be epidemic at the time or not; and that, when persons who have recently experienced either of those maladies, or when the clothes of one who has recently died of it have been conveyed into places where the disease did not then exist, but where it soon afterwards broke out and prevailed, the infection of that particular malady was actually introduced.

On this point, Dr. C. will doubtless not be contradicted. But when from this he starts to remark, that observing those occurrences so frequently as to become familiar in respect to those distempers, and knowing that occurrences identical with these in every particular have taken place in regard to the yellow fever, we cannot be so blinded by a false doctrine, or by prejudice, as not to infer that the latter belongs to the same category as the former—that the one is contagious as well as the other; when, I say, he arrives at this conclusion, it may be doubted whether he will find many followers. The diseases he enumerates as objects of comparison are known to be contagious—they are so nearly to the same degree everywhere; whereas the contagiousness of the yellow fever is not proved, and constitutes the very question at issue. The attack of the latter disease only takes place in particular localities where the sufferers would have sickened nearly as readily without exposing themselves to the contact of, or approaching the sick. The circumstance of an attack following to such exposure proves nothing—so far, at least, as concerns the contagiousness of the yellow fever, whatever it may do in relation to the other disease. Hence, when it takes place after such an exposure, we have no right to conclude that it was due to the latter, especially as we find that attacks in those who do not approach the sick are, to say the least, of very frequent occurrence; while in thousands of well authenticated cases, exposure, however direct, has proved perfectly harmless.

We may go further, and say that, even if it were true that the fever occurred more frequently among physicians and nurses who attend on yellow fever cases, as well as among the friends of the sick, than among individuals who, though living in infected localities, guard against exposure, the fact would not lend much assistance to the advocates of contagion. Confinement in a sick room, amid the effluvia issuing from the excretion of a diseased subject—the fatigue of watching and nursing—the anxiety of mind of surrounding friends,

and numberless other contingencies inseparably connected with attendance on and care of the sick, may well be viewed as capable of predisposing to the disease, or calling into action the efficient cause lurking around or floating in the system of individuals so circumstanced, without having recourse, in accounting for the occurrence, to a contagious poison, exhaling from the sick. Were the disease any other than the yellow fever, attendance on the patient would, were he placed in an infected locality, entail the same danger on nurses and friends. They would be more liable to sickness than if they abstained from exposure; and their sickness would in all probability be yellow fever; while physicians, during the existence of an epidemic are, from the very nature of their professional duties, more exposed than others to the operation of the efficient cause of the fever, to say nothing of the state of predisposition occasioned often by exhaustion, want of rest, exposure to night air, &c. In a word, we must conclude this section by remarking, with the late Dr. Drake, that in the prosecution of our inquiries relative to the contagion of yellow fever, the rules of logic require the exclusion of all cases in which the disease is said to have been contracted by going on board of a ship or boat where there were yellow fever patients, or by visiting a patient in a town affected with the fever when it is epidemic, for the individual, in these cases, is exposed to the same atmosphere which may have produced the fever in the sick; and it cannot be told whether he contracted the fever from them, or from entering the same localities which had occasioned it in them (ii. 287).

It may not be improper to add, that the question as to whether the impure air of a sickly locality may be carried about in the way last mentioned, and transmit the disease to remote places, is one of considerable importance, and upon which much has been said in recent times. Instances illustrating, as it is thought, such a mode of transmission, have been adduced in this country and abroad—never, however, more frequently than during the last few years. The wide dissemination of the disease in the southwestern portions of this country has been mainly ascribed to the agency of such means of transport; and the truth of some, at least, of the reports published, or otherwise circulated in reference to the matter, seems admitted by respectable authorities. In whatever way we may view the statements that have been made on the subject—whether they are regarded as of a nature to establish the reality of the occurrence, or whether they are found to fail in that respect, the point at issue does not necessarily involve the question of contagion, and will, therefore, be more appropriately examined under another head.

CHAPTER XXVII.

PROOFS OF NON-CONTAGION—CONTINUED.

Effects of Seclusion.—In contagious diseases, a careful seclusion from the sick, even in those districts of a place where the disease prevails most extensively, will always prove a more or less complete security against an attack, care being taken to avoid contact with objects likely to transmit the morbid virus. In this way it is that whole families have lived in safety and enjoyed uninterrupted health, though surrounded on all sides by pestilence. In diseases depending on local sources of infection, on the other hand, such precautionary measures generally prove illusory, for the atmosphere being charged with poisonous exhalations, and circulating freely, as well in the places of seclusion as in every other, these exhalations are enabled to produce the same injurious effects on individuals who thus endeavour to shut out the disease as on those who neglect such precautions. This difference of results is too well known to require elucidation in this place. The effect is admitted by all parties. It constitutes the basis of one of the arguments adduced by one party in support of the contagious character of the plague and other kindred diseases supposed to possess that character; and, on the other hand, is equally appealed to by the advocates of their local origin and non-transmissibility.

When, bearing in mind the security afforded by seclusion in disease really contagious, and the contrary effects in affections destitute of such properties, we inquire into the results obtained in the yellow fever, we discover ample reason for refusing our assent to the doctrine of contagion, and adhering to that of local origin. Doubtless a number of facts going to show the advantages of seclusion have been adduced, not only in this and other cities of the United States, but also in tropical climates—though more particularly in Europe; and their supposed success has fortified physicians and the public at large in the belief of the transmissibility of the fever by personal contact. We know, too, that Dr. Rush, long after he had abandoned the cause of contagion, stated that “several families who shut up their front and back doors and windows, and avoided going out of their houses, except to procure provisions, escaped the disease.”¹ But when we come to investigate the subject with all the attention it so richly deserves—when we carefully analyze all the facts on record, weighing the testimony of those by whom they are adduced, testing the statements made through means of the counter statements of other authorities, and scrutinizing the circumstances under which those facts have occurred—we arrive at a conclusion greatly at variance with that above mentioned.

¹ Fever of 1793, Inq., iii. 83.

In the first place—admitting, for the sake of argument, that instances have presented themselves occasionally in which careful seclusion appears to have afforded security against an attack—such instances are easily opposed by a large number of others, in which whole families have escaped, during wide-spreading epidemics, though not using any precautionary means of the kind. Few, if any, visitations of the disease, here or elsewhere, can be pointed out, in which some houses—occasionally many of them—have not remained free throughout, though their inmates attended to their various out door avocations, perchance visited the sick, and received their friends within; and that, too, in the immediate vicinity of other dwellings in which the system of isolation was rigorously enforced. We have seen, also, that while in one story of a house the number of cases and the mortality may be very large, the disease spares all the individuals residing in other parts of the building. Now, if such instances of exemption without seclusion are of frequent occurrence during most periods of sickness—if whole families among the classes most prone to the infection, who do not seclude themselves, remain free from the disease—we cannot with propriety appeal to cases of exemption attendant on a different course as a strong evidence of the efficacy of that course, since it is impossible to decide whether or not those who thus escaped would not have been equally fortunate had they acted otherwise. This was particularly exemplified at Gibraltar in 1828, where, while it was shown that nothing like complete seclusion existed in the cases adduced by Pym, &c., it was found that the immunity which the families so secluded enjoyed was more satisfactorily accounted for on other grounds—perhaps the situation of their houses. “There were several families, neighbours of them,” says Dr. Smith, “who never put themselves in quarantine, and never caught the disease; showing that seclusion was not the cause of the immunity enjoyed by these families.”¹ Even were it true that, as a general rule, success results from the system in question—that those who seclude themselves escape more readily than those who mix freely with all around—it can scarcely escape the attention of the medical inquirer that the beneficial effects alluded to cannot be ascribed solely to the avoidance of the contagious poison, inasmuch as it can with equal propriety be explained on the principle that individuals who thus escape do so in consequence of their avoiding many of those influences which contribute to excite the disease or predispose to an attack—insolation, night air, and, in the case of prisons—which have often been cited as places of safety—by being subjected to a course of regimen utterly incompatible with any kind of intemperance. Add to this that, in most of these instances, the closing of doors and windows may act beneficially, as it does in reference to malarial fevers generally, by shutting out the poison floating in the atmosphere, and that individuals who remain within doors are less exposed to the injurious effects of that poison.

On the other hand, instances are not wanting to show that, in this city and elsewhere, the disease has penetrated among individuals who secluded

¹ Edinburgh Journal, xxxv. 34.

themselves in the strictest manner, with a view of avoiding direct or indirect communication with the supposed sources of contagion. By the late Dr. Monges, whose experience in our epidemics, from 1793 to 1820, was not inferior to that of any other physician, as well as by our older practitioners, I have often been told that they were frequently called to attend individuals affected with yellow fever in families that had carried the measure of seclusion to the utmost extent possible—who avoided all communication with the exterior, allowed no one from without to approach them, closed the front and back doors and windows, and whose dwellings, in consequence, had the appearance of being completely deserted. That in our later epidemics observations of this kind have not often been made, is true; but simply because the idea of security from seclusion has been totally discarded, and both physicians and the inhabitants generally place their reliance in flight.

In Europe, similar and very striking instances of the inefficacy of seclusion are recorded. We learn that during the severe epidemic of Barcelona, in 1821, “some families, who isolated themselves in their houses, employing the most exact precautions for avoiding external intercourse and communication, did not by any means preserve themselves from the malady.”¹ Dr. Maclean relates the case of the ex-regent of the *audiencia* (corresponding with chief-justice in England), Dr. Francisco Olea, who “secluded himself, with his family and servants, at the commencement of the sickness (of Barcelona), having previously laid in a large stock of provisions, and throughout observed the most rigid precautions. His journals and letters were received on the end of a stick, and were twice immersed in vinegar and dried before perusal. After being shut up for thirty-seven days, seven individuals of his family sickened. The disease, indeed, although possessing the distinctive character of the epidemic, was not of a malignant kind, and all the members of this respectable family happily recovered.” Still, it was nothing more nor less than the yellow fever. O’Halloran relates a similar instance. A gentleman at Barcelona observed the strictest seclusion, shut himself up in his house with one servant, received even the water for drink through a bamboo, and every article of necessity with equal circumspection. He ridiculed the temerity of those persons who exposed themselves to the impression of a disease which, according to his idea, could only be got by contact. The master and servant sickened at the same period. Both died.²

At Gibraltar, in 1813, “Mr. Kerling, Mr. Lindblad, and Mr. Morison, respectable merchants, residing in Irish Town, on the first alarm of the fever, placed themselves in strict quarantine; yet, nevertheless, they were all attacked with the disease. A Mr. Jacks and his wife, who had retired to a place called Inness’s Farm, situated more than two-thirds of the way upon the rock, and at a considerable distance from any other house, some time before the fever made its appearance, and, being supplied with every necessary on the first alarm, placed themselves in strict quarantine; yet they were both attacked

¹ Manifesto in Maclean, on Evils of Quarantine, p. 135.

² O’Halloran, Yellow Fever of the South and East Coasts of Spain, pp. 85, 86.

with the disease, and perished.”¹ Mr. Humphreys, in his account of the same fever, remarks that “those who shut themselves up, and who may be considered to have placed themselves in quarantine, perfectly insulated, were attacked as readily as those who mixed indiscriminately with the people.

The same writer relates the following facts, which came under his immediate notice. A lady, an officer’s wife, residing in the Moorish Castle, which is considerably above and out of the town, never left that place, and was so alarmed that she would not allow any individual to approach her. But all to no avail. After the fever had existed five weeks or more, she was attacked, and died. Her husband, who was constantly at her bedside during her illness, escaped the disease. Similar precautions had been taken by the ordnance storekeeper, R. Pringle, Esq., who had adopted the most rigid quarantine for more than three weeks; yet he did not escape.² Dr. T. Smith, in his essay on the fever of Gibraltar in 1828, says he could, were it necessary, quote fifty cases of the above kind, and adds: “The family of a Mr. Farquhar, for example, who lived below the Naval Hospital, put themselves in strict quarantine at the commencement of the epidemic, one of whom caught the disease and died.”³ Other instances of the same character will be found recorded in works of undoubted authority respecting the fever of Europe.⁴ The same precautionary means have been tried in tropical climates, and with what success we may form some idea from the language of Dr. Ferguson, who, in speaking of those who adopted them, says: “In the army of St. Domingo, it was notorious that they were ever the first to be taken ill, and the surest to die; and during the yellow fever epidemic of 1816, at Barbadoes, I have recorded remarkable instances of the same kind, both from my own observation and that of others.”⁵

In 1805, the disease penetrated the State prison in New York, without the possibility of attributing the occurrence to communication, direct or indirect, with the sick. The first case commenced at Bridewell, with symptoms of bilious colic, which not unfrequently precede the characteristic appearance of the fever, and he died on the fourth day of the attack with decided marks of malignancy, and, among the rest, with the appearance of black vomit. The second case was seized with fever on the 12th of August, the day *before* the admission of the former, and exhibited “during his illness, in the marked affection of his stomach, the discoloration of his skin and in several other appearances, indubitable evidences of labouring under yellow fever.” The patient was visited by several physicians, by most of whom it was pronounced to be yellow fever. “It deserves to be remarked,” say the reporters, “that the circumstances of the cases preclude all suspicion of their having infected one another, or of the disease having arisen from any foreign or contagious source.”⁶

¹ Burnett, p. 321.

² Ibid., p. 346.

³ Edinb. Journ., xxxv. 35.

⁴ See Chervin, *Fever of Spain*, pp. 61, 67, 92, 173-4, 179, 181; Bancroft, p. 433.

⁵ Notes and Recollections, p. 158.

⁶ Walker and Quackenbos, *Med. Reposit.*, x. 59, 60.

Dr. Porter, of U. S. Army, in a recent essay on yellow fever, relates the following fact, obtained from the Misses —, maiden ladies, as having occurred in their family, in 1838: "Resided in St. Mary's Street, near Cooper River, Charleston Neck; had the young daughter of a country friend, 13 or 14 years of age with them, who had not been from the house and yard (detached from other dwellings) for six weeks, and had seen no sick person; she was taken with yellow fever, had black vomit, and died."¹

Dr. F. M. Robertson, of Augusta (now of Charleston), in his report on the fever of Augusta in 1839, makes the following statement: "Several instances occurred of persons being attacked with the prevailing fever, who had neither seen, nor held any intercourse whatever with the sick. Your committee will here relate one remarkable instance: Norris, a criminal, under sentence of death, who was confined in jail, was taken with the prevailing fever on the 12th of September, and died on the first attack with black vomit and hemorrhage. He was immured in a cell; no person had communicated with him, previous to his attack, but the jailer and turnkey, and he was the first person in the prison who took the disease."²

The epidemic of 1853, in and about New Orleans, and along the coast of the Mississippi and its tributary streams, has afforded many and striking instances of the futility of seclusion as a guard against the inroads of the disease. Dr. Stone stated to the sanitary commission that many old creoles, in the parish of Feliciana, secluded themselves entirely and would not go out at all. They seemed to have the disease as fatally as others.³ Dr. Copes stated that many old creoles secluded themselves entirely from their sick neighbours, but did not escape notwithstanding.⁴ Dr. E. H. Barton, in his very able report on the sanitary condition of New Orleans that year, states in proof—not of contagion but of the existence of an epidemic cause—that the disease reached insulated places, as jails, penitentiaries, and lockups, heretofore exempt. Even insulation on a plantation did not always exempt the inmates (p. 263). Dr. Fenner was told by the owner of a plantation on which the disease broke out spontaneously, of a young lady in the neighbourhood, "who kept herself entirely secluded, but was attacked with yellow fever." No other case occurred in that family.⁵ The fever broke out on several plantations near Donaldsonville, where strict non-intercourse was observed, and avoided others where no precautions were taken.⁶

Except to individuals, irrevocably wedded to preconceived notions, the preceding facts must prove beyond every possibility of doubt, that the disease could not have been communicated to families thus secluded by contagion. There was neither actual contact in any form whatsoever, nor exposure to effluvia issuing from the sick; and yet we find them attacked as readily by the prevailing malady, as those who continued to mix with people at large. Much more natural is it to admit that these cases point out the existence of

¹ Amer. Journ., January, 1855, p. 90.

³ Report of the Sanit. Com., p. 10.

⁵ Report in Tr. of Med. Assoc., vii. 510.

² Report, &c., p. 10.

⁴ Ibid., p. 41.

⁶ Ibid., p. 511.

a local cause, acting indiscriminately on the whole population of the place, and which defies barriers of the kind under consideration.

In addition to what has been stated to disprove the immunity afforded by isolation, it is necessary to remark, that of the large number of cases recorded with a view to establish the reality of that immunity, it is impossible to point out a single one which will stand the test of analysis. They have either occurred beyond the infected district, when necessarily the security enjoyed by the secluded was enjoyed also in an equal degree by all others—or the statements are found to be incorrect—no complete seclusion having been enforced, or, if enforced, not having been attended with the results claimed. The report of the Royal Academy of Medicine of Paris on the Documents of Dr. Chervin (p. 66, &c.), as also the work of the latter on the fever of Spain (pp. 60, 167, &c.), exhibits numerous proofs of the erroneousness of the facts adduced by the French medical commission sent, in 1821, to Barcelona, while a similar fate has attended the statements of Audouard, Sir William Pym, and other contagionists to the same effect.

Thus, it is stated by the commissioners that the soldiers attacked with the yellow fever were placed in a building separate from the hospital, and were not allowed to communicate with other individuals, and that by this means the disease was prevented from spreading. Dr. Chervin proves on the authority of the chaplain, Don Jose Rieza, that notwithstanding the enforcement of the most stringent precautionary measures, the fever extended to the other parts of the establishment.¹ It is further stated in the report of the commissioners, that the citadel of Barcelona was completely secured by the same means. We find, however, from a document signed by the surgeon of the citadel, Francis Borrás, that the yellow fever attacked thirteen or fourteen of the confined criminals.² Contrary to the statement of the commissioners, based on information obtained from Dr. Ferran, Dr. Chervin shows in the most positive manner, that, notwithstanding the strictest isolation, the fever in 1800 penetrated the Carracas or arsenal of Cadiz, where, in a population of 1000, the deaths amounted to 93.³ The monks of Santo Domingo, at Xeres, in 1800, so far from being exempted from the fever by the adoption of a rigid system of seclusion, lost 36 out of 90 of their members.⁴

In speaking of the yellow fever epidemic which prevailed at Gibraltar in the year 1813, Sir William Pym states that “of 500 persons confined to the dockyard during all the time of the sickness, there was not an instance of one of them being attacked.”⁵ Again, he says: “The labourers belonging to the naval works have been kept in strict quarantine in the dockyard, very near the spot where the disease showed itself in 1810, and if there is a situation in Gibraltar favourable to the generation of marsh miasmata it is there; and in 1804 it shared the fate of the other parts of the garrison; yet those

¹ Rapport de l'Acad., p. 71; Chervin, *Fièvre Jaune d'Espagne*, p. 181.

² Ibid.

³ Chervin, *Fièvre Jaune d'Espagne*, pp. 60, 61; see also Berthe, p. 40; Fellowes, p. 47 (note)

⁴ *Fever of Spain*, p. 67.

⁵ Pym, on *Bulam Fever*, p. 55.

people this year have continued healthy as well as another party of inhabitants who established themselves in Camp Bay, and cut off all communication with the infected.”¹ On this passage, Dr. O’Halloran² makes the following commentary, which sets the matter at rest: “The perusal of the foregoing quotations in the work of Dr. Pym, struck me forcibly on my arrival at Gibraltar in the present year. I thought the immunity of the dockyard from fever in the year 1813, a singular circumstance, and one which strongly operated against the doctrine which I am inclined to embrace. I had not doubted the assertions of Dr. Pym and Mr. W. W. Fraser; for, from their rank in the service, the one being at the head of the health office in London, and the other at the head of the medical department in Gibraltar, it was reasonable to expect information of authenticity, for their opportunities of attaining it exceeded that of others. It happened, however, by accident, that a medical gentleman, who saw the epidemic of 1813, observed, in the course of conversation, that fever prevailed to some extent in the dockyard that year; and that, by an application to Mr. Buck, who was secluded with the others, and who is now the superintending officer in charge of the establishment, particular and authentic information might be obtained on the subject.

“I applied to Mr. Buck, and the information which I have obtained from him and his head clerk is the following; it may be depended upon as officially correct: ‘The number of persons secluded, according to Mr. Buck’s account, who examined the books in my presence, amounted to 170—Dr. Pym makes it 500.’”

Dr. O’Halloran then quotes from the records of the dockyard the names of the persons who were there attacked with yellow fever—together with the names of those who died of that disease amounting in all to 24, of whom 16 recovered and 8 died, or 1 in 21 of the persons secluded. The dockyard authorities concluded the certificate, which they gave to Dr. O’Halloran, in the following words: “The truth of the assertion as to deaths having taken place there (the dockyard) cannot be doubted, and the list of sick fall far short of the numbers affected; but the dread of being sent to the Lazaretto, and being separated from their friends and relations, caused the sufferers to conceal the disease in many instances.”

Dr. Gillkrest confirms by his own personal examination the correction thus given by Dr. O’Halloran. “During my residence at Gibraltar,” he says, “I had ample means, by referring to the official authorities at the dockyard, of confirming the assertion of Dr. O’Halloran, that several cases of the fever prevalent in 1813 occurred there, as well as some deaths; but to my utter astonishment I found Sir William Pym’s statement repeated in the second edition of his work (p. 34), printed in 1848, though he had been twice in Gibraltar, after the error had thus been publicly exposed.”³

¹ Pym, on *Bubonic Fever*, p. 139.

² *Remarks on the Yellow Fever of the South and East Coasts of Spain, &c.*, p. 170, &c.

³ *Second Report on Quarantine, &c.*, p. 24; see also p. 160; *Cyclop. of Pract. Med.*, ii. 292.

Conversely, we find that in situations where immunity was ascribed by the same commissioners to isolation and seclusion, the measures employed were but imperfectly enforced, and altogether inadequate to the attainment of the object for which they were intended. Such was the case with the convents of the Capuchins, the Angels, St. Theresa, the Hieronymites, and the Carmelites,¹ all of which are represented as having escaped in this manner; for the documents obtained by Dr. Chervin, from unimpeachable sources, prove beyond a doubt that they all communicated as freely as heretofore with the city. The Capuchins begged their food, as usual, throughout the healthy and infected districts; the Sisters of the Angels distributed holy water, a reported specific against the disease, to a great number of individuals, of all ages and both sexes; and those of St. Thomas were no less indiscriminate in their intercourse with infected houses.² Such was the case, also, with the Charity and Orphan Asylums,³ the Charity Depots,⁴ the prison,⁵ the jail of Malaga in 1803⁶—finally, such was the case with the examples adduced by Sir W. Pym as having occurred at Gibraltar in 1828; in all of which, as Dr. T. Smith has satisfactorily shown, communication was kept up more or less freely with the town.⁷

Sir William Pym, in enumerating several private families who escaped an attack at Gibraltar, in 1828, by cutting off all communication with the infected, places in this number the family of Judge Howell; but Judge Howell authenticated before the Board of Inquiry, of which he was a member, the evidence which he had previously given to the Anglo-French Commissioners, which was to the effect that he had not secluded himself; that he was in the constant habit of receiving people on business into his house—paymasters of regiments, for example, to swear to their pay-lists, and many others; and that Mr. Amiel, surgeon of the 12th regiment, had been in the habit of visiting his family, often directly after leaving the wards of the hospital.⁸

From all that precedes, we may, without any great fear of error, conclude that attacks of the disease which occur in persons so secluded are not due to communication of the healthy with those already infected, or to the latter carrying with them the seeds of a contagious poison. If such cases are attributed to an infraction of the system of seclusion, the contagionist must be asked to explain the non-transmissibility of the disease at the short distance of a few yards from the place where that infraction has given rise to such direful effects. On the other hand, they must be called upon to explain how it comes to pass that in houses and public institutions where the inmates appear to obtain a certain degree of—sometimes complete—immunity from a system of seclusion or isolation, the introduction therein of a case or cases of yellow fever is not necessarily and in fact seldom or never followed by an extension of the disease.

¹ Rap. de l'Acad., p. 68; Chervin, *Fièvre Jaune d'Espagne*, p. 167, &c.

² Rep., p. 66.

³ Ibid., pp. 66, 67.

⁴ Ibid., p. 67.

⁵ Ibid., p. 70.

⁶ *Fever of Spain*, p. 93.

⁷ *Edinb. Med. and Surg. Journ.*, xxxv. 33–36.

⁸ Smith on *Fever of 1828*, *Edinburgh Med. and Surg. Journ.*, xxxv. 35, 36. See also *Second Quarantine Report*, pp. 24, 25.

We have seen that Dr. Rush and others state that, during some epidemics, persons who secluded themselves within doors, and were careful to close their front and back doors and windows, very often escaped an attack. In several of these secluded families the disease found its way, but did not spread. The same observations have been made in some prisons. In the *New Orleans Directory* for 1852, Dr. Dowler gave a summary of his researches, based on documentary, traditional, and living testimony, showing that in both the old calaboose and new prison, yellow fever had never prevailed, even during epidemics, although no means had been adopted, as quarantine, fumigations, and seclusions, to prevent the introduction of the supposed contagion of yellow fever. The conclusion drawn upon that occasion is thus cautiously expressed: "There is, if we may reason from what is known, but one certain method of escaping yellow fever in New Orleans—*incarceration*! That may always fail hereafter, but so far it has not." "Failed it has, during the far-reaching epidemic of 1853," says Dr. Dowler in another work, "but the failure has been so limited that the general rule is not yet invalidated." A document, copied from the jail record, shows that the average number of prisoners in the parish prison from June 2d to October 4th, was 170, a large portion unacclimatized; among whom twenty-two were attacked with yellow fever and six died, two having had black vomit. Twelve of these prisoners were admitted after the 11th of May, anterior to which several cases of yellow fever had appeared in the city, as it has been asserted. Four of these prisoners were admitted in May, one in June, three in July, two in August, two in September, and all but three had been admitted during the year 1853. "Now if twenty be deducted for the acclimated, the residue, 150, if they had been at large, exposed to the sun, dissipation, &c., would probably have lost fifty of their number, belonging, as they did, to the reckless class."

We also learn that the records of the jail show there was but one death more in 1853 in the jail than in 1852, among the same average number of prisoners, and for the corresponding period of the year.¹ Here we have the proof that the benefit derived, on some occasions, from a system of seclusion or isolation, is not due to the exclusion from houses or establishments where that system is applied of individuals affected with the disease, but to other circumstances, having no relation to contagion, and which produce similar effects in malarial fevers, the non-contagious nature of which is fully recognized. Let a few examples suffice.

Dr. Dundas having noticed that fever attacks were very common among the inmates of the British Hospital, of which he had charge, at Bahia (Brazil), set about remedying the evil. In the first place, he made several alterations in the hospital, and had those windows nailed up which admitted directly the current of wind suspected to cause the mischief. At the same time, effectual measures were taken to prevent convalescents from being exposed without clothing. The results of these measures were immediate, and the evidence they offered was complete and apparently free from all fallacy. From that

¹ Dowler, *Fever of 1853*, pp. 27, 28.

time intermittent fever almost completely disappeared from among the convalescents in the Bahia Hospital.¹ Of course Dr. Dundas, who ignores malaria, attributes these results to a cause very different from the mere shutting out of a poisoned atmosphere; but his fact is more useful than his explanation.

Varro, in his *Treatise upon Agriculture*, relates that his namesake, Varro, a Roman general, who was in great danger of suffering, with a large fleet and army, from a malignant fever at Conyra, having discovered the course of the miasmata which produced it to be from the south, fastened up all the southern windows and doors of the houses in which his troops were quartered, and opened new ones to the north, by which means he preserved them from the fever, which prevailed in all the other houses of the town and neighbourhood.

Dr. H. McLean, in enumerating the general causes which produce or dispose to ill health in St. Domingo, mentions the land winds, which prevail more particularly in June and July. To guard against their effects, the French, who are guided in that matter not by theory but by experience, "shut their doors and windows, and go out as little as possible whilst they prevail."² Dr. Lind, who is good authority on subjects of the kind, says that when a ship at anchor is near marshy ground or swamps, especially during summer or in hot weather, and the wind blows directly from thence, the gun-ports, which would admit such a noxious land breeze, ought to be shut, especially at night; or, if the ship rides with her head to the wind, a thick sail ought to be put upon the foremast, along which the smoke from the fire-place might be made to play and ascend.³ Long after the days of Lind, another British naval medical officer of experience in all that has relation to subjects of this kind has dwelt on similar points. "On land, the opening of tents in encampments situated in the vicinity and to windward of unhealthy spots should be invariably to leeward; and, if possible, there should be a barrier of some kind, as a piece of rising ground, between them and the quarter whence the wind blows. If a vessel be on a windward coast, and the soil is such as to give out pestilential miasmata, she should avoid, in as far as she can, any close approximation to the shore; she ought, especially, to be protected forward or at the bow, for, as a general rule, when a vessel is at anchor, she rides "head to wind," or with her bow to the breeze; and, for the purpose of protection, "the ports should be closed during the night."⁴

But it is not necessary to dwell any longer on this subject, allied as it is to a fact well known to all physicians, that the course of malarial fevers—that of yellow fever included—is wafted by the wind, and that its extension or diffusion is arrested by trees, walls, hills, rows of buildings, canvas, and other such obstacles.⁵

¹ *Sketches of Brazil*, p. 233.

² *Mortality of the Troops*, pp. 23, 24.

³ *Diseases of Seamen*, p. 73.

⁴ *Wallace, Edinb. Med. and Surg. Journ.*, xlv. 282, 283.

⁵ *Lancisi, op. cit.*, p. 89, &c.; *Donus, De restituenda Salubritate Agri Romani*, 1667; *Rigault de l'Isle, in Johnson, Trop. Clim.*, pp. 307, 314, 316, 317; *Mitchell (J. K.)*, pp. 26, 100; *Macculloch*, pp. 116, 252-3; *Williams, Morbid Poisons*, ii. 448, 453; *Johnson*,

As connected with this subject, it may be proper to state here that quarantine, in its application to prevent the introduction of the yellow fever from foreign countries or distant towns, on the principle of its being a contagious disease, has not, so far as can be decided from a survey of all the facts before us, proved of any avail, or afforded any grounds to deduce from its effects a proof of the fever being propagated by virtue of a poison evolved from the bodies of those labouring under it. This conclusion is predicated on the fact that the various modifications of quarantine hitherto devised in this and other countries visited by the yellow fever have utterly failed to fulfil the object for which they were intended, as well as on the circumstance that while the disease baffles all efforts of the kind mentioned, to check its progress, it has often spared, and continues to spare, other places, where no measures of the kind are adopted, and where the communication with infected localities remains uninterrupted.

In no place has the failure of quarantine, in excluding the disease, been more notoriously exhibited than in the city of Philadelphia, where, since the memorable season of 1793, a rigid system has been in operation and vigilantly and faithfully executed, and where, nevertheless, the disease has prevailed at times to a vast extent. Similar have been the results in New York¹ and other cities of this country, and similar will be found to have been the case in Europe and in tropical regions. No one can be ignorant of the fact that in Spanish ports, as well as in Gibraltar, the regulations are stringent, as applied not only to individuals, sick or healthy, but also to ships and merchandise; and yet, in the teeth of all these regulations, detrimental to commerce and vexatious to individuals subjected to them, the disease has often appeared and prevailed extensively. In the West Indies, quarantine has been tried with no better success. Some years ago, the public authorities of Martinique established a strict quarantine, particularly directed against Guadeloupe; and yet, notwithstanding, the colony was, to use the expression of Dr. Ferguson, consumed with yellow fever.² The same occurrence was repeated in 1816 and 1838, when the city of St. Pierre,³ in that island, ordered that vessels from Demerara and Guadeloupe, where the fever prevailed, should proceed to Fort Royal and there undergo quarantine. Two months after, the disease broke out, not at Fort Royal, but at St. Pierre, where the pretended contagion could not exist. It did not break out at the former place until after it had prevailed four months in other parts of the island.⁴ The disease was not imported from Rio Janeiro to Montevideo in 1850, though

Change of Air, p. 143; Evans, p. 15; Caldwell on Malaria, p. 135; Tournon, *Etudes sur Rome*, i. 209; Watson's *Practice*, p. 453; Bonnet, *Fièv. Interm.*, p. 311; Drake, i. 727; Monfalcon, pp. 93, 94, 160; Annesley on Diseases of India; Wilson Philip on Fevers, i. 79; Hosack, *Practice*, p. 174; Dundas, *Sketches of Brazil*, p. 246; De Renzi, *Miasmi Paludosi*, p. 30; Wooten, *N. O. Journ.*, iv. 4; Bartlett, *Thompson's Annals*.

¹ Miller, Report on Fever of 1805, in *Works*, p. 120.

² *Med.-Chir. Trans.*, viii. 143.

³ Rouvier, p. 18 (note); Chervin, *Rep. on Rufz's Mem.*, p. 97; Dariste, p. 22.

⁴ Rufz, pp. 6, 27.

vessels which arrived there had the disease on board. This was, of course, attributed to the effect of the quarantine established in the latter place; but this quarantine was only nominal, and not prolonged beyond eight or ten days. Such being the case, and the passage from Rio to Montevideo being very short, the disease could not, had it exhibited a contagious property, have been prevented from showing itself in the latter place.¹

A system of quarantine was long ago established in some of the French West Indies;² but its success being more than problematical, it has either been abandoned, or generally unattended to. It proved ineffectual in exempting the town of New Amsterdam (Demerara) during the epidemic which broke out at Georgetown in 1838, although stringent regulations were adopted long before the fever made its appearance.³

In Charleston, the quarantine system, if not strictly enforced now, was so in former days. Yet it was found that when the disease did not appear, the exemption from it could not be due to such enforcement, for in such seasons—as the late Dr. Tucker Harris, of that city, observes in reference to the year 1793, 1798, 1803—when the disease prevailed, “it will be in vain to pretend that a more strict attention was paid than on other occasions to the quarantine order.”⁴

Quarantine was established at Natchez in 1819, as soon as it was heard that the fever prevailed at New Orleans. It was rigidly enforced. Notwithstanding this, the epidemic began on the 1st day of September with several cases at the upper end of Maine Street. No boat, with fever from New Orleans, had arrived. The Alabama, with cases on board, stopped at Vidalia, opposite Natchez, but no one crossed over.⁵ After the epidemic of 1823, the quarantine was abandoned; and for a considerable part of that time, as Dr. Monette himself, a stanch contagionist, informs us, cases were taken from New Orleans boats, through the town to the hospital;⁶ still no case occurred from 1829 to 1837, although the fever was epidemic in New Orleans four times.

Dr. Drake, writing in 1844, records the fact, and further calls attention to the following: “Since the year 1817, yellow fever has been epidemic nineteen times in New Orleans, six times in Natchez, and once in Washington. If Washington received it that time from Natchez, what prevented her receiving it the other five times? And again, if Natchez received it six times from New Orleans, what prevented it reaching her the other thirteen times? Those questions remain to be answered. Meanwhile they stand in the way of the theory of importation, and strengthen the conclusion to which I am carried by the history of each epidemic, that the recorded and recollected facts concerning it do not establish its importation. It seems, in fact, to have been

¹ Saurel, *Essai d'une Climatologie Médicale de Montevideo*, p. 115; Anglada, *Traité de la Contagion*, ii. 51.

² Moreau de St. Méry, *Loix et Constitutions des Colonies, &c.*, ii. 313; *Ibid.*, *Description de St. Domingue*, i. 534; Moreau de Jonnés, p. 63.

³ Blair, pp. 56, 57.

⁴ Barton's *Med. and Phys. Journ.*, ii. 26.

⁵ Drake, ii. 265; see Perlee.

⁶ Monette, *Essay*, p. 75; Drake, ii. 275.

of local origin, although the assigned local causes of most of the epidemics are far from being proved to have been such."¹

The legislature of Louisiana, in 1817, passed a quarantine law, which went immediately into operation. Yet, notwithstanding, in 1818, cases of the fever occurred in pretty large number, and forty-three cases were admitted into the Charity Hospital. In consequence of this, the legislature, in the following winter, repealed the quarantine.² But the fever having appeared epidemically in 1819 and 1820, the legislature re-enacted, in 1821, a code of quarantine regulations. The following season was exempt, but in 1822 the disease, setting at defiance all restrictive measures, prevailed extensively. Indeed, it has been found in that city, that the appearance and prevalence of the fever are not influenced by the enforcement of quarantine laws. It has prevailed there when those laws existed, and when they were rigidly enforced; it has prevailed even during the war, when, from the want of arrivals, no quarantine could be required, and it has failed to prevail at periods when no restriction was placed on the intercourse with infected places in the West Indies and elsewhere, either by war, embargo, or quarantine.

Quarantine was enforced in Mobile in 1854 as stringently as the purest contagionist could well desire; and yet the disease broke out and prevailed to a small extent. It may be remarked, that while quarantine did not shut out the disease from the port, the futility of the measures of exclusion in force was shown by the fact, that cases were received in other parts of the city without communicating the disease.³ Here, then, we find the disease appearing spontaneously in one part of the place in defiance of measures adopted to exclude it, while in other quarters, cases actually introduced do not contribute in any way to the spread of the infection. The same thing occurred in Philadelphia on several occasions. To cite one: In 1854, quarantine measures were strictly enforced—nevertheless, the fever made its appearance; and while the introduction of it by sea was prevented, cases reached us from Savannah, and other infected places, by land, and—as several times mentioned—failed to spread the evil.

That the absence of quarantine does not, notwithstanding the existence of intercourse with sickly ports, entail the prevalence of the fever, is satisfactorily proved by the preceding and other indisputable facts. The disease has never been conveyed to Great Britain, or if so conveyed, has never been propagated, although passengers or vessels from the West Indies are there exempted from quarantine restrictions unless actually labouring under it, or unless the vessel is itself a focus of infection—which is seldom the case. In France, similar results were obtained before the adoption of such sanitary measures; and so completely has the inutility of these been demonstrated, that they have been abrogated. In this city, where, since 1793, we have been, notwithstanding the strict observance of quarantine regulations, fre-

¹ *Op. cit.*, p. 277.

² Dowler, pp. 24-5; Fenner, vii. 541; Drake, ii. 203.

³ Ketchum, *Trans. of State Med. Soc. of Alabama* for 1855, p. 111.

quently visited by the disease, the latter was not introduced from 1762 to the above-mentioned year, although during that long period of thirty years, the intercourse with West India ports—where the fever prevailed epidemically or sporadically—was as extensive as it has been since, and the restrictions imposed were scarcely more than nominal.

“The experience of quarantine in the United States,” says Dr. Miller, of New York—writing in 1805—“speaks little in its favour; for although, during the last ten years, it has been scrupulously in force in several ports, we have heard ten times more of imported contagion and of its ravages at these very ports during that short period, than for a hundred years before, when no quarantine was in existence.” During the revolutionary war, the intercourse between New York and the West Indies was necessarily considerable; nevertheless, though no quarantine regulations existed, and many cases were received from the fleet and otherwise, no mischief accrued; the yellow fever did not spread. The same thing occurred during the war of 1756, when the fever prevailed to an unprecedented extent in the West Indies. During this period, the intercourse between these and the port of New York must have been extremely frequent; many cases of the fever must, as a consequence, have found their way there, and yet, notwithstanding the absence of quarantine regulations—at any rate of any worth mentioning—we hear nothing during this time of the importation of the disease.¹ In some of our seaports, where no quarantine laws are in operation against individuals or their personal effects, we do not find that the yellow fever is more commonly noticed than in this and other cities where they are rigidly enforced.

Turn we to Spain, we find that before the commencement of the present century, neither vessels, passengers, nor effects, from tropical climates, were submitted, in the ports of that country, to any quarantine regulations; the disease having been declared by royal ordinance (under date of 21st October, 1761) to be void of contagion. Nevertheless, until the fatal year of 1800, the yellow fever was scarcely known in any part of the Peninsula. In Cadiz, it had not prevailed—except, perhaps, sporadically—since 1764, when it broke out in 1800, and assumed the epidemic character. During that respite of thirty-six years, when no quarantine was in force, twenty times more vessels entered the Spanish ports than during an equal number of years since; and though the fever, at the same time, frequently committed the most awful ravages in many of the ports whence those vessels had sailed, no injurious effects resulted from their admission; while from 1800 to this day, notwithstanding the adoption of a different course, the disease has often appeared epidemically.²

We find that, while quarantines at Martinique were of no avail in warding off the disease, at Dominica, Tobago, St. Vincent, &c., where none were established, there was not, as far as can be ascertained, a single case. I have already referred (*ante*, p. 501) to Dr. Ferguson for the fact, that at Barbadoes the hospitals had of late been in a regular course of importation of yellow fever from the navy, but that not even inoculation has been able

¹ See Miller, pp. 115, 116.

² Chervin, *Fev. of Spain*, p. 36; *Rapport*, p. 38.

to produce the disease upon any member of the hospital corps, by whom the sick had been received with open arms.¹ In another part of the present work, I have also referred to several examples of the fever being actually introduced into places where no quarantines existed, or had been in force, and where, notwithstanding, no injurious effects resulted.²

During the lamentable devastation in 1847-48, in Barbadoes—a description of which has been given by Dr. John Davy—the intercourse between that colony and Demerara, which is immense, was not for a single day interrupted; yet the disease was not communicated—Demerara having been free from that scourge since 1845.³

A survey of all that precedes will, so far as I am able to decide, bear me out in the opinion, that the result of quarantine restrictions, as applied to individuals positively, and to their effects probably, can afford no conclusion in favour of the contagious character of the yellow fever; for if, while bearing in mind that the fever does not, under any circumstances, appear epidemically, but at intervals of greater or shorter duration, we find that it breaks out in spite of the restrictions in question; and that at other times, even under the most favourable circumstances, it spares places in which no such restrictions are imposed, we cannot, in strict logic, attribute to the enforcement of these, the exemption enjoyed by any place where precautionary measures of the kind under present consideration have been adopted with a view to guard against the introduction of the fever, since it is impossible to affirm, without risk of error, that the result would not have been the same had a different course been pursued.

On this subject, however, I shall have occasion to advert more appropriately in a forthcoming chapter, and need only remark, in conclusion, that while maintaining, that from the operation of quarantine laws—even admitting these to be more generally successful than we find them to be—no proof can be adduced of the contagiousness of the yellow fever, I am not willing to be understood as ignoring altogether the utility of restrictive measures. All I here affirm is that if quarantines are useful, they are useful in a way very different from that contended for by pure contagionists. It is not by preventing the introduction of individuals affected with the disease, or carrying the seeds of it in them from infected localities; but by putting an obstacle to the arrival of vessels in which the fever has been developed spontaneously, and of which the cargo, or timber, bilge water, and filth constitute the source of infection. I have in another place stated that by “them the disease, or its cause may be, and has been, introduced into healthy places, and communicated to those who have gone on board, or approached sufficiently near to be placed under the influence of the effluvia issuing from them. But such an introduction differs materially in its consequences from that of a contagious poison; for the disease resulting from it will cease when the imported source of infection has been removed; while, in the other case, it is perpe-

¹ Ferguson, viii. 143; Report in Bancroft's Seq., p. 188-9.

² Ibid.

³ Blair, Br. and For. Med.-Ch. Rev., vii. 557.

tuated by passing from one individual to another, and spreads long after the removal of the vessel to which it owes its origin.”¹

The introduction of the disease in a vessel from some other port furnishes, therefore, by itself, no proof of its contagiousness. Neither can we from any obstacles that may be placed successfully to such an introduction, build up an argument in support of that opinion; while the unsuccessfulness of all restrictive measures directed against individuals—the appearance of the disease, notwithstanding a strict observance of such measures—and the exemption of places where they are not adopted, or are not enforced, or where they are evaded, go far to establish the contrary doctrine.

Let it not be said that the appearance of the fever in localities where a quarantine system is in operation is no proof of the local origin and non-contagious nature of the disease, on the ground that the occurrence is the effect not of the inutility, but of the violation or imperfect enforcement of that system—that, however stringent the laws enacted to guard against the introduction of the contagion may be—whatever be the care with which they were executed, they are continually evaded, and that, in many instances, in which the fever has appeared, something of the kind has been discovered, and that if it were possible to arrive at the truth, it would be found that such is the result in all.

The argument is vitiated by the fact that in some cases the fever has broken out under circumstances which forbid us completely from attributing it to importation—when, from the non-arrival of vessels or sick, it could not have been due to the infraction of quarantine laws. It fails, also, from the circumstances that the fever, as already stated, does not appear in many places, where there are no laws of the kind, or where they are not enforced, and where vessels from infected ports, as well as individuals labouring under the disease, are allowed to enter; and, again, it is vitiated by the well known fact, that while the occurrence of the fever in a particular place is ascribed to the evasion or violation of the laws in question by individuals labouring under the disease, or carrying in or about them the seeds of the contagion—those same laws have been evaded or violated, on the largest possible scale—sometimes by hundreds of individuals—without giving rise to any evil effects. One of the early cases which occurred in this city in 1854, was that of a young Irish servant girl, residing in a sailors’ boarding house in the first street parallel with the Delaware, and in the very centre of the infected district of the year before. Some of our wise heads were disposed to ascribe her disease to a contagious germ communicated to her from some boarder who had arrived from an infected place—although the particular individual who had been guilty of the transgression, could not be pointed out. In that very season, I myself saw several individuals who had arrived from Savannah while labouring under the disease, or who sickened soon after their arrival; and who died or recovered in hotels, the hospital, or private houses, without communicating the fever to any one. If any person wishes to know how quarantine laws may be and have been evaded without evil consequences, let him examine into the doings

¹ Amer. Journ., April, 1852, p. 355.

at Barcelona in 1821. Dr. Costa Siere informs us, that while French troops guarded the frontiers against the introduction from that city into France, of individuals or merchandise, some three hundred smugglers daily crossed the Pyrenees and entered France by circuitous routes.¹ As to the cordon (established around Barcelona), says Dr. Maclean, "it was wholly a matter of traffic. At a village called Orta, through which it passed, there was a house at once *sucio y limpia*—foul and clean—the foul part being supposed to be within and the clean part to be without the cordon; but, of course, having free communication with one another. At this house resided the medical superintendent of the cordon, who did not neglect to reap a famous harvest. Passports for Barcelona for the day were currently given by the alcaldes of the villages without the cordon; and the bearers of them went freely to that city, where they spent the day, and returned in the evening to the outside, to which they might have conveyed pounds of contagion, if they could have procured it. In short, it was in derision called by the people the half *picetta* cordon, half a franc being the current price of liberty of infraction." Mr. C——y, an English gentleman assured Dr. Maclean that, for one dollar, he received a certificate of his having resided twenty days beyond the cordon, although he had been only *one* day from Barcelona. Nor was this system reckoned remarkable, or peculiar to Catalonia. It would have equally prevailed in any other part of Spain that might have been empested. "Thus, then, it appears clearly, that if pestilential contagion really existed, and non-intercourse was really a preventive, the provisions of the sanitary code would be rendered by their infractions wholly nugatory as to any beneficial purpose."²

CHAPTER XXVIII.

PROOFS OF NON-CONTAGION—CONTINUED.

Single Attacks in Yellow Fever, no Proof of the Contagious Nature of the Disease.—Contagionists have not unfrequently adduced as an argument in favour of their opinion, the peculiarity which, as has been seen in a former chapter, the yellow fever possesses like the smallpox, and other diseases of admitted contagious character, of exhausting the susceptibility of the constitution to the action of the poison. But before much weight can be attached to this argument it would be necessary to show, that such a property of exhausting the susceptibility is really the unfailing attribute of contagious diseases, and that it is never possessed by complaints of a different character.

¹ De la Non-Contagion de la Fièvre Jaune, p. 96.

² Evils of Quarantine Laws, p. 171.

Now it is well known, that many undeniably contagious diseases—as syphilis, cellulitis venenata, psora—do not secure the constitution from further attacks. Others, of which the contagiousness, though called in question by some, is religiously believed in, and maintained by a large number of physicians, are alike destitute of that property. Thus, the typhus of Great Britain and Ireland is viewed as a highly contagious disease, particularly in Edinburgh, Glasgow, Dublin, and other large towns, where it chiefly occurs. The same has been and is said of hospital, camp, and ship fevers everywhere; and yet these fevers, though viewed otherwise by Hildenbrandt,¹ Lombard,² and others, so far from being found to possess as a natural attribute the exhausting effect in question, have been thought to predispose to reinfection.³

In oriental plague itself, which relatively to the subject before us, has served as an object of comparison, on account of its supposed contagious character and power of affording immunity ever after, has been shown even by contagionists themselves, to present more frequent examples of reinfection than has been laid to the score of the yellow fever. “In plague,” says Russel, “contrary to what happens in the smallpox, persons are liable to be infected more than once, not only at long intervals, and in different climates, but in the same town, in the course of the same pestilential season. This fact, which has been doubted formerly by some physicians, and by others expressly contradicted, has long been established upon the best authorities.”⁴ Diemerbroeck cites two cases out of 120 in the same season; but saw several at Nienmegen who had had the disease before.⁵ Some cases of reinfection occurred at Geneva in 1657.⁶ Some at Moscow in 1770–71;⁷ some at Vicenza.⁸ Chicoyneau⁹ and Bertrand¹⁰ met cases of the kind at Marseilles in 1720. They are mentioned by Olivier,¹¹ D’Ohson,¹² Wellman,¹³ Howard,¹⁴ Samoilowitz,¹⁵ and others. Dr. Hennen was unable to ascertain, after the most careful inquiry, a single authentic instance of the plague having attacked the same individual twice at Malta. He, however, was informed by a native physician that many of the attendants on the sick and expurgators, who had passed through the disease, repeatedly had pestilential eruptions on the hands and arms, as well as painful swellings of the glands on the armpits, if they handled infected clothes without the customary precautions.¹⁶

¹ Du Typhus Contagieux, p. 144.

² Archives Générales, 2d series, xii. 88.

³ Barlow, Lond. Lancet, Feb. 1840; Amer. Journ., lii. 463.

⁴ A Treatise on the Plague, &c., p. 180.

⁵ De Peste, lib. iv.; Hist., pp. 37, 45.

⁶ P. Antero Mariadi S. Bonaventure Agostino, p. 299.

⁷ Mertens, Obs. Med., p. 123; Orraeus, Descriptio Pestil, &c., p. 60.

⁸ Massaria, Tract. de Peste, p. 509.

⁹ Traité de la Peste, p. 322.

¹⁰ Ibid., p. 381; Relat. Hist., p. 209.

¹¹ Travels in the Ottoman Empire, Egypt, and Persia, i. ch. x.

¹² Tableau Gén. de l’Empire; Othomer, ii. § vi.

¹³ Travels, p. 518.

¹⁴ On Lazaretto, p. 33.

¹⁵ Mém. sur la Peste, qui en 1771, Ravagea l’Empire de Russia, sur tout Moscou, &c., pp. 35, 39, 40.

¹⁶ Topography of the Medit., p. 508.

Dr. Brayer informs us that second attacks of plague, after intervals of one or several years, are very common. "The Armenian dragoman of Dr. B. had it twice. The female servant of a Frank lady of my acquaintance had it six times—an officer (*huissier*, or doorkeeper) of one of the Frank chancelleries (consulates) had it six times; a porter at Galeta had it seven times; finally, an apothecary of that faubourg frequently pointed out to me an individual—a great drunkard—whom everybody knew to have had it fourteen times. He died of it the next year."¹

Nay, by not a few among both old and recent inquirers, the plague is regarded as almost completely destitute of the power contended for. Clot Bey, who is of that opinion, states that, during the epidemic of the disease which prevailed in Egypt, in 1834–35, he and his colleagues saw many persons perish who had passed through it before.² It is also a matter of observation, that even in diseases admitted generally to possess in a high degree the power of exhausting susceptibility, cases of reinfection are, at times, so frequent as to almost justify the idea of its not forming one of their characteristics.

Already, in a former chapter, it was mentioned that cases are on record in which even smallpox (and, also, scarlet fever and other kindred diseases) has done, with respect to its repetition, as much as can be related of the yellow fever; and reference was made to one instance in which the disease was repeated eleven times in the same person. Admitting this to be an exceptional case, we find other facts fully establishing the frequent occurrence of reinfection. Dr. Cross calculates the proportion of second attacks at 1 in 300 cases; Bousquet, however, found it to amount, at Marseilles, to 1 in 100.³ In 1830–31, 8,067 cases of the disease occurred at Malta. Of this number, there were 91 instances of second attacks.⁴

With these facts in view; seeing that some diseases of admitted contagiousness are destitute of the power in question; that others which usually possess it present numerous examples of reinfection; and that others again, the contagiousness of which, though denied by some, is fully admitted by those who recognize in the yellow fever the possession of that property, are destitute wholly, or in great measure, of the exhausting power; seeing all this, I say, we cannot admit the propriety of concluding, from the fact that the yellow fever is endowed with that power, in common with some contagious complaints, that hence it must itself be contagious.

The conclusion must appear the more inadmissible when we find that contagionists themselves—as Dr. McWilliams,⁵ whose orthodoxy on that point cannot be called in question—seem inclined to doubt the preservative power of the yellow fever.

On the other hand, it may be remarked, that the property of exhausting the susceptibility of the constitution is not incompatible with the non-contagious-

¹ *Neuf Années à Constantinople*, ii. p. 304.

² *De la Peste*, p. 63.

³ *Williams, on Morbid Poisons*, i. 211.

⁴ *Dr. J. Davy, Notes on Blair*, p. 86.

⁵ *Fever of Boa Vista*, p. 112.

ness of the disease so possessing it; that if the effect is admitted to be produced by some contagious morbid poisons, there can be no reasons why it should be produced by other poisons, though these originate from causes of a different kind. But the erroneousness of the conclusion does not rest on such grounds alone; for we find that other diseases whose contagiousness is not much better proven than that of yellow fever, have, like it, the power of exhausting the liability of attack. Such is the case with the Dothineria, or typhoid fever, which, according to competent authorities,¹ attacks but once, and is not unfrequently regarded, notwithstanding the denial of Bretonneau (*Archiv.*, xxi. 57), Gendron (*Ib.*, xx. 185, 199, &c., *Bul. of Acad.*, ii. 369), Leuret (*Ib.*, xviii. 161, &c.), Misler (*Gaz. Méd.*, 1834, p. 422), Gauthier (p. 144), as non-contagious; peculiarities which are held up as marks of distinction between that disease and the true typhus of England, &c.

The Asiatic cholera is very generally, and in this country almost universally, admitted to be a non-contagious disease, and yet we hear of as few cases of reinfection in it as we do in the yellow fever.² Mr. Jameson informs us in reference to this matter, that to many medical officers who possessed large opportunities of observation, there did not occur a single instance of repetition in the same season, and that the centre division of the Bengal army hardly afforded half a dozen instances. In the left division and Rajapootana force, according to the unanimous declaration of the medical staff, no case of re-seizure occurred after the strength of the patient had been fully restored. In the Naypore force, two or three instances came to notice, but came under the strict denomination of relapses. With the Kurnaul division, not a single case offered. The same immunity from secondary visitation was observed in every quarter in which the epidemic prevailed; and we should, perhaps, be not far wide of the truth—Mr. Jameson adds—were we to affirm of the many myriads attacked, the returns of the whole country do not afford a score of well authenticated cases of the occurrence of the disease after the removal of the debility, and every other consequence of the primary attack.³

Several forms of malarial fever afford, if we may credit the statements of respectable authorities, protection against reinfection. In the West Indies, there are several forms of fever distinct from the true typhus icterodes, and which Dr. Copland has denominated bilio-inflammatory or ardent fever, and adynamic marsh fever, which act as seasoning fevers to Europeans who arrive in hot climates; the former appearing in robust plethoric persons who have emigrated to the West Indies, intertropical Africa, &c., the other in those less robust, or who have not been attacked by ardent fever.⁴ Dr. Stevens also recognizes the existence of two fevers independent of the true yellow fever, or, as he denominates it, African typhus, and which, like Chisholm, Pym, and some others, he regards as a native of Africa. One of the other two aforesaid

¹ Montault, *Mém. de l'Acad.*, vii. 370; Gauthier, *ib.*, vii. 100; Chomel, p. 333; Piorry, *Mém. Ac. Reg.*, vii. 145, 146; Barlow, *Lond. Lancet*, Feb. 29, 1840; *Am. Journ.*, lii. 463; Bartlett, p. 98.

² Williams on Morbid Poisons, i. 622.

³ Madras Reports, p. 23.

⁴ *Dict.*, ii. 1104.

fevers, he designates the climate fever. It corresponds to the bilio-inflammatory of Copland, and is a seasoning fever, and, as a general rule, is not taken twice, unless the individual so attacked has left the West Indies and returned again.¹ The Batavian and Edam fever, which, notwithstanding some points of similitude with the yellow fever of the West Indies, and the occasional occurrence in it of a few of the symptoms appertaining to the latter, cannot be considered as identical with it, is also evidently a seasoning fever.²

The pernicious fevers of Algeria, France, and other localities—the periodic and malarial nature of which cannot be disputed—have never, in the experience of Dr. Boudin, attacked the same individual a second time.³ After observing that, with one exception, the exanthemata, which occur only once, have a quartan type, Dr. Lacock⁴ says that “it is at least a curious coincidence, that a person who has had a quartan ague is not liable to a second attack.” Sydenham many years ago made the remark, and stated as a fact worthy of observation, that if any person be seized with a quartan who has had it, though long since, it terminates spontaneously after a few fits, of whatever age or constitution he be.⁵ After him, Van Swieten, as Dr. Lacock reminds us, expressly stated and insisted upon the same fact; and Dr. Wallis, in his note upon the above passage in Sydenham, confirms the statement, and remarks that “it is known from observation, in the marshes of Essex, the fens of Cambridgeshire, and other places where intermittents are endemic, that those who have laboured once for a series of time under this complaint, which will sometimes continue two or three years with short intervals, and escape the fatal consequences, will afterwards live totally free, or subject only to very slight attacks, which spontaneously disappear; though this is not always the case.”⁶ Sir Gilbert Blane states of the fever of Walcheren, that it is well ascertained that strangers, if they survive the first attack, become thereafter much less liable to the endemic intermittents.⁷

Mr. Boyle, in his volume on the topography and diseases of the African coast, says that the success or accuracy of the result of any attempt to generalize the average annual mortality among the European residents at Sierra Leone is rendered exceedingly doubtful, from the very great difference of risk between the newly arrived European who has not had fever and the one who has had it, however short or long a period either may have been in the colony; and also from the nature of the engagements of the different individuals, whether leading to great exposure soon after their arrival or otherwise. It is found, that if a man has really gone through genuine Sierra Leone fever, such disease is not likely ever again to attack him, except during his convalescence from the first attack. This assertion, Mr. Boyle continues, may appear to be made with too much confidence; but it is built upon the fact that during four years' local residence at Sierra Leone, with very exten-

¹ On the Blood, p. 194.

³ *Géographie Médicale*, p. 46.

⁵ Sydenham, Wallis's edit., i. 82.

⁷ *Dissertations*, i. 224.

² Johnson, *op. cit.*, p. 151.

⁴ *Lancet*, 1842, i. 162.

⁶ *Ibid.*, p. 82.

sive opportunities of judging, no one instance occurred which at all tended to shake or invalidate the position assumed. Mr. B. acknowledges, that unquestionably persons who have gone through the genuine form of that disease may be repeatedly attacked with *fever*; but these attacks will not bear any similarity in their course, or in the period of their duration, to the disease which has been termed genuine endemic, or original bilious remittent fever (pp. 150-1).

In a former chapter, attention was called to the fact, that in this and other countries, one attack of common bilious remittent or intermittent fever does not appear to destroy the susceptibility of the system to the further inroad of the disease. It is doubtful, however, that the remark is applicable to all the forms of these fevers, and in every place; and I have every reason to believe, that when individuals who suffer from a severe attack of the disease are restored to health with sound organs, they are, without being entirely secure, less prone to the disease than new comers. Sure is it, that in some parts of this country, the remark has been made by physicians competent to judge in matters of the kind. "I can testify from long and extensive experience," says Dr. S. Jackson, "that persons who have passed through a long and severe attack of remittent miasmatic fever, are generally exempt for many years, if not forever; and that one who has fairly worn out the intermittent by the strength of his constitution, seldom suffers from it in after times."¹ In respect to the malarial fever of Ireland, Dr. Jamieson states that second attacks are common; but adds, that it is not meant to deny that after recovering from the first fever of whatever kind, there is a diminished liability to severe invasions.²

We have here, therefore, the frequent occurrence of second attacks in diseases avowedly or reputed to be contagious—and, on the other hand, the destruction or diminution of the susceptibility of the system to further invasions in complaints as avowedly non-contagious. The existence of that power in yellow fever cannot, therefore, be viewed as militating in favour of the contagion of the disease. In concluding this subject, it may be proper to remind the reader of a remark of the late Dr. Drake, that, on the method of analogical reasoning respecting the contagiousness of yellow fever, founded on the fact of the similarity existing between it and eruptive diseases with respect to exhausting the susceptibility of the system to further attacks, the former ought to be eruptive as well as contagious; that if it fail in one, it may fail in the other also; and that relation between eruption and contagion is, in fact, much closer than between immunity from second attacks and contagion—repetition of attacks being common in some avowedly contagious complaints. The same eminent writer further adds, that there is, moreover, a difference between yellow fever and the true eruptive fever as to exemption from second attacks; for residence in a cold climate destroys it (sometimes?) in the yellow fever, but no change of climate has that effect in the eruptive fevers.³

¹ Summary of College of Physicians, p. 398.

² Dublin Journ. of Med. Sci., xvi. 358, N. S.

³ Drake, ii. 289.

Altitudinal and Geographical Ranges different in Yellow Fever and Contagious Diseases.—In a former chapter attention was called to this subject, and to the fact that while the yellow fever has not been known to prevail beyond certain geographical limits—the elaboration of the cause and the dissemination of the disease being prevented by the intense cold or short and cool summers of northern latitudes, and possibly by the intense heat of countries south of those which it has heretofore visited—and while it originates and prevails more generally in low, flat localities, and has never been ascertained to appear or extend at an elevation much exceeding 3,000 feet, and even but seldom that high, contagious eruptive and other diseases are not amenable to such a law, but prevail more or less readily wherever they are introduced. The smallpox, measles, scarlet fever, &c., may be traced north and south, east and west, far beyond the furthest limits assigned to the yellow fever—far beyond those to which malarial fevers generally, with which it is in some particulars allied, extend. Nor is it less true that the geographical range of zymotic contagious diseases, whether febrile or otherwise, far exceeds that of the yellow fever. On these points, therefore, a line of demarcation may be drawn between the yellow fever and those diseases, while the former approximates to some extent, at least, to fevers of a non-contagious character; for though the geographical and altitudinal ranges of malarial fevers extend far beyond those of the yellow fever, they stop short of those of diseases originating in, and propagated by, contagion.

Effects of Night Air.—The yellow fever is not only more readily taken and more severe in character in certain localities, but also at particular periods of the twenty-four hours. On no subject connected with the etiology of the disease are writers more unanimous than in attributing to night air, in an infected district, a more baneful influence than to that of day. Of the large number of such writers scarcely one is to be found unprepared to admit that exposure to such districts by night is almost sure to be followed by an attack. During our various epidemics, this baneful influence was manifested on such innumerable occasions as to leave no doubt as to the fact on the minds of our physicians, by some of whom it has been recorded in the most emphatic terms. It is so referred to by Drs. Rush,¹ Deveze (pp. 113, 114), J. K. Mitchell (pp. 53, 54), &c. “As for our yellow fever,” says Dr. Chapman, “it is well known that it is infinitely more hazardous to visit the infected portions of the city after night; and there is a common impression, probably correct, that the liability to take the disease is lessened by retreating early in the evening into houses, with closed doors and windows.”² In illustration of this, Dr. Emlen³ mentions the fact “of gentlemen sickening of this fever by remaining on the wharves at night, or visiting these very early in the morning, whilst those escaped with impunity who only remained there during the day.” Another of our writers remarks: “When the yellow fever prevailed in Philadelphia, individuals who spent the day in the city, in attention to business or for other

¹ Fever of 1793, p. 50; Works, iv. 18, 85.

² Med. and Phys. Journ., viii. 379.

³ N. A. Journ., v. 327.

purposes, but left it before sunset, and passed the night in the country, usually escaped the disease; but those who remained in the infected atmosphere both night and day, for the most part suffered."¹

The same observation has been made in other cities of this country. When the yellow fever broke out in Baltimore in 1819, it was found, according to Dr. Reese (p. 39), that "in almost every instance where any person visited the Point at night, the disease was contracted; while those who were only there in the day time escaped with impunity. Several physicians, who had been attending patients through the whole of the fever, in the very centre of infection, in the day time, were exempted from the malady; but by visiting once in the night they became diseased." The same facts are attested, of this and former epidemics, by other writers of that city.² Dr. Merrill, of Natchez, in a private communication to the present writer, says, in reference to the fever of that place: "I know of no instance in which the disease was ever taken from exposure in the day time, and then returning home to the country." The same difference will be found to hold in New York,³ Charleston,⁴ Mobile,⁵ Norfolk,⁶ Wilmington,⁷ and New Orleans.⁸

Sir John Carr, in his travels in Spain, mentions, on the authority of Mr. Laird, who had resided forty-eight years in Malaga, that the bakers, who daily brought bread to that city from the villages of Alhaurin, Churriana, &c., and returned in the evening, entirely escaped the epidemic; while those who passed the night there were attacked by it, and commonly died.⁹ This greater liability from exposure to night air was strikingly exemplified at Gibraltar, during the various epidemics of that place,¹⁰ as also in various parts of Spain;¹¹ while Humboldt, at Vera Cruz (p. 774), and a large number of writers on the fever as it prevails in hot and tropical climates, have called attention to the same fact.¹²

¹ Caldwell on Miasm, p. 507; Ibid. on Malaria, pp. 128, 144; Ibid. on Fever of 1805, p. 77.

² Drysdale, Med. Mus., i. 31.

³ Seaman (1795), p. 40.

⁴ Moultrie, p. 62; Dickson, Essays, p. 346; Ibid., Med. and Phys. Journ., iii. 257; Simon, p. 18.

⁵ Nott, N. O. Journ., iv. 580.

⁶ Valentin, pp. 140-1, 228.

⁷ Dr. Vaughan, p. 19; Ticknor, N. A. Journ., iii. 221.

⁸ Thomas, p. 78; Gros, p. 8; Chobert, p. 109.

⁹ Quoted by Bancroft, Seq., 322.

¹⁰ Wilson, p. 61; Amiel, xxxv. 277.

¹¹ Chervin, Fever of Spain, p. 171; Bally, p. 95; Rochoux, p. 119.

¹² Rollo, p. 144; Ibid., Observations, p. 18; Caillot, p. 260; Grant, p. 58; Monro, Dis. of the Army, p. 45; Dariste, p. 218; Lempriere, ii. 113; Bryson, p. 217; Boyle, pp. 170-1; Warren, p. 20; Lind on Hot Climates, pp. 30, 31, 147, 235, &c.; Ibid. on Seamen, pp. 57, 65, 75, 78; Chisholm, Manual, p. 12; Moseley, pp. 56, 57, 432; Trotter, i. 456, ii. 86; J. H. Dickson, Edinb. Journ., xiii. 51; Robertson, Meteor. and Phys. Obs., pp. 33, 98; Gillespie, pp. 7, 13, 38, 46; Wallace, Edinb. Journ., xlv. 273; Towne, p. 14; J. Wilson, p. 66; Hillary, p. 146; Dict. des Sci. Méd., xv. 361; Gilbert, p. 81; Hume, p. 245; Ralph, ii. 77; Bancroft, p. 232; Celle, pp. 292-4, 298; Ferguson, Med.-Chir. Tr., viii. 140; Ibid., Recollections, pp. 194-5, 200-1; McArthur, in Johnson, p. 350; Arnold, pp. 28, 60; Valentin, pp. 77 (note), 140-1; Dyott, in Musgrave, Med.-Chir. Rev., iv. 1004; Savarésy, p. 238; Hunter, p. 18; Henderson, p. 68; Bally, pp. 335, 368-9; Pugnet, pp. 335, 338,

Far be it from me to assert that danger is never incurred from visiting infected districts during the day. That such appears to have been the case in some of the instances mentioned, is doubtless true; and at every return of the disease, here or elsewhere, individuals are found who are attacked for visiting an infected city or locality at any time before sunset. But it does not follow that it always has been and always will be so. The object of what has been stated is to show that the risk of an attack is much greater from exposure at night than from a like exposure by day—not that communication with infected localities can be indulged in with perfect impunity. Dr. Nott states that, at Mobile, persons often take the yellow fever by coming into the town during the day for an hour or two; and instances of the kind have been occasionally noticed in this and other cities.

In regard to the greater danger from exposure to night air, the yellow fever approximates to diseases of miasmatic or local origin and of the non-contagiousness of which there is no doubt. Rigault De Lisle remarks that the evening dew is so much dreaded at Rome, from the proximity of the Pontine Marshes, that, as soon as it begins to be perceived, all the inhabitants shut themselves up in their houses. He further states that it is more dangerous to inhale bad air in the night than in the day; that the least critical moment is when the heat is greatest and the sun highest above the horizon—the most dangerous that which accompanies the setting and that which precedes the rising of the sun.¹ Other authorities have been as explicit on the subject of the Roman fever.²

Dr. James Johnson states that four out of five who suffer from the fever of Bengal are attacked or receive the deleterious principle in the evening (p. 61). The same observation has been made in other parts of India,³ at Batavia,⁴ at the Island of Edam,⁵ as well as on the coast of Africa,⁶ on the coast and in the islands of the Mediterranean,⁷ in France,⁸ &c. Finally, in this country, bilious remittent and intermittent fevers afford, at every return of the sickly season, examples of the injurious effects of night air. “All who know anything of fever,” says Dr. Simons, of Charleston (p. 18), “are aware that exposure to night air is particularly dangerous, and especially to sleep

339; Williams, pp. 454; Blane, pp. 92, 118, 230; Musgrave, *Med.-Chir. Trans.*, ix. 110, 116; Brown, *Cyclop. of Pract. Med.*, iii. 63; Chervin, *Exam.*, p. 17; Macculloch, pp. 127, 128.

¹ Rigdault de Lisle, in Johnson, pp. 311, 312; Chapman, viii. 384.

² Lancisi, p. 60; Folchi, *N. A. Journ.*, vii. 252; Clark's Notes, p. 81; Williams, p. 454; Chateaufvieux, *Edinb. Rev.*, xxviii. 49, 57; *Quart. Rev.*, xxx. 135; Bancroft, p. 232; Brocchi, *Stato Fisico del Suolo di Roma*, p. 261.

³ John Clark, *Long Voyages*, p. 124; Badenoch, *Med. Obs.*, iv. 156; Lind, pp. 148–9, 308; Fontana, p. 11; *Ib.*, French translation, p. 12.

⁴ Johnson, p. 128.

⁵ *Ibid.*, p. 134.

⁶ Boyd, p. 301; Bryson, *Edinb. Journ.*, lxix. 115; Chervin, *Exam. Crit.*, p. 171; Pritchard, pp. 180, 183.

⁷ Boyle on Dis. of Minorca, in Johnson, pp. 299–301; *Ibid.*, Fever of Sicily, *Edinb. Journ.*, viii. 186; Burnett, p. 17; Denmark, *Med.-Chir. Trans.*, vi. 300.

⁸ Monfalcon, pp. 73, 196; Macculloch, p. 127.

exposed to that air. Thus it is well known that we can visit the plantations and the adjacent country in the day time, without any danger; while if we sleep at night there, fever is the result." Most physicians of our southern States have noticed the same fact, which is also found to occur in other parts of the country.¹

Desultory Course of the Disease.—The histories of our epidemics, and, indeed, of all epidemics in this and other countries, show that no relation exists between the source of the disease and the diffusion of the latter to individuals. It may be stated also that all attempts to trace progressively the propagation of the fever to any number of persons, from the first case, or from any single point of infection have invariably failed. This is not characteristic of contagious diseases, in which the succession of cases, either at the outset, or during the course of an epidemic, can, in most instances, be easily traced to a known original source, while it is an unusual feature in all diseases arising from local causes and diffused independently of personal agencies. That the facility in question is experienced in the yellow fever has been affirmed very generally by contagionists, who usually lay much stress on their success in the matter, and who, at every visitation of the disease point with confidence to the supposed starting point—tracing the link connecting all succeeding cases to one or more preceding them, or to some individual who has communicated directly or indirectly with a focus of contagion. This every one knows. But all who have examined the subject know also, that, in most instances, the structure thus raised has been overthrown by subsequent and more accurate investigation; that the facts adduced to establish this affiliation have been shown to be based on popular statements only, or on testimony of doubtful authenticity, and that those that have not been so scrutinized, may justly, from the fate of the former, be viewed with suspicion.

The disease not unfrequently commences at several points and spreads in a desultory manner—attacking individuals having no communication with those first affected, and often situated at a considerable distance from them; and, though sometimes the epidemic progression of the disease is slow—cases succeeding each other at comparatively long intervals; in other instances, the spread is very rapid—too much so, indeed, to be ascribed to personal communication, direct or indirect. The degree of rapidity with which a febrile disease, possessing the contagious character, spreads, is and must be proportional to its power of infecting. No one can deny that the contagion of smallpox is much more active and indestructible than that of yellow fever (admitting the latter to *be* contagious). Yet, in its progress through a city, the march of the latter is tenfold more rapid than that of the former. In the space of a month, yellow fever has often pervaded an entire community, which, by the unassisted action of contagion, smallpox would not overrun in a year.²

¹ Nott, N. O. J., iv. 580; Smith on Epid., p. 74; Rush, Means of preventing Autumnal Diseases, iv. 124; Caldwell on Miasm, p. 508; Ibid. on Malaria, p. 144; Emlen, N. A. Med. and Surg. Journ., v. 328.

² Caldwell, Mem., 1826, p. 129.

If the disease commence at one point only—as often occurs—not many days elapse before several cases start up in the vicinity or at a distance. That, in some instances, the cases succeeding to the original ones have had communication with them, there is no reason to doubt. It can scarcely be otherwise. But, in many other instances—perhaps the greater number—this communication cannot be made out, and as the disease, when carried elsewhere, is not disseminated; and as such communications, when they take place, do so generally in infected localities where there are many reasons to think the whole atmosphere is contaminated and exercises a baneful influence on all—whether they visit the sick, or keep aloof—we may presume that the cases in question would have occurred even had there been no intercourse. Nor is it less true, that the effect is not to be ascribed to the contamination of the air by the sick, and the consequent diffusion of the contagious miasm. For admitting the possibility of such a contamination, it would take place at the height, not at the onset of an epidemic, when the number of cases is limited, and, as a matter of course, incapable of polluting the atmosphere of a whole city or district.

Be this as it may, this starting up of new cases having but a remote or no connection with each other on the heels of the original ones, has been noticed in this city and elsewhere, and may be shown to have occurred at Boa Vista on the events of the epidemic of which the contagionists lay so much stress. Well may Dr. Miller remark, that “if the first ten or twenty cases which occur in any season be strictly scrutinized, most of them are found, in their origin, to be distinct and independent of one another. Instead of pervading families, or creeping slowly from one neighbourhood to another in the track of infection, as is invariably the case with the contagious distempers, this disease is oftener found scattered at distant and unconnected points, and cases start up singly in situations where contagion could neither be traced nor suspected” (p. 98). In our epidemic of 1820, it was found that at Hodge’s Dock, most of the cases occurred simultaneously, and the greater number in the course of ten days. They were scattered over a space the two extreme points of which are about a long pistol-shot distance, and none of those who were sick had communication with any one labouring with the disease, prior to their attack.¹ The same circumstances were observable at Walnut Street Wharf. “Several individuals were often seized on the same day, without having had intercourse, or been near any one who was sick, and more than half the cases took place in ten days, twenty-six having been attacked from the 6th to the 16th of August. The same was observed in Duke Street and its vicinity.”²

In New Orleans, the fever of 1822 commenced at different places. One case was observed at the basin situated at one of the extreme points of the city. Another immediately after, or simultaneously occurred in a central locality; a third in Royal Street very far from the others; a fourth in the Faubourg Marigny, at another extremity, and a fifth on the opposite side of the river. It then progressed very irregularly in the Faubourg and city, and spread with great rapidity.

¹ Jackson, p. 86.

² Ibid.

Dr. Axson, in his excellent Report on the Epidemie of New Orleans in 1853, says: "All that we definitively know of the laws of contagion or infection, lead us to expect the gradual and progressive diffusion of a disease depending on such means within ascertained limits as to space and time. It forbids the idea of spontaneousness in several remote and distinct localities, and of exactness as to time. There must be precedence in a given place, progressive spread therefrom after a proper interval of time, and within a limited space. But such were not the phenomena we have just recited. There was neither progression nor succession, but spontaneousness of outbreak in distant places, and exact coincidence in the moment of their occurrence."¹

Dr. Nott, of Mobile, informed me, says Dr. Drake, "that on the 20th of September (1839), four cases of the fever occurred on the same day. One at the corner of Royal and St. Francis Streets, two in St. Francis near Claibourne Street, and one in Ann Street near the forks of the road to Spring Hill. There were none in the ships, lying either at the wharf or in the harbour near the mouth of the bay. No other cases occurred for three weeks, when it broke out all over the city, affecting great numbers."²

At Barcelona, in 1821, it was not found that the persons attacked in the suburb (Barcelonette) where the epidemic commenced, belonged all to the same house or the same street; nor that the disease spread progressively to those of the vicinity, as takes place in contagious complaints. On the contrary, the individuals attacked were distributed in different parts of the Faubourg, so that it was rare to find a house in which there was more than two patients of the kind.³ Dr. Humphreys, in his account of the epidemic of Gibraltar, in 1813, remarks, in like manner, that the disease did not creep gradually from one part of the town to another; but showed itself in different points at the same time, attacking persons who had never approached the sick.⁴ Again, we have the testimony of Mr. Amiel for the fact that, in 1828, the progress of the disease in the same place was not marked from a known focus of contagion to one or more individuals. As in 1813, instead of creeping from one family to another, cases frequently appeared unconnected and scattered at different points. In some instances, it spread with the rapidity of the electric fluid, and attacked persons who never had approached the sick, nor any assignable source of contagion.⁵

Dr. Hector McLean long ago remarked, in reference to the yellow fever of St. Domingo, that many individuals are seized at the same time, assigning as a reason, that great numbers are exposed to the perpetual action of a powerful agent (p. 81); and others have called attention to the rapidity of propagation of the disease in the West Indies, and to its desultory course. "It is often observed," says Dr. Wilson (John), "that the disease extends in an

¹ Report of the Sanitary Commissioners, &c., p. 492.

² Drake, ii. 220; Lewis, N. O. Journ., ii. 287.

³ Report of the Supreme Junta; Chervin, Fev. of Spain, p. 101.

⁴ Burnett, p. 346.

⁵ Amiel, Ed. Journ., xxxv. 264.

unaccountable and apparently capricious manner; and that becoming manifest at one or more points, it leaps, as it were, thence to other points of population, without any traces being perceived by which it proceeded" (p. 172). Dr. Ralph attempted, in 1816-17, at Barbadoes, to trace the spreading of the fever from the individual first seized, but failed, the progressive propagation through communication with the sick not being detected. There, as often elsewhere, the cases were diffused, and generally occurred in several and remote situations at the same time. "Whole families were in many instances swept off; but it rarely happened that the members fell sick in succession. On the contrary, it was a common occurrence for the whole of the inhabitants of a particular hut to be brought to the hospital on the same day, all of whom would be labouring under fever."¹

At Antigua, during the epidemic of 1816, the cases, after the disease extended beyond the Point, did not go from house to house, or from street to street, but generally the sufferers had had no sort of intercourse with their predecessors; and Dr. Musgrave, from whom we derive the statement, often ascertained that they had never been within a considerable distance of a house containing a patient.²

But while the diffusion of the disease is frequently too rapid to be accounted for on the principle of a communication, direct or indirect, of the sick with the well, in other instances the progress of the epidemic is so gradual and limited, notwithstanding the freest intercourse, as to be equally incompatible with the idea of contagion. In this city, in 1853, intercourse with the infected district, or with the sick, was never interrupted; and yet, in the long space of three months, the number of deaths did not exceed 170.

The desultory progress and capricious course of the disease are illustrated in the fact, frequently observed, that the cases during an epidemic, are dispersed in many houses and streets at more or less distance from one another, and that each of these contain but few sick. Indeed, the proportion of single cases in the midst of families is usually large; the instances of any large proportion of families being attacked are comparatively rare. Dr. Miller found in New York, in 1805, that "there were thirty-one streets of the city, most of which continued to be filled with inhabitants through the whole season, in which only a single case in each occurred; and in the mass of six hundred cases reported to the Board of Health, there were only thirty-five houses in which more than a single case was found." It was found, also, that there were forty streets, and those generally crowded, in which only one death in each took place. (*Miller*, p. 99.)

That instances of a contrary kind have occurred in which the disease is in a great measure circumscribed within narrow limits, attacks whole families, and prevails very generally in a few streets, is true. Facts of the wide spread of cases in particular families have been stated on the authority of Ralph. In New York, in 1822, out of 422 cases (the whole number), 295—near two-thirds—

¹ Ralph, *Edinb. Med.-Chir. Trans.*, ii. 56.

² *Med.-Chir. Trans.*, ix. 115; see also Blair, p. 153.

occurred in 86 houses, giving a proportion of 3.46 cases in each house, all of which were either opposite to or adjoining each other, or contained from 2 to 8 cases each; 276 of the whole number occurred in 67 houses. But it would be wrong to deduce from such facts, as has been done by Dr. Townsend in the instances before us, a proof of contagion; for it will be found that they occur only in very malignant and extensive epidemics, when the disease spreads extensively; and even then only in the infected districts, where all are exposed to the action of a general cause, and where the latter exists in its fullest force. In other localities, during even these severe epidemics, matters take a different turn, and the desultory, leaping, and capricious course of the disease is observed. And in New York, in the year mentioned, 127 cases out of the 422 reported, occurred in nearly separate houses. Were the disease propagated by contagion, there would be no reason for this difference. It would spread among families as readily in one district as in another—the hygienic conditions of all being similar. If in regard to really contagious diseases a contrary result sometimes obtains, the cause must be sought in the fact that it meets, in some places, with subjects protected by former attacks. In our latitudes, the number of those protected against the yellow fever is small, and the difference in question must be explained otherwise.

It is to be borne in mind, too, that in its mode of spreading through a family, the yellow fever pursues an equally desultory course. It does not, in its progress, as Dr. Caldwell¹ well remarks, pass gradually from one individual to another, but attacks several at the same time, or in a succession by far too rapid to be effected by secreted contagion. In many instances, the individuals attacked are not those who have most approached the sick. It not unfrequently happens that a case occurs in a family in the early part of the season, and that the subsequent cases manifest themselves weeks or even months after.

What has been said in the preceding pages in reference to the simultaneous origin of the yellow fever in different spots of a city, and of the rapidity with which it there spreads, without the possibility of referring the cases to communication with the sick, applies equally well to the origin and rapid diffusion of the disease in different places more or less remote from each other during an epidemic season. To those who have become informed of the occurrences in the southern and southwestern portions of this country during the summer and autumn of 1853, the following remarks of Dr. Axson will appear well founded. “If we look at the history of the epidemic in its totality—at the wide spread surface of country over which it was diffused, and at the time it required to effectuate this, the mind cannot fail to discover a conformity in each of these particulars to the well known habits of epidemic diseases. General causes, over which the art of man is powerless, seem mainly concerned in its diffusion. Nothing that we possibly know of the most virulent contagions will favour the opinion that they can in so short a

¹ Fever of 1805, p. 130.

space of time—commence, progress, culminate, and decline over an extent of country, and among a population as diverse and separate as that which fell subject to the dominion of our late pestilence by virtue of any property of contagion merely.”¹

The several epidemics of Spain have exhibited a spontaneousness of origin, a rapidity of diffusion in some instances, and a slowness of progression in others, equally incompatible with the idea of contagion. Take, for example, that of 1804. From a table given by Arejula, we learn that the fever broke out in Malaga on the 29th of June. At Velez, and at Antiguera—the former about ten, the latter about eighteen miles distant from that city—it appeared on the 2d of August; at Alicant on the 9th; at Montilla on the 11th; and at Arcos on the 14th of August.

On these facts, Dr. Brown remarks: “A reference to a map of Spain will show, that if Velez and Antiguera are within a few hours’ walk of Malaga, Montilla and Arcos are remote, and not even on the main line of communication. Assuming Montilla to be a new point of departure, it travelled north twenty miles to Cordova, between the 11th of August and the 28th September; for Arejula ridicules the idea of its being introduced by ‘un lino’ from Malaga, and says the communication was as free as between Madrid and Carabanchel—or, to use an illustration near home—between London and Hampton. Its progress, however, in this direction, had been comparatively rapid, for it took ten days longer to reach Ecija, only ten miles west of Montilla. On the 28th of August, the inhabitants of Cadiz were variously blaming Malaga, Gibraltar, or Havana, for sending them this fatal scourge, while it had been quietly established for a fortnight at Arcos, on the Guadalete, about eighteen miles from them, and half that distance from Xeres, which it nevertheless did not reach until the 23d of September. From Alicant it travelled twelve miles to Guadamar, at the mouth of the Segura, between the middle of August and the 21st of October; and finally it managed to get over the ten miles of ground from Paterna to Ximene, between the 30th of August and 27th of October.”²

Here, then, we have a fever which, though sometimes starting up in one spot, and remaining there for some time before diffusing itself at a distance, more frequently originates simultaneously, or nearly so, in different spots; sometimes spreading with the rapidity of lightning without any apparent communication between the sick and the well, and at other times progressing with remarkable slowness notwithstanding the freest communication. I know of no contagious disease chargeable with such a capricious course. On all these points, the yellow fever approximates to autumnal fevers, which originate from local sources of infection, and extend over a locality or city with more or less rapidity through means independent of personal contagion.

Proteiform Character of the Disease.—It has been remarked that although in some cases a general disease depending upon contagion may differ almost

¹ Report of Sanitary Commission, p. 494.

² Second Quarantine Report, London, 1852, p. 292.

infinitely as to degree, yet there is neither variety nor dissimilarity in the concurrence of symptoms, nor great uncertainty in the periods of their appearance or duration, rendering their nature doubtful, or giving the semblance of diseases of a nature totally different.¹ In the smallpox, for example, there may be a considerable difference between cases denoted by the fewest discernible spots and those indicated by numerous and confluent pustules. In some few overwhelming cases, death may occur without any eruption having taken place. The same variableness in regard to this phenomenon is observed in other complaints of kindred character. They may be at times of an inflammatory, at others of a malignant nature; but, except in a very few cases, the experienced physician finds no difficulty in detecting the nature of the diseases before him. The physiognomy of these remains the same; their period of incubation differs but slightly, if at all, in various cases, and the duration of each stage is marked with considerable precision.

Diseases of the febrile class, arising from local causes and spreading without the aid of contagion, as, indeed, many of the ordinary phlegmasiæ, present, as a general rule, under ordinary circumstances, considerable regularity in regard to some of the above points. Their duration is, to a certain extent, apparently fixed; each variety runs its course in a certain number of days, and its physiognomy and characteristic features are easily recognized. Nevertheless, in these diseases, considerable latitude in all these respects is observable. Though not usually cut short by remedial or other means, each variety of such diseases is susceptible of being divided into several species or classes, differing more or less from the others in point of duration, and each of which may appear sometimes in the garb of an *ephemera*, while at other times it requires many days to run its course. The period of incubation is very various in point of duration, the difference extending from a few hours to several months. They assume, under particular circumstances, some one of the several types—the intermittent, the remittent, and the continued—either retaining the one assumed to the close of the case, or passing from it to another, according to the various degrees of severity of the attack, or to other well-known circumstances; while they present in particular localities, and in the same localities at different seasons, and even during the same season, a different train of phenomena, depending on the affection of particular organs, or other causes, too well known to be dwelt upon in this place.²

Judged in reference to the above characteristics, the yellow fever approximates in many points to the latter class of diseases, and recedes from those of a contagious nature. Though usually presenting, not only during the same season and in the same place, but also in different localities and at different times, a train of phenomena imparting to the disease a considerable degree of uniformity, and thereby enabling the physician to distinguish it from other diseases, yet experience teaches that the diagnosis of the disease is sometimes somewhat obscure, and must be based on observations made on a large num-

¹ Maclean, i. 238.

² Phil. Med. and Phys. Journ., xii. 109.

ber of cases ; and that if reliance were always placed on the result obtained in a few individual cases, the practitioner would incur the risk of arriving at erroneous conclusions. Like non-contagious febrile diseases generally—and, indeed, in a more marked manner than others—the yellow fever differs considerably, in its general aspects, in different seasons. A comparative examination of the various epidemics that have occurred in this and other countries, will show that no two of these resemble each other ; and at every return of the fever we discover that no less a difference presents itself at different periods of the same season, in different localities of the same infected place, and not unfrequently in various individuals residing in the vicinity of each other.

Everywhere and at all times we find cases differing in point of severity and in the force of reaction displayed ; everywhere we find cases manifesting an inflammatory character, or a greater or less degree of malignancy ; everywhere we note that at one season the one or other of these characters predominates. But this is not all ; we find cases that present trains of symptoms of so unusual and unexpected a nature, as to create a doubt, even in the minds of experienced men, respecting their claims to be considered in the light of true yellow fever, were they not linked together by, or accompanied with, one or more of the phenomena pathognomonic of the reigning pestilence, and showing these cases to depend on the same general cause as others of more ordinary appearance ; while others, again, are seen in which these pathognomonic signs are absent. What Dr. Maclean remarks in respect to epidemic diseases generally, in contradistinction to contagious complaints, will apply to the yellow fever : “There is, not only in respect to the pestilential maladies which occur in different countries, but to those which occur in the same country at different seasons, and even to those which affect different persons in the same country and in the same season, such a great diversity of phenomena, depending upon the variety of combinations and degrees in which the different organs are affected in different individuals, as to give to the same pestilence the semblance of many maladies of a dissimilar nature.”¹

In some cases, the disease is of so trifling a character as scarcely to confine the patient to his room or bed, and terminates favourably in a very short time. In other instances, the patient, though in appearance scarcely, if at all, indisposed—sitting up in bed or in his room, nay, even walking about, reading, smoking, or otherwise amusing himself—is suddenly seized with black vomit, and quickly dies, to the astonishment of those around. In others, again, the disease continues several, and even many days, and terminates either in recovery or death. To these peculiarities, which show the insidiousness of the disease, and justify in some respects the appellation of *proton*, which has been applied to it, may be added the fact that the period of its incubation, like that of non-contagious febrile diseases, and unlike those of a contagious character, varies considerably in point of duration—from a few hours to many days.

¹ On Epidemics, i. 238, 239.

Classes, Races, Sexes, and Ages affected.—In the history of no contagious disease do we find that it spares a whole class of individuals, unless these are protected by having already passed through an attack, or through another disease which possesses the power of preventing an attack of the former. The smallpox, when it breaks out in a community or family, exercises its baneful influence upon every one that comes within the sphere of its action, and leaves few untouched who have not suffered on some previous occasion or passed through the vaccine disease. The same remarks are applicable to every other complaint, the contagious character of which is well and incontestably established. In no country or city do we find the native population proof against the morbid agency of these complaints, and owing that security not to the protection derived from the means mentioned, but to the effect of an habituation to the peculiar condition of the atmosphere and to the temperature of the localities they inhabit. In other words, acclimatization is of no avail against contagious diseases, which everywhere attack natives, long residents, and recent arrivers with nearly, if not quite, the same certainty.

On the other hand, almost all diseases against which acclimatization is of avail are devoid of contagious properties, and are of local origin. As might be expected, this has been denied by contagionists, who maintain that the effect in question is observed to hold in regard to some forms of pestilential diseases, and attributing the exemption in these and other complaints usually regarded as non-contagious to the habit of breathing the noxious effluvia emanating from the bodies of the sick, reject all idea of deriving from the protection in question a proof of the non-contagiousness of diseases in which it is found to hold good.¹ But all that has been said in favour of this supposition will be found to apply to complaints—the plague, the typhoid fever, &c.—the contagious character of which has not as yet been fully established, while the facts cited, if founded in truth, lead to the conclusion that the diseases to which they relate were really non-contagious, or indicate that the protection obtained was the effect of a habit acquired by constant exposure to contagious miasms. Of the reality of the influence of such a habit there is no doubt; but it is of comparatively limited power, can only apply to a limited number of individuals (certainly not to a whole population), requires for its preservation a continuance of exposure to the morbid poison, is never acquired without such exposure, and is lost in a short time by a cessation of the latter. In these respects, therefore, the effect is different from true acclimatization, which is enjoyed by nearly the entire population of tropical regions; may be, and often has been, acquired by residence alone; and is obtained without exposure to the sick, and even although the disease may not have prevailed epidemically during the time of that residence.

Be this as it may; acclimatization, properly speaking, being of no avail in relation to contagious diseases, while complaints against which protection is obtained by its means are devoid of that property, and arise from the opera-

¹ Caillot, p. 216, 217.

tion of causes incident to the places where they occur, we may from those facts derive an argument against the contagiousness of the yellow fever; for in this disease, the power of acclimatization is as strikingly manifest as in any other known complaint. In another chapter it was shown that the natives and long residents of tropical climates, particularly of tropical cities where the disease prevails annually to a greater or less extent, are almost completely exempted from the disease; that the same immunity is enjoyed by natives and long residents of our southern cities; that individuals so protected carry this protective influence to, and enjoy it for a time at least in, other regions where they encounter the disease; that this influence, though often lost by a prolonged residence in cold climates, is regained after a while in tropical regions; and that it is obtained without a necessary communication with the sick, and even, though not so effectually, at periods unmarked by the epidemic prevalence of the disease. In all these respects, the yellow fever allies itself closely to non-contagious and endemic fevers, of hot climates particularly, and differs from diseases arising from, and propagated by, contagion. It may, therefore, be regarded as itself devoid of the latter property, and as resulting from the operation of causes analogous to those giving rise to the others.

The force of this argument appears conclusive, and had not escaped the attention of the older tropical physicians, by one of whom, Castalbondo (Jos. de), it was pointed out at Carthage, more than a century ago.

From the difference of effects of the yellow fever, and of contagious diseases on particular races of individuals, on the two sexes, and at various periods of life; and, conversely, from the analogy between the effects of the former and those of endemic fever, which are known to be void of contagious properties, we again arrive at the same conclusion respecting the non-contagion of that disease. We have seen, in a former chapter, that the yellow fever spares either completely, or in great measure, individuals of the negro race—and that if these take the disease, they have it in a milder and less fatal form. We have also seen, that the fever is usually less prevalent and severe among women, persons advanced in life, children, individuals of a lymphatic and weakly temperament, than among men, adults of middle age, and persons of a robust and sanguine constitution. On all these peculiarities as to the selection of subjects, the disease allies itself, as has already been stated, to the endemic remittent fevers, of various grades and forms of hot and other climates. From these, negroes are usually in great measure exempt, or they have them in a milder form; and what has been said of the difference of susceptibility to the yellow fever of the two sexes, and of persons of different ages, constitution, &c., applies, on some points at least, equally well to the other forms of fever mentioned. If, with these facts before us, we turn to diseases of undeniable contagious properties, we arrive at a different result. From them the negro race suffer as readily as the white. To smallpox, indeed, blacks are particularly obnoxious in hot and other climates, as was shown during the extensive epidemic of that disease in this city in 1823–24. In these diseases, the two sexes suffer almost equally; while differences of age and temperament avail nothing—the young and old,

the feeble and the strong, when unprotected by a former attack (or in the case of smallpox by vaccination) being as readily attacked as others differently situated.¹

CHAPTER XXIX.

CONTINGENT CONTAGION.

CONTAGIONISTS, as we have seen in a former chapter, are divided into several classes. The 1st embraces those who believe the disease to be invariably contagious; regarding, indeed, the power of communication to be like that of other kindred affections, more actively displayed in crowded, ill ventilated, and filthy places, and rendered somewhat less so, under opposite conditions; but always more or less manifest under whatever circumstances it may be observed. The 2d class includes those who, though believing the disease to be naturally endowed with contagious properties, admit that the latter requires for its manifestation the influence of an impure atmosphere; in other words, that a pure atmosphere so far neutralizes the contagious virus as to prevent its manifestation; while in an impure air it will almost certainly show itself and exercise its baneful influence. A 3d class embraces those who affirm that the disease, though not naturally endowed with contagious properties, may and sometimes does acquire these through the influence of the peculiar circumstances to which the contagionists of the second class attribute their manifestation—impure air, crowded and filthy localities, &c. The difference between the adherents of the first, and of the second and third of these classes is that, according to the former, the disease is *always* contagious, and according to the others only contingently so. Between the second and third, the difference is but trifling. The two classes merge into each other; for, according to both, the disease, through the force of the same influences, is often free from a contagious character, and at other times endowed with the power of communication, the difference resting on the peculiar agency which the impure air is supposed to exercise towards the accomplishment of the result. In the one case, the impure air calls into play a morbid property which otherwise would remain latent and harmless; in the other, it is instrumental in generating a property not before appertaining to the disease. The one set believe the power to be inherent, while, agreeably to the other, that power is acquired. According to the first of these classes, the disease though occasionally, but rarely, of spontaneous growth, is almost invariably the offspring of a contagious transmission; according to the second, it is sometimes, especially in certain climates, the effect of causes analogous to those of endemic fevers; and, according to the third, it arises almost invariably from the operation of those causes.

¹ S. Brown, on Yellow Fever, p. 88.

As regards the doctrine of constant and invariable contagion, little need be said in this place. The facts and arguments adduced in preceding chapters, show, unless my labour has been fruitless, that it is not tenable. It will be sufficient, therefore, to refer to what has already been said on the subject.

Indeed, it is as much to a conviction on the part of some—who, having early embraced and warmly defended the doctrine of contagion, felt loth to abandon it completely—of the impossibility of sustaining the ultra views in question, and of denying the, at least, occasional absence of such a property, as to a belief entertained by some of the upholders of the local origin of the disease, that, in some instances, domestic causes appear insufficient to account for the propagation of the latter, that we are indebted for the doctrine of contingency. Be this as it may, from this change from pure and uncompromising contagionism to a doctrine of a more modified character; as well as from the present limited number of those who entertain those ultra views, we derive a strong argument in opposition to their correctness.

The doctrine of the requirement of contingent circumstances to excite into activity the contagious property of the disease, has long been advocated. In this city, it was, as stated, first suggested by the late Dr. Currie, in his account of the epidemic of 1799, and afterwards upheld in several of his other writings.¹ It was also advocated by Dr. Seaman, of New York (*Rep.*, i. 331) and Dr. Barnwell (p. 374), and has since enlisted in its favour Palloni (p. 14); *Ed. Journ.*, ii. 88; *Reposit.*, viii. 426; Strobel (p. 207); Monette (pp. 51, 55, 61); Whitmarsh (*Bancroft, Seq.*, p. 166); Townsend (p. 224), and others. But no one, in this country, at least, has taken more pains to propound and propagate the doctrine than the late Dr. Hosack, of New York, who, however, did little more on this subject than amplify or modify what had been suggested by others. In a letter to Dr. Chisholm, on the classification of contagious diseases, written in 1808, and published in an early volume of the *Edinburgh Med. and Surg. Journ.*, v. 427, as well as in a variety of subsequent works,² Dr. H., placing the yellow fever with the plague, typhus (jail, ship, hospital, or lake fever), and dysentery in a separate class—the third—the 1st consisting of disease communicated exclusively by contact; and the 2d, those communicated both by contact and the atmosphere), which he thinks are only communicated in general through the medium of an impure atmosphere, remarks that, “in a pure air, in large and well ventilated apartments, when the dress of the patient is frequently changed, all excrementitious discharges immediately removed, and attention paid to cleanliness in general, these diseases are not communicated, or very rarely so, from one to another. But in an *impure* air, rendered so by the decomposition of animal and vegetable substances, as takes place in low marshy countries, or by concentrated human effluvia, as in camps, jails, hospitals, or on shipboard, they

¹ See Chervin, *Opinion des Médecins Américains*, p. 14.

² *Observations on the Laws of Contagion*, 4to., pp. 5, 6; *Med. and Philosophical Register*, ii. 14; *Essay*, i. 258; *Med. Police*, p. 6; *Ib.*, in *Essays*, ii. 14.

are rendered not only extremely malignant and mortal in themselves, but become communicable to others who approach the sick, or breathe the same atmosphere, which has become *assimilated* to the poison introduced, inso-much that *the same specific disease* is communicated, whether it be the *plague*, *yellow fever*, *typhus*, or *dysentery*."

The advocates of the other branch of contingent contagionism—who believe that the disease, though not naturally and ordinarily contagious, may become so through the influence of the modifying agencies mentioned, are more numerous than those of the other. In this city and country, the opinion of this contingency was formerly very generally admitted. It was upheld by Dr. Bond,¹ and by Dr. Rush, till his final retraction in 1802;² as also by Drs. Caldwell, Dewees, Cox, Physick, Reynolds, Sayre, Otto, Boys, Cooper, Stuart, Pascalis, and Strong.³ It has also been, and is to this day, advocated by numerous physicians of more or less note;⁴ while others content themselves with admitting, theoretically, the possibility of the occurrence, but are far from affirming a decided belief in it.⁵

But numerous and ably defended as these two modifications of contingent contagion may be, and plausible as some of the reasons assigned in their favour may appear, they are both open to objections—faulty in principles, and unsupported by the very facts adduced in their support. Far be it from me to deny or doubt the influence of the contingent circumstances to which reference has been made, in modifying, and, to some extent, controlling the manifestation of contagious and other diseases. On that subject my opinion has already been expressed in a way that cannot be misunderstood.

¹ N. A. Med. Journ., iv. 271.

² Rush, Fever of 1793, p. 160; 1797, p. 37.

³ Ibid., p. 57; Letters by Acad. of Med. on Fev. of 1797, p. 13.

⁴ Bayley, Fev. of 1795 in New York, pp. 14, 50; Hennen, Top. of Medit., p. 108; Humboldt, ii. 770; Rochoux, Typhus Amaril, pp. 127, 154, 166; Ib., Dict. de Méd., article Contagion, p. 546; Copland, ii. 238; Brit. and For. Med.-Chir. Rev., i. 66, 384–8; McWilliams, p. 104; Blane, i. 317; Fellowes, p. 403; Archives, v. 287; Burnett, pp. 17, 276; Ib., Fev. of Ascension, pp. 18, 25, 52; Ib., Correspondence respecting the Eclair, pp. 55, 78; Bally, pp. 352, 390, 404; J. H. Dickson, Edin. Med. and Surg. Journ., p. 128; James Johnson, on Trop. Cl., p. 163; Lind, Hot Cl., pp. 90, 130, 135; Dancer, p. 81; Warren (in Tytler), p. 500; Gillespie, pp. 8, 62; Denmark, Bancroft's Seq., p. 155; O'Brien, Ib., p. 162; Donnet, Ib., pp. 165, 385; Amiel, in Johnson, p. 270; Pardon Bowen, Med. Reg., iv. 338, 344; Robert, Guide Sanit., i. 131, 219; Rogers, Davidson, Forsyth, Alexander, Ducachet, doc. furnished to Dr. Chervin; Op. des Méd. Am., p. 101; Rapport, pp. 11, 20, Ceresa, Med.-Chir. Rev., ii. p. 384; Porta, Periodico de la Soc. di Salud. Pub., N. 222; Salva, Analysis de la Fièvre Amarilla, and other Sp. physicians; Decados de Med. Chirurgie, vi. 377; Boyle, p. 269; Faure, Arch., xviii. 587; Belcher, Edinb. Journ., xxiii. 247; Bryson, p. 45; Clark, Collection, p. 61; Trotter, i. 213, 214; Humphrey, in Johnson, p. 276; Edinb. Journ. 1816, p. 178; Ferrari, Edinb. Journ., xix. 375; Leblond, pp. 11, 106, 281; Lefoulon, pp. 277, 351; John Clark, on Long Voyages, i. 153, 166; Blin, p. 44; Broussais, Ann. de la Méd. Physiol. Oct. 1822; Palloni, p. 29; Med.-Chir. Rev., April, 1841, p. 458.

⁵ R. Jackson, Sketch, i. 31; Ib., Outline, pp. 221–3; Ralph, Edinb. Med.-Chir. Tr., ii. 62; Doughty, pp. 209, 210; J. Clark, p. 23; Arnold, p. 6; Gilbert, pp. 51, 60, 70–1, &c.; Desportes, i. 40; Inmray, Edinb. Med. and Surg. Journ., liii. 93.

The effect is produced in all zymotic diseases, whether contagious or otherwise, which are more prevalent and severe in a foul and close than in a pure atmosphere. But all this is very different from bringing out a latent or generating a contagious property. What is true of a non-contagious fever is true of smallpox, scarlatina, measles, and typhus. Foul air, as well as other morbid agencies—putrescent food, foul water, imperfect ventilation, starvation, intemperance, by lowering the tone of the system, diminishes the power of the latter to resist the force of the morbid impression of the efficient cause of those diseases. But they do not generate a new attribute in them. The contagiousness of smallpox or measles is never produced by them; and if those agencies operate in a particular way in these diseases, it is safe to conclude that their effect is similar in other complaints. They predispose the system to an attack, they render the disease more virulent; some of them may produce peculiar forms of fever. They may effect all this without being able to generate in the system a contagious poison which would not have been there without their agency.

It may be remarked, that by the adoption of the doctrine of contingency—particularly of the second branch of it—a great approach is made to that of non-contagion, for by those who advocate the idea of that form of contingency, the disease is admitted to have a local origin in the majority of instances, and to become contagious only under the peculiar circumstances stated; and by those who prefer the other form, the same mode of origin is recognized to occur sometimes, and in some places—if not everywhere within the yellow fever zone, at least in certain sections of it; whilst by all the disease is recognized to be sometimes, if not generally, free from contagious properties. By admitting, therefore, that the disease is not contagious in a pure atmosphere—in localities properly ventilated and free from undue crowding—the contingent contagionists have given up at least one-half of the means of support of the doctrine of contagion, properly so called; for it is as much to facts of this kind as to any others of a more positive nature, that the advocates of the latter have invariably appealed.

If such be the case—if the disease requires for the manifestation of contagious properties the aid of an impure air, what becomes of the account we have from Drs. Tilton, Sayer, Stanton, Wistar, and others, of its introduction in Wilmington, Marcus Hook, Swedesborough, Germantown,¹ and other situations, which the opponents of the local origin of the disease affirmed in the most emphatic terms, were free from all sources of atmospherical contamination? What becomes of the cases of “communication of the yellow fever in the pure air of the country” recorded by Dr. Hosack?² What becomes of the many other instances of the same import adduced by some of the contagionists of our southern and southwestern States, and by those of Spain? One of two things must be admitted—either the disease is contagious

¹ Currie (1798), pp. 136, 138; Additional Facts by College of Physicians, pp. 36, 72.

² Hosack, Essays, i. 291; Spalding, Med. Repos., iii. 8; Moore, Med. and Philos. Register, ii. 21.

out of an atmosphere charged with poisonous exhalation, or the accounts in question, as well as many others that could be collected, and which are still referred to by contagionists generally, are not to be relied upon. These cases, and others recorded, and which they have triumphantly brought, and continue to bring forward as convincing proofs of contagion, are, it is true, regarded as exceptions to a general rule, and adduced in illustration of the strong tendency the disease sometimes has of manifesting or assuming a contagious power; but if the doctrine were correct, such exceptions would not occur, or at least would be less numerous than they have been represented to be, while every one must perceive that their occurrence may be doubted, from the fact that they conflict, as we have seen, with numberless instances in which the disease has failed to be communicated under circumstances to which the contingent contagionists attach so important an agency.

By adopting the idea of the disease being contagious, but requiring for the manifestation of that property the concurrence of contingent influences, and pertinaciously rejecting the fact of local origin in this country, Europe, or other places, physicians have been led into contradictions, and have hazarded assertions which do not always reflect credit on their understanding, their learning, or their judgment. Thus, Dr. Hosack gravely tells us that the yellow fever arises in the West Indies from the action of a high temperature upon the unacclimatized constitution—"and this frequently aggravated by other exciting causes, as violent exercise, ardent spirits, free living, exposure to night air, &c." This disease, according to him, is not limited to *Siam*, in the East Indies, as its birthplace, as has been believed by some writers; nor to the *coast of Africa*, as urged by others, but extends to the tropics in general. Here, then, we have a fever originating—without prior contagion or contamination—not in one part, but in the whole, of a vast expanse of the earth's surface, from the action of a high temperature on particular constitutions; and yet the same and other writers deny the possibility of such results occurring here or elsewhere, where they cannot deny that the same causes exist during a certain period of the year, and where the same constitutions abound. Dr. Watts (p. 317), in commenting on this subject, remarks that the disease thus created within the tropics without the aid of contagion, having passed the bounds of the locality where it had sprung up, spreads its ravages in distant places, the climate of which is supposed not only unfavourable, but hostile to its production. But in these places, so incapable of engendering it, it acquires the character of a contagious disease, and is spread by personal communication with the sick; but this property of contagion is again lost as soon as the sick are removed to a pure air.

"This peculiarity," says Dr. Watts, "puts us in possession of a very singular species of contagion, that may be fearlessly and harmlessly imbibed in the vehicle of pure air, but becomes a noxious and fatal poison when wafted to us in a tainted atmosphere. It presents to us the singular and novel compound contagion, made up of the essence of disease and of the atmosphere itself" (p. 318).

Drs. Griffitts and Parks, of this city, were of opinion that the yellow fever is not contagious in the West Indies, but becomes so in filthy and crowded vessels in their passage thence to the United States.¹ To the same effect, Sir James Fellowes says: "It is possible that persons coming from Vera Cruz or the Havana, and carrying with them the seeds of diseases admitted to be endemic in those places, might, during their passage in a crowded ship, undergo such a change of constitution as to produce the disorder, with the additional property of generating it in others highly predisposed" (p. 403). The advocates of such views have neglected to inform us how the disease acquires, by transportation, the property in question. Is it by fomites? But the place of its origin can scarcely be thought to furnish fomites capable of reproducing the same disease, and especially of imparting to it a property it did not possess before. Is the effect supposed to result from something formed in and adhering to the vessel itself? If so, why are not similar effects observed while the vessel remains in the West Indies? The fever often prevails most extensively in such vessels while at anchor in tropical ports, or while in those localities, or sailing in tropical seas; and if, in passing over to temperate regions during the hot season, the disease undergoes the important change in question, it is difficult to understand why it does not undergo it in the same vessels while they remain in hot latitudes—the very place of origin of the fever according to these physicians, and where there is as much heat, filth, and other such causes, as in this country or in any other place where the disease is said to be carried.

These inconsistencies are scarcely less glaring than those of Pariset, who traced the fever of Spain to the United States, where he admits it is not contagious; or even to Calcutta, where he acknowledges it never has existed at all; and of Chisholm, who referred its origin to the island of Bulam, and the acquirement of contagious properties to the influence of contingent causes on board the ship Hankey—properties it would appear to have retained ever since, and invariably exhibits in the highest degree wherever it has manifested itself.

But the doctrine of contingent contagionism of the Hosack school appears faulty in principle, and withal purely hypothetical, and sustained by false and irreconcilable analogies. Pure contagionists, as Chisholm,² say that it merely increases the susceptibility to the impression of the virus already received into the system. By some physicians, as Currie, impure air acts as a conductor (p. 12 [1799]). Dr. Hosack, dissatisfied with these explanations, maintains that a vitiated atmosphere produces some chemical combination with the virus secreted from the diseased body, and that thereby the contagion becomes more or less extensively multiplied, according to the extent and virulence of such vitiated atmosphere.³ Rejecting the idea of a *tertian quid* being formed in the operation, he conjectures that the contagion

¹ See Chervin, Rapport de l'Académie, p. 16.

² Strictures on the Classification of Contagious Diseases proposed by Dr. David Hosack and by Chisholm, Med. and Phil. Reg., ii. 125.

³ Laws of Contagion, p. 35, and Essays, i. 303.

is augmented by a process very analogous to the assimilation of the fluids of the living body to the primary taint—as in smallpox or syphilis, or to what happens in fermentation (*Ib.*, pp. 35, 304).

It has been remarked, that if we suppose the cause of yellow fever to enter into chemical combination with the surrounding foul atmosphere—if we admit that such a thing can take place as assimilation or combination between the product of living and dead bodies—a contagious virus or poison and noxious exhalations from animal and vegetable substances in a state of decomposition—an entirely new compound must be formed, the effect of which will be a disease differing essentially from the original one; for it is not natural to suppose that the same effect can be produced by a morbid poison and by a compound formed of that poison with exhalations of a totally distinct nature.

If, on the other hand, the air serves merely as a medium for transmitting the poison to a greater distance, no reason can be assigned why, if we approach near enough to the sick, contagion should not display itself as well in a pure as in an impure atmosphere.¹ The emanations from the sick must exist, in greater or less abundance, under all conditions of the surrounding atmosphere, and their morbid agency on those who come within their reach cannot cease to be exercised. We are yet to find facts to prove that a pure atmosphere neutralizes or destroys contagion. So far from it, experience demonstrates the impropriety of entertaining such a belief. That when diffused through an extensive space, the atmospheric air not only lessens the virulence, but sometimes renders harmless the power of contagion, is doubtless true; the effect, under such circumstances, is obtained—not by the decomposition of the contagion—but by its dilution and dissipation; and whenever the diluting and dissipating agent is rendered more circumscribed, and thereby less efficient, the power in question becomes again manifest. Nor is it less true, that diseases, as regards the contagiousness of which there is no doubt, derive no additional force from an impure or confined air, other than that resulting from the concentration, within a small space, of a greater quantity of contagious matter, and an increased degree of susceptibility to its impression accruing to those placed under its influence from the exhalations of a sick room, and the bodily fatigue and the mental anxiety therein experienced.² But such diseases, though modified in the way mentioned, are always, and everywhere more or less contagious; instances where they are not so, are exceptional. They require no impure localities to render their power of transmission by personal communication evident.

The doctrine is not less evidently invalidated by an appeal to facts; for, on inquiry, it will be found that the disease is not more communicable in an impure than in a pure atmosphere. Were the doctrine correct, we might expect to find the disease manifesting contagious properties in all places where the air is impure and confined, where there are accumulations of filth,

¹ Beck, Appendix to Gregory, p. 653; N. Y. Med. and Phys. Journ., ii. 483.

² Miller, Works, p. 96.

and where the population is dense. Now we have already seen that during all our epidemics, and the epidemics of every country, individuals labouring under the yellow fever when carried to, or attacked after exposure in infected districts, in the most impure, crowded, and unhealthy localities, may uniformly be approached with as perfect an impunity as in the pure air of the country. The disease is never propagated there by the sick; it terminates with the recovery or death of those carried there. We have seen, besides, that the same absence of contagious communicability is observed in hospitals filled with yellow fever or other cases, and where surely, if impure air elicited the manifestation of contagion, the effect ought to be produced. And surely, if such is the fact in localities of the kind—if individuals there are not affected, though fully exposed—and if, at the same time, others suffer from the disease though placed beyond the possibility of such exposure, there is no necessity to attribute the cases that occur to exposure, and to regard the effect as due to the generation of a new element, or to the revived activity through the instrumentality of a peculiar condition of atmosphere of one forming part and parcel of the disease, but which heretofore had remained in a latent or quiescent state.

That cases of fever break out in such establishments, and in certain localities among persons approaching the sick, is a fact too true to be denied. On examination, however, it will be found that such occurrences are to be met with in districts only where the disease prevails, and hence cannot lend support to the doctrine in question; for, as has been already shown, the disease is there propagated not less to individuals having no communication with the sick than to those who approach these, or even after these and the whole population have been removed.¹ Were it otherwise, we would be compelled to admit that the air in such localities must be more venomous than the poison evolved directly from the diseased bodies in other situations; in other words, the poison which in the infected district containing patients labouring under the yellow fever acts at a distance from these, and is consequently diluted, and which, when all the sick have been withdrawn, may well be supposed to be still more diluted, must nevertheless, if the doctrine be well founded, be more powerful than the pure poison itself, whatever be the circumstances under which the latter is applied.

Now, if the disease be contagious in an impure atmosphere, and in no other, it must have acquired that character from some peculiarity in that atmosphere. If so, the conclusion is natural that the principle of contagion resides in the air itself, and that the latter, under these circumstances, produces the disease, independently of the sick that may exist in the place. Nor will this conclusion, as to the independent and sole agency of the impure atmosphere in the production and the propagation of the fever, apart from contagion, fail to appear still more satisfactory and irresistible when we find the effect produced without the presence of any sick, to the exhalation from

¹ See Chervin, *Rap.*, p. 27; Watts, pp. 230, 243; Bally, p. 154; Burnett, p. 346; Maclean, on Epidemics, i. 350; Musgrave, *Med.-Chir. Trans.*, ix. 110, 116.

whose bodies it can be attributed; while it constantly fails to arise from communication with the latter in other localities. Nay, we may further conclude that if the disease is the product of the impure air of an infected district; if, as already shown, its production is generally connected with the presence of local sources of exhalation; if it has originated in ship or on land under circumstances which leave no doubt of its being the result of these atmospheric impurities; and if it is not communicated, even in an impure atmosphere, away from the infected district, we must refer it—whenever it breaks out in any place—to the presence or extension of such exhalations, and not to contagion, however difficult it may sometimes be to discover the source whence the poison is derived.

A very distinguished writer of this country, the late Dr. Drake, has properly remarked that the hypothesis under consideration recognizes the action of two causes, and that if either be absent, the fever will not appear. It may be called the hypothesis of compromise, for it tolerates or requires all that both parties demand. According to this writer, it may be objected to this hypothesis that it combines an admitted and an assumed cause: the local contamination is a fact—the contagion an assumption; that in many invasions of the fever, cases have occurred under circumstances which almost preclude the possibility of communication with the sick, and must therefore be referred to the admitted local cause; and that having admitted one cause, which appears sometimes to have been sufficient of itself for the production of the disease, it is unphilosophical to admit another, the existence of which has not been proved (ii. 290).

It is not less worthy of remark that the impurity of atmosphere, which, agreeably to the doctrine of contingent contagion, is requisite to produce the effect in question, is that of particular places, but not all kinds of aerial contaminations. As a general rule, it may be said to be due to the impurities of towns, not to the impurity of the country—of our wharves, not of swamps; for if a case of yellow fever, or a ship or steamboat having the disease on board, or simply the supposed seminum of contagion, the fever may be, and often is, according to the contingent contagionists, produced in the former, while the arrival of such patient, or ship, or steamboat in the latter will never or seldom be productive of mischief. Indeed, if the many facts to which I have called attention can be relied upon, no effect will be produced although the fever is introduced in places in which the atmosphere is, and cannot be otherwise than, very impure—as in hospitals, jails, ships, courts, alleys, &c.—provided those places be situated beyond infected localities. We may, then, take it as a settled fact that the yellow fever poison exerts the transforming power in question only in the impure air of certain localities, and that in the absence of such impurities there is and can be no spread of the disease. Dr. Drake, like others, had noticed the objection which this fact furnishes against the doctrine under consideration, and enters into some details which have reference to an etiological point I shall touch upon in the next chapter, and apply to the subject before us. In town localities, the impurity of the air produces often, during the intervals between yellow fever

epidemics, other fevers bearing a close resemblance to those occurring in rural districts. Now, is it not remarkable, when these malaria approach so near to perfect identity, that the imported disease should become contagious through the agency of the civic malaria, but fail to become so when brought under the influence of the other?

From a consideration of all that precedes, we may unite in sentiment with those who maintain that to consider a disease as contagious, which at the same time exhibits no appearance of that quality but in certain climates; in such climates only in certain places, at such places only in certain seasons, and even at such seasons only after a particular degree of heat and moisture, and in an impure atmosphere, is undoubtedly to lose sight of all the established properties and laws of contagion.¹ All the facts regarded as favourable to the doctrine are more readily and satisfactorily explained in some other way. They mostly have taken place in localities where the disease occurs without personal communication with the sick and would, doubtless, have all done so, though there had been no sick to justify the idea of contagion, and hence if the local causes existing in those localities are of themselves fully competent to produce the disease without the co-operation of any contagious principle, the supposition of the existence and agency of such a principle is unphilosophical and inadmissible as it implies the co-operation of two causes to produce the same effect.²

Similar, and, indeed, stronger objections may be raised against the views of the other branch of contingent contagionists—those who, while admitting that the disease is almost invariably the offspring of local and domestic causes, admit also its assuming often a contagious character through the action of the agencies already enumerated. That it should enumerate a number of advocates cannot be a subject of astonishment. It is convenient, of easy application, and removes all difficulties—sometimes of a puzzling nature. Let the disease occur in its favourite localities, and where sources of poisonous exhalations are obvious to the senses, and when intercourse with the sick cannot be traced, it is recognized as being the effect of those exhalations, and the idea of contagion is discarded; but let it spread rapidly; let it present itself in situations where such causes are not easily discovered, and let it occur in those who chance to have had intercourse with the sick, then the disease is supposed to have acquired a contagious power by which, under the agency of particular circumstances, it is enabled to propagate itself.

It may be remarked, that the facts which are supposed to lend support to the belief in this occasional manifestation of contagious power on the part of the disease, have mostly occurred in infected districts, and may, therefore, be accounted for as readily and more consistently on the principles of local causes; that, as already said, other cases arising in different localities should not, without the most convincing reasons, be ascribed to a mode of propagation, which, it is conceded, forms no natural attribute of the disease; but should be referred to its usual cause, though this may not be always easy to

¹ Miller, Works, p. 102.

² Bancroft's Sequel, p. 194.

discover; and that admitting, that in a few instances, circumstances are such as to appear to sanction the belief in the engraftment on the disease of a power of which it is naturally destitute, such cases being in diametrical opposition to thousands of others, it is more reasonable and philosophical to suppose that some error has crept into the detail and history of the former than to recognize the occurrence of a transformation which nullifies all our previous knowledge in regard to the causation of the disease.¹ No one, so far as I have ascertained, has as yet been able to point out the time when, and the manner in which the fever is revolutionized to such an extent in its nature as to pass from a non-contagious to a contagious disease. "It does not appear that the change can be determined by rapidity, or extent of progress, severity of symptoms, or amount of mortality, or by anything within the cognizance of the senses, on which the mind can fix as a limit, to separate the disease as originally constituted, from the disease as altered in its nature and power by contingents. If there be nothing, in the character or effects of the disease, to determine when it has acquired the contagious property, how can the existence of that property be ascertained?" In the instance before us, as in others of a kindred nature, the operation in question is mostly inferred from the difficulty, in some cases, of accounting for the occurrence and progress of the disease on any other principle; and sooner than simply recording their inability to discover, in these cases, the existence of the ordinary cause of the disease, and acknowledging that these may exist, though they are not evident to the senses, the advocates of the doctrine do not hesitate to attribute the effects to a power very different from that to which the disease commonly owes its origin.

Contagious diseases require for their production the agency of specific causes. Each of them is the effect of a special cause which is essential to its production, and gives rise to no other complaint. Again, as Dr. Caldwell remarked long ago, there are no half truths in medicine any more than in religion. Hence, a disease is contagious, or it is not. A complaint contagious only under certain circumstances is not contagious at all. "If yellow fever is contagious," says Dr. Barton, "*it is a law of the disease*. This, it must carry into all places and under all circumstances (like smallpox). A *contingent contagion* is a medical misnomer, is void of a precedent, and has no parallel in the annals of the science."² In other words, no disease is ever contagious unless it has originated from contagion, and contagious diseases can only be produced by their respective contagions.³

I am not unmindful of an observation long since made, that, in some instances, diseases of an indubitably contagious character, and some, upon the contagion of which some doubts are entertained, appear to arise spontaneously, and cannot be traced to an antecedent communication of those affected with the sick. But this spontaneousness of origin of a contagious power, cannot be adduced as evidence of the supposition of the generation of a new character in yellow fever. The local causes of the latter, accord-

¹ Bancroft, p. 285.

² Sanitary Condition of New Orleans, in 1853, p. 273.

³ Arnold, p. 62.

ing not only to the non-contagionists, but to the second class of contingent contagionists are evident. In the case of the other diseases referred to, no such causes can be assigned to them—they never arise from local sources of exhalation, however modified and concentrated these may be. We know, in fact, of no cause that can produce them, except the specific poisons to which they are ordinarily found to be due.¹ When they occur without our being able to trace them to these, the result is mysterious, unless Dr. Williams is right in supposing the cause or poison of these (of some at least) to exist, generally diffused through the atmosphere, at all times, but varying in quantity and intensity, as scarlatina, measles, coryza, whooping-cough, and plague.² Unless we admit the development by circumstances which escape our means of observation of a germ heretofore dormant and concealed, we must confess our ignorance of the mode of occurrence; but we cannot conclusively deny the agency of the true poison and attribute the disease to a new creation. Be this, however, as it may, there is in such instances no transformation of a non-contagious into a contagious disease, as is maintained in reference to the yellow fever.

By admitting this modification of a contingent contagionism, we should, as already stated, violate a well-established rule in philosophizing, by having recourse to more causes than are sufficient to explain the phenomena in question; first referring the disease to a local cause, and next maintaining that at times it is due to another species of poison, of a diametrically opposite character.

In a word, we should commit, what the late Dr. Currie, of this city, long ago called a physical absurdity (*Bil. Rev.*, p. 200), by admitting that the same specific disease may be the product of two poisons essentially different in their nature, and proceeding from sources of a very opposite character—the one a miasmatic poison arising from the decomposition of dead organic matter; the other an animal poison, emitting from the living body, and secreted by a morbid action of the vessels, or a peculiar process of the disease.

Dr. Good (ii. 65), it is true, and perhaps a few more, maintains that human and other effluvia are identical in nature, and, therefore, can produce identical effects. But nothing that has, as yet, been adduced on the subject, has been able to shake the belief in the great dissimilarity of the poisons in question, and to justify the conclusion that diseases resulting from their action on the living body must differ materially from each other also. For this reason, it may be doubted whether complaints arising from local causes, or materials having no connection with the human body, are ever contagious from their origin, or can become so at any period of their progress, seeing that this power depends on a poison elaborated in the living human system.³

¹ Deveze, p. 140; Beck, N. Y. Med. and Phys. Journ., ii. 483; Miller, p. 95; Bancroft, p. 104; *Ib.*, Sequel, pp. 194, 303.

² Williams, on Morbid Poisons, i. 117, 167, 259, 304; *Ib.*, ii. 267.

³ Blane, Dissert., ii. 119; Armstrong, on Typhus, p. 299; Snow, on Continuous Molecular Changes, p. 34. Lond. 1853.

In support of the idea of the yellow fever becoming at times endowed with contagious properties, it has been alleged that the ordinary bilious remittent fever of this and other countries, dysentery, puerperal fever, cholera, &c., when they reign epidemically and assume a malignant character, acquire similar properties, and are communicated from the sick. It is, moreover, alleged that typhoid or typhus fever, though arising often from malarial exhalations, is nevertheless generally contagious; and that the latter fever, which is always so, has not unfrequently been the result of the accumulation of individuals—healthy or sick—in confined, filthy, and ill-ventilated localities, thus exhibiting an example of the new generation of a specific contagious virus. On these points, however, doubts, to say the least, may reasonably be entertained.

I have never been able to satisfy myself of the contagiousness of bilious fever; and among the practitioners of our southern, western, and other States, where the disease prevails extensively, it would be difficult to find one who entertains a different sentiment. That disease may accrue under circumstances such as have been mentioned, from communication with cases that have taken a malignant or typhoid character, may be true; but the disease thus produced is not the bilious fever, but a typhus fever, which, when the former prevails epidemically or endemically, may be associated with bilious symptoms. Indeed, on examination, it will be found that most of the instances of supposed contagious communication have occurred in localities where the disease prevailed more or less extensively, and where, consequently, we cannot say positively that the attacks would not have taken place had no communication with the sick been held. The same thing may be said of dysentery. The disease has long been, and continues to be, held as void of contagious properties. Dr. John Hunter, more than half a century ago, averred that it did not appear to be infectious in the hospitals of Jamaica, nor during the epidemic which prevailed in London in the years 1779 and 1780. While not disposed to deny the possibility of the disease being at times contagious, that clever writer shrewdly remarks that “there is some difficulty in determining a question of this kind; for unless the proofs of infection (contagion) are clear and decided, they may be easily confounded with the effects of the cause that is generally diffused and co-operating upon all more or less, such as the cause of dysentery must be.”¹ Like bilious fever, dysentery only spreads by personal communication when combined with symptoms of a typhous character; nor is it less possible that the accumulation of dysenteric patients, under circumstances inimical to health, will create disease; but that disease will be some form of malignant fever, combined, if dysentery prevails epidemically, with symptoms of the latter.² It may happen, also, that the exhalations issuing from the bodies of the sick, the vitiated atmosphere of the localities where they are crowded, and the noxious effluvia arising from their excretions, irritate the gastro-

¹ Transactions of London College of Physicians, p. 177. See also Ballingale, p. 40; Williams, ii. 579, 580; Dublin, Hosp. Reports, iii. 10, 76; Renton, Edinb. Med.-Chir. Trans., ii. 376; Bancroft, pp. 543, 544.

² Broussais, Phlegm. Chron., iii. 24; Wood, 1st ed., i. 570.

intestinal mucous membrane, especially in hot and damp weather, and will give rise to the disease in times of epidemic. But in all this no one can discover the existence of contagion, properly so called.

As regards the cholera, I can have little to say, being a decided disbeliever in its contagiousness—an opinion entertained by a large majority of the profession.¹ Nor shall I dwell on the subject of erysipelas, the contagiousness of which is denied, and I think justly, by the highest authorities. It is a disease frequently observed in the hospitals of Paris. Nevertheless, Andral remarks that the opinion of its contagiousness “retains advocates nowhere but in England. It will not bear examination, and is daily overthrown by the result of observation.”² Even Dr. Williams, who inclines to that opinion, disbelieves the power of the system, under certain circumstances, to generate the poison, which he refers to a principle existing at all times in the atmosphere (i. 259). Hence, even if this be correct, there is no transformation of a non-contagious to a contagious disease. For what relates to the supposed contagiousness of puerperal fever, I must content myself with referring the reader to the work of my excellent friend, Dr. Meigs, who has investigated the subject with his usual talent and ingenuity.

I will go further, and maintain that typhus and typhoid fevers—whether they be the same or separate diseases—are not only originally non-contagious, but are never transformed, strictly speaking, into a disease of that kind. They are not produced by secreted poisons, but by effluvia generated by foul excretions, often by noxious miasms having a different origin. But whether the effluvia arise from the body of the sick, or from other sources, these diseases are the result of the decomposition of substances which either never were, or no longer are, connected with the body; and when they are communicated to those coming in contact with the sick in localities calculated to further the concentration or increase the amount and virulence of those effluvia, they cannot justly be said to have been transformed into contagious complaints, since neither in such cases nor in any other are they produced by the secretion of specific poisons, which alone could impart to them that character. That this is true, we may infer from the fact that persons in health, when long confined in close and filthy apartments, will often produce a miasm similar to that which frequently surrounds the bodies of those affected with malignant fever. It is easy to foresee that such excretions will be more tardy in their effects than those eliminated in putrid fevers, for the latter are in a vitiated or putrescent state at the very moment of their formation, and must hence necessarily become rapidly pestiferous. But no material difference exists between them, as is obvious from their producing similar effects. Were those diseases the offspring of specific secreted poisons, they could not originate in, and be transmitted by, persons in health at the time, and remaining so, for it

¹ Jamison, Twining, Annersley, Orton, Moreau, Taylor, India, Smirnow, Lefevre, Gibbs, Russel, Barry, Chambert, Brierre de Boismont, Gallois, Jännichen, Foy, Penel, Verat, Baum, Gendrin, Molison, Mackintosh, Budd, Tullock, and Williams.

² Cours de Pathologie Interne, p. 460.

is impossible to suppose that these would not be affected by poisons secreted in, and eliminated from, their own systems—the very secretion of such poisons implying the existence of disease in the system so secreting them.

CHAPTER XXX.

NATURE OF THE YELLOW FEVER POISON.

THE many facts that have been presented in the foregoing chapters relative to the history and progress of the yellow fever in this city, as well as to others, of similar import, that have been observed elsewhere, taken in connection with the non-contagiousness of the disease, prove that in most instances the latter could not have been derived from abroad; and that in other instances, of a less conclusive character, the importation has not been satisfactorily made out, and, from analogy, may justly be regarded as not having taken place. It is not to be denied, however, that in this, as in other localities within the yellow fever zone, facts have occasionally occurred, which, if correctly reported and interpreted, would be somewhat difficult to explain on the principles I have throughout advocated, and would be calculated to create doubts in the minds of many, even among those who disbelieve the contagiousness of the disease, as to the correctness of the doctrine of domestic origin, or its applicability to all cases, as well as to lead some to attribute not only a few, but most epidemics, especially those occurring in temperate climates, to importation from the legitimate birthplace of the disease—tropical regions; and, consequently, to regard it as susceptible of transmission from one place to another, more or less contiguous, within or beyond those regions.

To reconcile such facts with the known non-transportability of ordinary non-contagious malarial poisons, and with the denial of personal contagion, or with the rapid spread of the disease in cases where it could not be traced to personal communication with the sick, several theories have been suggested. According to one set of physicians, the poison of the yellow fever—which some regard as a miasm produced by materials having no connection with the living body, and others as a contagious virus evolved from the sick—is carried from place to place, in ships, merchandise, clothes, about the persons of travellers, &c., and, as the contagionists aver, in the system of the sick; and, when so imported, is multiplied and extended epidemically by assimilating to its own nature something it meets with there. In other words, the peculiar poisonous effluvium, which, if imported into a perfectly clean locality, would occasion no evil effect, acts very differently when brought, during hot weather, into a place replete with materials from which issue morbid exhalations; for it then plays on those materials the part of

a ferment, and through their agency is enabled to reproduce itself, or out of them to give origin to a substance of the same nature, and endowed with identical properties, as yeast is generated during the vinous fermentation which yeast has set in motion.

Such is the belief, I repeat, of a respectable body of physicians—contagionists and non-contagionists—who differ, however, in this, that when the disease is thus introduced, the former believe it is communicated from the sick to the well; while the others view it as transmissible only through the agency of the contaminated atmosphere. If we admit this, we have certainly an easy explanation of the difficulties by which the question of the origin and dissemination of the disease among us and elsewhere is not unfrequently beset, and we need no longer trouble ourselves about seeking for proofs of the domestic origin of the yellow fever. We must cease to deny the importation of the latter on the ground that it is void of contagious properties. The transportation of this ferment, from place to place, can present no greater difficulty than that of any other ferment. The means by which it can be effected may generally be discovered, and have, indeed, been invariably pointed out; the quantity of it required is small; and as, when it finds the proper materials to act upon, and a suitable temperature to insure the success of its nefarious agency, it spreads widely, and may, though originally of trifling amount, poison the greater part or even the entire extent of a city, it follows that every time the disease breaks out in this country, or elsewhere, we may justifiably lay it to the score of importation, not only when its local origin cannot be made out in a perfectly satisfactory manner, but in instances when there can be no doubt that local causes capable of producing the effect could without much effort be pointed out.

Plausible as the theory may seem, and much as it may assist in explaining facts which otherwise might appear to some as of an intricate character, unless by reverting to the doctrine of direct importation, it is open to strong objections. It would be so even were the ferment viewed in the light of a contagious fomity, the product of a diseased body; for it is difficult to understand how it could in that case assimilate to itself the products of the materials it meets with elsewhere; since these products issue from materials of a totally different kind, which have no affinity with the other, and could scarcely be converted into anything like it. The effluvium thus generated would be a compound of a contagious virus, the product of an organic operation going on in a *living* body, and of exhalations resulting from the decomposition of dead organic matter. It would, itself, when thus formed, become endowed with the power of occasioning a disease, also possessing contagious properties, transmissible from one individual to another, and similar to the one produced by the ferment alone, and without the assistance of the infectious exhalations in question. All this appears incomprehensible, and must be passed by without further comment.

The views of those who, while disbelieving the contagiousness of yellow fever, or admitting that in any epidemic described by them the disease did not manifest properties of the kind, contend nevertheless for the mode of intro-

duction referred to, are not more satisfactory. It must be remembered that the correctness of the facts adduced, as imparting an appearance of probability to the doctrine of importation, and lending support to that of infectious fermentation, is of more than problematical character; that we can scarcely point out a single instance of the kind that may not be explained in some other way; that many of those that were set forth with the greatest confidence by the new-fangled importationists have been completely refuted on the most satisfactory grounds, and that others, if not yet refuted, do not appear likely to share a different fate as soon as the task is taken in hand by some competent inquirer. It must be remembered, too, that in no well-authenticated instance has the disease been communicated by clothing or merchandise; that even in those instances in which such an origin appears entitled to most credence, the disease did not extend, and ceased with the death or recovery of the individual who had introduced it, or who may have sickened after exposure to the objects through means of which the matter of infection was introduced—whether the hold of a vessel or steamboat—the effects of the passengers—merchandise, or trunks of clothes—supposing the occurrence possible; and that, consequently, the most that could be made out of them is, that such articles had been impregnated with infectious effluvia which were dissipated as soon as exposed to the air. It must be borne in mind, in addition, that in every instance in which the disease has been traced to a ship, its cause was local in the latter, and, as already stated, did not spread its morbid influence beyond a short distance, and that the influence ceased to be felt after its removal or the dissipation of the effluvia it had left behind. At the same time, it must be remembered that hundreds of facts occurring in places where the importation is supposed to have been effected—or in others, more or less near—establish conclusively the local origin of the disease; and that if, in a few instances, there is wanting some link in the chain of evidence respecting such an origin, we must, instead of concluding in favour of the exotic origin of the poison, base our opinions concerning those obscure instances on analogy with those in which the evidence is more complete. But let this be as it may, in some instances the disease has broken out among us, and elsewhere, under circumstances when no ferment could have been introduced. No doubt, had vessels from the West Indies, or with offensive bilge-water, reached our wharves at the propitious season, they would have been regarded as the means of conveyance. But there was no clew to the supposition, and the disease was then, as it must now be admitted to have been, the offspring of domestic causes. If such was the case then, why should we suppose that in other seasons, when vessels *have* arrived from the West Indies, Africa, &c., they have carried an infectious ferment, and that the disease which ensued was the product of the chemical action of this, on the materials it met with after it reached its new destination? There is no more necessity for such a belief at one period than at another.

The idea of this assimilative fermentation, like the phenomena of perpetuation it is adduced to explain, can be viewed in no other light than that of

a mere hypothesis, unsustained by a single well-accredited fact or plausible analogy. The property in question is not an attribute of any known variety of non-contagious febrile poisons.

If the exotic ferment reproduces itself in the way mentioned, there must be, in the places where it is introduced, materials of the kind necessary to bring about the elaboration of the particular poison giving rise to the yellow fever. Were it otherwise, it could not be multiplied by an assimilating fermentation. Its originating in tropical and southern regions, evidently indicates, too, that it requires the concurrence of high and long-continued heat, and sources of infectious decomposition. If such materials and such heats produce the disease and its generating ferment there, or if they do so in the hold of a ship navigating in hot climates, there is no reason why they may not generate it in temperate regions, and even in the cities and towns of our Middle States, provided circumstances of analogous character present themselves.

Now, when taking into view the admission of the existence in this city in 1853, and in all places, indeed, where the yellow fever is supposed to have been introduced, of the materials to be acted upon by the imported ferment, we inquire into the condition of the other influencing agent—high atmospheric heat—it is discovered that the circumstances under which the fever exists here and in other parts of temperate regions, and prevailed in this city during the summer mentioned, correspond exactly with those under the influence of which it appears within the tropics; for on turning to the history of the epidemics of the disease that have prevailed here and in other cities of the United States and Europe, the reader will perceive that the yellow fever requires for its development a certain range of atmospherical temperature—never appearing in seasons not characterized, during some length of time, by a heat equal to that existing within the tropics; while, on the other hand, in the latter, the materials of infectious exhalation are analogous to those which in temperate regions are supposed requisite for furnishing the matter to be acted upon by the ferment. In fact, whatever may be the local peculiarities and atmospheric conditions of places where the disease is supposed or admitted to originate, the same are found to exist in localities where it is thought to have been transmitted, and to have taken root. From this the conclusion is natural, that as the elements which enter into the composition of the poison exist in this and other cities and towns of middle latitudes where the fever prevails; as the fever cannot break out and spread there unless such materials exist, and as in these places, to use the words of an American writer who has investigated the subject with great care, the summer and autumnal seasons are frequently characterized by meteorological phenomena, which render them, in effect tropical, the supposition of an exotic poison reproducing itself by an assimilative fermentation is totally unnecessary to explain the origin of the yellow fever epidemics. (*Smith*, p. 84.) The more unnecessary, indeed, because the fever, as we have seen, has occurred without the possibility of such an importation—a result, the truth of which is fully admitted by some of the warmest advocates of the doctrine in question.

It has been further objected, and with much reason, that it would be impossible, on the supposition of the poison operating as a spark thrown among highly combustible materials, and reproducing itself by assimilating existing materials to its own nature, to account for the slow and progressive manner in which localities usually become pestilential. (*Smith*, p. 85.) If the theory in question were correct, the air of a whole city, so far from becoming gradually infected, would very soon assume a pestiferous character; for the poison being scattered by the wind, would quickly reproduce itself alike in every quarter. The laws which govern the changes occurring in fermenting mixtures will not hold in the present case; for in all instances with which we are as yet acquainted, the fermenting materials exist in a palpable form; whereas, in the present, both the ferment supposed to act, and the effluvia upon which it is to act, are placed beyond the reach of observation.

Besides, there are no facts to justify the supposition that specific germs or aeriform poisons assimilate others to their own nature. If it be said that the poison of yellow fever is absorbed by the soil, and by the adventitious materials by which the latter is covered; that when thus combined with corrupting materials, it induces therein a peculiar fermentation, by which it is reproduced, and is subsequently exhaled in the same manner as if it were primarily of domestic origin, the answer is easily found. The supposition is purely gratuitous; and, being neither supported by facts nor warranted by analogy, must make room—especially when we remember those instances where the importation of a ferment was in every way inadmissible—for the theory which ascribes the yellow fever to a poison generated in the localities where it breaks out, and whence it spreads epidemically.

The theory, or rather hypothesis, proceeds on several, indeed on nothing but, assumptions. It assumes that contagion is a ferment; that the gas, or exhalation, or whatever it may be, that contaminates the atmosphere in an infected place, is susceptible of fermentation; and that the product is the same as the ferment which excites it. So far as true contagions are concerned, it may be asked if it be proven that they are ferments. Does the smallpox, or scarlet fever, or rubeolic poison act in that way? If they are ferments, their effects are produced on the living body, not on surrounding materials. They always require the presence, at a greater or shorter distance, of an individual labouring under the disease. Again, as no one has as yet been able to seize and analyze the particular something which contaminates the atmosphere, and acts as the efficient or remote cause of fever, it is impossible to ascertain whether or not it is susceptible of fermentation; and to say that that same cause, when introduced in any place, reproduces its like, by a chemical operation, on surrounding materials, is only to say what no one can prove.

The ferment thus contended for, if it really exists, appears to require only certain kinds of impurities, to be enabled to produce the mischief in question. It produces no effect when introduced in an atmosphere which gives rise to autumnal fever. Here there is no yellow fever fermentation—no transformation of the kind mentioned. The disease does not exist beyond the limits of

the vehicle by which it has been introduced. Let a vessel with the yellow fever on board, or with her hold brimful of the West India atmosphere, stop near the swampy shores of the Delaware or Chesapeake, at the Balize, or near a thousand places that might be cited; let her land passengers and merchandise, and no ill effect will result—no fermentation and transformation will ensue. Individuals who go on board will possibly take the fever, but those who leave will communicate no fermentative leaven.

It appears, then, that the power attributed to the yellow fever ferment is only exerted on the impure air of certain localities. Let this impurity be wanting, and, however extensively the materials producing common fevers may exist, we shall have no yellow fever. This requisite impurity exists only in cities or towns, principally commercial ones. Now, it has been asked whether the malaria of these is inert and harmless of itself, and whether, in the absence of the ferment in question, it produces no morbid effects. Need I say that the answer is, *by no means*? Many localities where yellow fever prevails occasionally, or often—sometimes during the very time when the disease is in existence—are visited more or less extensively by remittent fevers, so analogous to those we daily see in the outskirts of our cities, or in rural districts, that every one is forced to admit their identity with the latter, and hence to view them as the product of the same cause. In other localities, pure remittent fevers rarely show themselves, but they do so occasionally in nearly all; and in many more instances cases occur, which, if not pure, are nevertheless so akin to the former, that they cannot but be looked upon as belonging to the same family of diseases.

Dr. Drake, from whom I borrow this objection, remarks that, in the country, this agent is generated in swamps and other humid places, where the recrements of plants and insects are subjected to decomposition; in the towns, where yellow fever oftenest prevails, other vegetable recrements, animal exuviae, and exhalations and secretions from a dense population are subjected to the same decomposition. As the decomposable materials are not identical, the products of the fermentation cannot be identical. The rural malaria produces one modification of fever, the civic another. Now, is it not remarkable, when these malaria approach so near to perfect identity, that the imported ferment should readily change the civic malaria into yellow fever gas, but produce no effect whatever on the rural malaria? When the two, judging by their effects, so far from being distinct species, are not even striking varieties, how is it that the foul air of an infected port or ship, or the exhalations from the body of a yellow fever patient, can start one into active and deleterious fermentation, and be utterly inert when mingled with the other?¹

Let it not be said, in support of the belief, that cases occur in which vessels or steamboats have deposited, during the few days or hours they remained at a wharf, a poisonous agent of some sort, which continued to thrive and spread its baneful effects after the departure of the instrument of its introduction; and that the latter, after making the fatal deposit, and being removed from the spot it had helped to contaminate, ceased to be itself a focus of in-

¹ Drake, ii. 292.

fection. The occurrence, in so far as regards the yellow fever, does not appear to be probable, and has never been fully established; and would, if true, be in direct contradiction to the result of former experience here, and in many places where the disease has often been observed. Were the yellow fever contagious—had it, in the instance in question, manifested properties of the kind, its continuance after the departure of the vessel which brought it could readily be understood. The event has often been noticed, and can admit of no doubt, so far as concerns contagious complaints; but the idea of its occurring in relation to diseases of a non-contagious character is not founded on fact or analogies. It is, at best, a mere hypothesis. We know of no febrile or other poison producing a disease of that character which is capable of giving rise to effects such as are here ascribed to the efficient cause of the yellow fever. We know of no instance in regard to any etiological phenomena whatsoever to which we may appeal as affording a plausible analogy.

The yellow fever, though far from being identical with, or a mere aggravated form of, ordinary malarial fevers, is nevertheless of the same family of diseases. Arising, as we have seen, from local sources of infection, it is like them a malarial fever also; and the poison by which it is produced, however varying from those which give rise to the several forms of the above-mentioned fevers, can scarcely be supposed to possess, when carried anywhere in ships or otherwise, properties very different from those with which the others are endowed. Yet nothing of the sort has been observed in regard to these. The poison of the African remittent fever, of the Chagres fever, of the remittent bilious fever of other parts of hot climates or of our own latitudes, has never been known or even supposed to produce the self-perpetuating phenomena alluded to. The same is found to be the case in relation to all kindred poisons; and until an exception to the rule can be satisfactorily made out, we are justified in doubting that matters take a different course in regard to the yellow fever. We must, I repeat, view the idea under consideration in the light of a mere hypothesis, an assertion to be proved. Besides, it multiplies causes without necessity. To insure the production of the fermentation in question, a peculiar impure state of the atmosphere is necessary, and that impurity can only be found in certain localities. Now, the fever occurs often under circumstances which, to say the least, render it probable that it arose solely from the deleterious action of those very conditions of localities above referred to, without the addition of any extraneous agent. If such is the case in those instances, there can be no necessity to have recourse to the supposition of the introduction of that agent, the existence of which cannot in any case be proved, to account for the production of the disease in others.

By the larger number of those who advocate the hypothesis under consideration, nothing is said of the manner in which the assimilation contended for is effected, or of the nature of the poison which acts as the ferment, or of that of the compounds which, being formed by the process, may thus be carried from place to place. Having assumed, as an incontestable fact, that the disease in the instances they describe was not of a strictly local origin, but due to a cause, one of the elements of which was derived from abroad, they

wisely abstain from all explanation as regards the nature of that element, or the manner in which it operates in the generation of the febrile poison, and leave the reader to draw his own conclusions in the premises. Others, more venturous than the preceding, fancy they can solve the difficulty, and confidently affirm that the travelling morbidic fermentative agent, as well as the poison which results from its operation, and gives rise, when formed, to the yellow fever, consists, like those of most other, if not all, zymotic diseases, in living organized microscopic beings. By some, these beings are referred to the animal, by others, to the vegetable kingdom. By all, they are regarded as produced out of pre-existing germs, under peculiar and favourable circumstances of locality and atmosphere, and as capable of propagating and multiplying to an indefinite extent whenever and wherever those circumstances are found to exist, and of giving rise, by their deleterious action on the system, to particular morbidic effects.

Whether the organic germs which constitute the matter of this travelling, transportable, and contagious poison be animalcular or fungous, the mode of their prompt, active, and wide propagation and extension offers no difficulty to the advocates of the theory. They do not entertain the idea that those germs assimilate to their own nature something they meet with in the atmosphere of certain localities. In this field of interesting inquiry and hypothesis the suggestion is offered, as at least a reasonable conjecture, that in the animal exhalations, infinitely abundant and varied, that are collected about the dense population of a crowded city, these organic germs may find occasionally all the elements essential to their germination and growth, and may propagate and multiply themselves in an atmosphere thus saturated with the pabulum adapted to their support and development. "A bulb or a seed," says Dr. Dickson, "which vegetates in the soil ordinarily, may, as we see every day, grow without soil, if laid upon a surface which conveys moisture to its roots, as in the flourishing of rice laid upon damp cotton, and the budding of a hyacinth in a glass bulb filled with water. Nay, the air-plants, properly so called, furnish us with a still closer analogy; and perhaps we may find another in the history of those minute insects which will produce several generations successively without access to their special food or habitation, as the aphids, and the common tick of our southern country." Thus, then, whether we regard the *materies morbi* of epidemic contagions as of animal or vegetable character, there is no difficulty in comprehending their propagation and extension under the circumstances mentioned, "favoured generally, as we notice, by heat, moisture, and atmospheric stillness, and repressed more or less by the opposite conditions of dryness, heat, and ventilation, by winds or dilution of air."¹

The animalcular hypothesis is of old date, and enumerates to the present time warm partisans, so far as it is applicable to the etiology of pestilential or malignant fevers generally, and of the one under present consideration among them; and in latter years has been extended to the explanation of the production and diffusion of the Asiatic cholera and other spreading diseases.

¹ Dickson, pp. 60, 61, new edit., 1855.

In reference to these several diseases, Bradley,¹ Pleniz,² Gattoni,³ Crawford,⁴ Mojon,⁵ S. Brown,⁶ Holland,⁷ Drake,⁸ Wood,⁹ Grogner,¹⁰ Nott,¹¹ Grassi,¹² Rasori,¹³ Milroy,¹⁴ and others,¹⁵ repeat much that was said in former days by Varro, Lucretius, Columella, Vitruvius, Kircher, Valisnieri, Lancisi, Linnæus, Nyander, Ricia, Hartsæker, and Moufflet.

The second hypothesis—that which ascribes febrile epidemics to the toxicological efforts of microscopic fungi, is of as ancient origin as the former—probably of greater antiquity—and has received the sanction of high medical authorities from the time of the revival of letters to the present day. Hood, in his work on pestilence (p. 91), has called attention to the fact that a rubigo, or mildew—a dew impregnated with highly corrosive powers—was anciently deemed one of the causes of epidemic diseases. Blasting and mildew are noted in the sacred writings as though there existed a physical relation between them and the plague; and the establishment, by the Romans, of the *festa rubigalia*, and the worshipping of an imaginary god under the name of Robigo to avert calamity on grain, and indirectly on the human species and the animal creation generally, implies a knowledge on their part of the pernicious effects of these fungi.¹⁶ In more modern times, epizootics have been referred to that cause;¹⁷ while Forestus, Schriber, and Diemerbroeck, the describers of the mortal plague of 1252, of the black plague of the fifteenth century, of Dantzic in 1709, of Oczacoff in 1739, and Hoffman, in his account of the occurrences of 1693 and 1694, mention the extensive development of such phenomena, attributing to them greater or less agency in the production of the evil; and Agricola and others regarded them as indicative of extensive disease.¹⁸ In our own days, Hecker (p. 205), is not far from conceding to

¹ The Plague of Marseilles considered, pp. 20, 21. 1721.

² Op. Med. Phys. 1762.

³ Mém. de la Soc. de Méd., x. 104.

⁴ Remarks on Quarantines, Baltimore Observer, April and August, 1807; Med. Reposit., xi. 86, 87.

⁵ Interno alla natura del Miasmo Choleroso Asiatico, Lucca, 1832. A French translation of this clever essay, by Julia de Fontanelle, was published at Paris in 1832. See review of it in the N. A. Med. and Surg. Journ.

⁶ Dissertation on Bil. Malign. Fever, Boston, 1797, p. 9.

⁷ On the Hypothesis of Insect Life, in Med. Notes, chap. xxxiv., Am. ed.

⁸ A Practical Treatise, &c., on Epid. Cholera, pp. 34, 44; Ib., Topogr. and Dis. of the Valley of the Mississippi, i. 723.

⁹ Practice of Medicine, i. 147, 306.

¹⁰ Archives de Statistique du Dept. du Rhône.

¹¹ Henle, Pathol. Contrib., Berlin, 1840; see Brit. and For. Med.-Chir. Rev., ix. 398; New Orleans Med. and Surg. Journ., iv. 563.

¹² Rapport à l'Acad. Roy. de Méd. sur la Peste (Pièces et Doc.), p. 418.

¹³ Ibid.

¹⁴ Quarantine and the Plague, &c., p. 11.

¹⁵ Am. Quart. Review, iv. 301, &c.

¹⁶ Pliny, Bk. 18, chapt. 29; Varro, De Re Rustica, lib. i. cap. 12; Ovid Fast, Bk. 4, V. 907.

¹⁷ Leger, quoted by Mitchell, p. 38; Reinesius, p. 218; Ramazzini, Const. Epid. Martinensis, Anno 1690, 1692, pp. 3, 4; Paulet, Rech. Hist. and Phys. sur les Malad. Epid., p. 443.

¹⁸ Hecker, on Epid. of Middle Ages, p. 206.

them an effect in the development of epidemics generally; and the fungoid origin of cholera Asiaticus has been insisted upon by Henle (*For. Quart.*, ii. 97) and other German writers; and is presented with much confidence and talents by Dr. Cowdell, of England, and others.¹ Its application to the yellow fever, and other complaints of paludal origin, has been spoken of in terms of commendation by Dr. Drake (p. 723), of Cincinnati, Dr. Wood (i. 306), of this city; and in a clever publication by Dr. J. K. Mitchell, also of this city, it is adopted to the fullest extent, and elucidated with characteristic ability and ingenuity.²

On examination it will be found that both these hypotheses are supported by facts and arguments which are entitled to our consideration. But, however true this may be—however ingenious and plausible those theories may appear—the fungoid particularly—however much they may assist us in the explanation of some of the supposed anomalies to which attention has been called—as also many other circumstances connected with the origin and progress of the yellow and other malarial diseases, further proofs, as has been remarked by those not disposed to reject them, are required, before either can be regarded as entitled to the entire approbation of the etiologist. In the present state of our knowledge, they must be looked upon as purely hypothetical, and as such could not deserve the time devoted to their development, even were they less open to objection than, on examination, they are found to be. It might be shown, that the anomalies so much insisted upon are as readily explained by the admission of other hypotheses not a whit less plausible than those in question. It should be remembered, too, that some of the facts, for the explanation of which it is thought necessary to have recourse to the morbid agency of animalcules or fungi, are of a questionable character; that the authenticity of some of them has been assailed on the strongest possible grounds by authorities as high as those by whom they are adduced; that the authenticity of the reports on which others have been admitted has never been satisfactorily made out, that many rest on hearsay evidence or popular rumors, and that they are all opposed by many more leading to very opposite conclusions.

Modes of Action and Introduction of the Poison.—Enough—perhaps too much—has been said on the nature of the efficient cause of the yellow fever. It remains now to add a few words on its mode of action upon the system, and the medium of its introduction therein. It is a well-known fact, upon which it is not necessary to enlarge, that poisons operate on the living system in three ways only. They attack and chemically change or corrode; or locally irritate and inflame the tissues upon which they are applied. Examples of the first variety of this mode of action may be pointed out in the effects of concentrated mineral acids, or alkalies, lunar caustic, corrosive sublimate, arsenic, &c.; and of the second, the effects of alcohol, cantharides, tartar emetics, di-

¹ A Disquisition on Pestilential Cholera, &c., by Ch. Cowdell. London, 1848.

² On the Cryptogamous Origin of Malarious and Epidemic Fevers, by John K. Mitchell, M. D. Phila., 1849.

luted acids, &c., are familiar to every one. Poisons, again, may produce their deleterious effects by acting—with or without the admixture of the corrosive or stimulating agency mentioned—directly on the nervous expansions of the tissues, and indirectly through the medium of these, with or without subsequent reflection on other organs, on the central organs of the nervous system, as we find to be the case with hydrocyanic acid, opium, strychnia, conia, alcohol, &c.; and, finally, they operate by being absorbed into the circulation, and thereby contaminating the blood, and affecting some particular organ or the system at large.

Whatever may be the mode of action of these deleterious substances, there is no reason to admit that the poison of the yellow fever produces its effects in the first of these several ways. Be its nature what it may, it possesses no corrosive—and, so far as yet found—no stimulating agency capable of enabling it to bring about chemical changes, or inflammation in the tissues to which it is applied; and though the disease be often characterized by derangements of those parts, such derangements, so far from being necessarily present, are frequently absent, and when present, make their appearance at too late a period to be ascribed to such a cause. Besides this, they differ, as do also in great measure the constitutional disturbance consequent upon their operation, from the phenomena observed in the disease before us. In a word, poisons of the kind mentioned, unless when absorbed into the system, produce local inflammation, often followed by symptomatic fever, but cannot give rise to an idiopathic fever of a specific character, which, like every other disease of analogous kind, sets in in a way different from that noticed in cases of poisoning from acrid or corrosive solutions, and consists necessarily in something differing greatly from mere local irritation and constitutional disturbance, or common symptomatic fever.

Less objectionable, doubtless, in its application to the mode of action of the poison of yellow fever, is the opinion of those who would ascribe its effects to a morbid impression on the nervous expansion of the tissues and to a transmission of that impression to the nervous centres, and thence by a reflex process to the organs mainly implicated in the disease; in other words, who view its operation as of a sympathetic kind. It is very natural to suppose that this, like other poisons of the same class, produces a local and sympathetic action of the sort mentioned, and that some of the symptoms of the disease—especially those of the early stage, and in cases which run their course rapidly—may be explained on that principle. All poisons, in fact, must produce an impression on the sentient extremities of the nerves, and a sympathetic action on other organs. But that the poison of the yellow fever, and the poisons of other kindred diseases do not act exclusively or even mainly in this way, we have every reason to believe.

Were its effects of so circumscribed a kind, there would be some difficulty in accounting for the *specificity* of the action it gives rise to in both solids and fluids. From this, as well as from a consideration of the main and characteristic phenomena, and of the pathology of the disease; from the occurrence of toxical effects under circumstances when the agency of the

nerves could be of no avail in their explanation, and was, in fact, completely prevented; from the ascertained effects growing out of the introduction of putrid and other substances into the circulation, or of the absorption of these when placed in contact with absorbing surfaces; from the extreme rapidity with which absorption and the sanguine currents take place—a rapidity sufficient to account for the effects of all toxical agencies; from a consideration, I say, of these several circumstances and facts, the correctness of which has been fully substantiated by the experiments of Magendie,¹ Delile,² Emmert,³ Orfila,⁴ Munroe,⁵ Coindet,⁶ Christison,⁷ Vernière,⁸ Coates and Lawrence,⁹ Blake,¹⁰ and Herring,¹¹ we are led to the conclusion that the poisons of the yellow fever, and of other diseases of analogous kind, act principally, and produce their specific constitutional effects, through the medium of the blood into which they are absorbed.

Such being the case, it becomes necessary to inquire into the manner in which the poison penetrates into the blood. But on this, as on many other subjects of the kind, much obscurity prevails, and we are, in great measure, compelled to content ourselves with conjectures and base our decision on analogies. One thing, however, is very certain—that the poison can only find admission through one or more of three absorbing surfaces—the cutaneous, the gastro-intestinal, and the pulmonary. Each of these channels has had, and continues to have, its partisans.

Lancisi¹² admitted the three sources of introduction. At a later period, Quesnay also admitted the probability of the poison gaining admittance through the several inlets mentioned; but endeavoured to show, that of these the pulmonary was the feeblest—indeed, that it was nearly null.¹³ This, indeed, was the commonly received opinion at the time, and even now it enumerates many advocates. But a further examination of the subject, and the results of experiment on these points, as well as the observation of facts more or less closely allied to the one before us, lead to an opposite conclusion. That the cutaneous envelop of the body is to a certain extent permeable to gases, is well known; for nitrogen, oxygen, and other substances of kindred character have been found to enter the circulation through that medium. If such is the case with these gases, it is not impossible that the poison of the yellow fever, floating as it does in the atmosphere, and coming continually in contact with the skin, may be introduced in the same way.

By those who advocate this mode of introduction it is remarked, also, that

¹ *Journal de Physiologie*, i. 26.

² Orfila, *Tox. Gén.*, ii. 1, 2d pt.

³ *Philos. Trans.*, 1811, p. 138.

⁴ *Bulletin de l'Acad. de Méd.*, iii. 426.

⁵ *Edin. Phys. and Lit. Essays*, iii. 334.

⁶ *Edin. Med. and Surg. Journ.*, xix. 163.

⁷ *Ibid.*

⁸ *Journ. des Prog. des Sc. Méd.*, iii. 121.

⁹ *Philad. Med. and Phys. Journ.*, iii. 273.

¹⁰ *Edin. Med. and Surg. Journ.*, liii. 35, 45; *ib.*, lvi. 412.

¹¹ Quoted by Christison, p. 15.

¹² *De Noxis Palul. Affluv.*, pp. 42, 43; *Mem. of Acad. of Surg.*, i. 4.

¹³ *Mémoire sur les Vices des Humeurs*, *Mém. de l'Acad. Roy. de Chir.* (8^o ed.), i. 30–34.

exposure of the body to night air in infected districts is frequently, if not usually followed by an attack; and that among the early symptoms of the disease, we notice an impaired condition of the secretory process and of calorification. But experience has abundantly shown that cutaneous absorption, so long as the epidermis remains entire, is with difficulty aroused, and at all times extremely feeble—too much so, indeed, to allow us to attribute to it the principal, or even an important agency in the admission of the poison in question; while in further answer to the arguments adduced in support of that agency, it may be urged that, though doubtless true that during exposure to night air the poison comes in contact with the skin, it is not less certain that it also comes in contact with other surfaces, the power of absorption of which is much more active, and more likely therefore to afford entrance into the circulation of any floating in the atmosphere; and that other functions are deranged as early and extensively as the cutaneous secretion and calorification.¹

As regards the admission of morbid poisons of the kind through the channel of the gastro-intestinal surface, little need be said. Admitted by Lancisi (p. 423), the opinion was advocated strongly by Quesnay (p. 4), as regards putrid and pestilential air generally, and has found, so far as relates to the poison of yellow fever, a warm advocate in Audouard (p. 218), who also thinks that poisons may be absorbed by the lining membrane of the nose, mouth, fauces, and pharynx. This opinion derives support from the known absorbing power of those surfaces—from the early derangement which they and their annexed sympathizing organs undergo, and, in most cases, from the subsequent development of positive disease in the stomach and bowels. But while recognizing the power of absorption,² as well as the occurrence of the morbid phenomena alluded to, it is impossible to close our eyes to the objections which may be urged against this opinion. The poison, in order to be absorbed by the gastro-intestinal surface, must reach the latter with the food, or drink, or saliva. Now, it is plain that in either way the quantity of it that could be applied to the surface must necessarily be very small—too small, apparently, to produce injurious effects either on the membrane itself, or, through the blood, on other parts of the system. Independently of this it may be urged that gastro-enteritic derangements have not always the precedence over all others—that instances are not rare in which no positive disease of that surface can be detected—that those that do occur may be traced to a cause independent of the direct application of the poison to the part affected; and that even admitting the necessity of such an application, it may be obtained through the medium of the circulation as readily as by the direct impression of the poison.³

But if from the cutaneous and gastro-enteritic surface, as channels of introduction of the poison, we turn to the pulmonary, we shall find it open to much less difficulty. The lungs present a vast extent of surface. At each

¹ Rochoux, p. 134; Drake, i. 728; Harrison, p. 145.

² See Christison, p. 698.

³ Drake, i. 729; Rochoux, p. 134; Harrison, N. O. J., ii. 145; Cartwright, Med. Recorder, x. 237.

act of inspiration, a large amount of atmospheric air is placed immediately in direct contact with the vascular parietes of the bronchial cells¹ by which it is more or less compressed. Absorption and exhalation are constantly going on through the cells. It is not alone the oxygen of the air which is taken in through their parietes and carried in the blood; experience shows that many other fluids contained in solution in the atmosphere find admittance there also. It is sufficient to breathe, during a few moments, atmospheric air charged with terebinthinate exhalations, to cause the urine to exhibit the odour of violet. The mere sleeping in a newly painted room produces the same effect. The experiments of Nysten, and since him of Edwards, have shown that oxygen, hydrogen, azote, aqueous vapour, and other gases, are promptly absorbed by the lungs.² And assuredly what takes place in relation to such gases may readily be admitted to follow in relation to the miasmal poison of the yellow fever, mixed, as it evidently must be, with the atmospheric air, and introduced with it into the air cells of the lungs. Indeed, the reality of this absorption is in great measure confirmed by the fact of Magendie having produced diseases in animals by forcing them to breathe air tainted by putrid exhalations, as well as by the asphyxia produced by the inhalation of various deleterious gases.³ Let it not be said in opposition, that the function of respiration is seldom much impaired in this disease, particularly in the early stages, except in so far as may be explained by the existence of arterial excitement at first, and vitiated blood subsequently; and that we seldom discover during life, or after death, evidence of acute inflammation; all of which would not occur were the poison to find admittance through the channel of the pulmonary tissue. Experience has amply proved that morbid agents do not necessarily produce disease in, or materially disturb the functions of, the parts on which they primarily act; that gases and toxical substances absorbed through the lungs produce, as do those absorbed through other surfaces, effects on distant organs by virtue of a power of selection which each possesses—whether by being carried thither in the blood, or by acting, as Addison and Morgan believe (p. 60), on the sentient structure of the bloodvessels, matters not here—without disturbing the pulmonary tissue; and that the same result may be readily admitted to occur from the absorption of the yellow fever poison, which, whatever be its nature, is not calculated to produce inflammation by its direct application to the tissues.

¹ Journal de Physiol. Expérimentale, i. 78.

² Journal de Physiol. Jan. 1823, pp. 19, &c.; Edwards, pp. 429, &c.

³ Harrison, p. 145; Rochoux, pp. 133-4; Cartwright, x. 237; Williams, ii. 464; Drake, i. 730-1; Monette, Western Journal, iv. 342; Levacher; Anglada, Traité de la Contagion, i. 251; Bertulus, de l'Intoxication Paludéenne, p. 17.

CHAPTER XXI.

INFECTION.

IN the preceding chapters I have endeavoured to show that the yellow fever of this city has never manifested contagious properties ; that, in the majority of instances of its occurrence epidemically, it has undoubtedly originated independently of importation ; and that if, in a few instances, owing to certain complicating circumstances, this home origin cannot be proved beyond the possibility of doubt, on the other hand, the reality of the exotic birth of the disease is far from having been clearly made out, and may, therefore, in view of the preceding, be justly regarded as open to well-founded suspicion. I have also endeavoured to show, by an appeal to well-authenticated facts, that the conclusion drawn in those respects from the history of the fever as it has appeared among us applies with equal force to the disease elsewhere, whether in other cities of this country, in Europe, or in tropical regions. Such being the case, we must inevitably refer the origin and mode of extension of the fever to the operation of causes appertaining to the localities where it occurs, or generated there temporarily through the effects of external agencies, which themselves are due to the morbid influences of surrounding circumstances.

What the origin of the cause thus generated is, becomes an object of interest to the medical inquirer, and is entitled to investigation. That it is not to be sought for in the operation on the system of excessive and long-continued heat alone, or of humidity, or of atmospheric vicissitudes or electrical changes, has been pointed out. It has been shown that neither separately nor combined can these produce the disease, and that, in accounting for the latter, recourse must be had to some other morbid power. They may all, and doubtless do, exercise a marked influence as exciting or predisposing, but not as efficient, causes of the disease ; while some of them—heat, humidity, electrical currents, &c.—are necessary instruments in the production of that cause, which seldom, if ever, manifests its existence, except during the prevalence of particular atmospheric and meteoric conditions.

I shall not inquire here whether those agencies may not produce, at times and under particular circumstances, certain forms of febrile complaints more or less allied to the disease before us. All that is necessary for our present purpose is, to know that such is not the case with regard to the yellow fever, and that the greater number of the others are due, not to the cause mentioned, but to different injurious agencies floating in the atmosphere. From the analogy existing between the yellow fever and other morbid states, the etiology of which is known ; from an analysis of the phenomena of the disease itself, and from an attentive survey of the laws which govern its origin, mode

of progression, and extinction, as well as of the circumstances by which its course is influenced, the conclusion has been reached that it is the result of the introduction into the system of a peculiar morbid poison, distinct, however, from those of a contagious character; and that this poison, whether animalcular, fungoid, or of any other nature, is the product of an elimination from materials located out of the body; not of a morbid secretion. In other words, we may conclude that the yellow fever must be classed among diseases and by infection, and hence placed in the same category with endemic remittent intermittent fevers, which, from the days of Lancisi and Baglivi to our own, have been recognized by the ablest etiologists of all nations as the products of causes of that kind. It would be a waste of time to dwell here on the proofs we possess relative to the malarial origin of these fevers. I have elsewhere performed the task. Others have doubtless done it better, and I need only in this place refer the reader to what has been said on the subject.

The Doctrine of Infection applicable to Yellow Fever.—The doctrine of infection, as applied to yellow fever, and the connection of the disease, on that score, with malarial fevers generally, is confirmed by the facts recorded in the foregoing chapters. 1. The disease has arisen here, and in other parts of this country, in Europe, and in tropical climates, as well as on shipboard, under circumstances which leave no doubt as to the reality of a local origin. The disease being the same everywhere, we may, from analogy, infer that, as it has arisen from such causes in some places and at some periods, it has been due to the same causes in other places and at other periods, though such an origin may not be as easily made out on these as on the former occasions.

2. It is, like other miasmatic diseases, under the influence of atmospheric vicissitudes, increases with the augmentation of heat or a particular hygrometrical state of the air, diminishes with the accession of cold weather, and is extinguished by frost.

3. It appears at those seasons of the year, and during or after the prevalence of certain atmospheric conditions, at and under the influence of which diseases arising from the operation of morbid poisons of the kind mentioned are produced and usually most rife, requiring, as it does, a certain amount of heat, certain hygrometrical conditions of the air, and particular states of the soil.

4. It is limited in the sphere of its prevalence, never extending beyond certain boundaries, attacking those who venture within those boundaries, even in the absence of the sick, and sparing those who keep aloof.

5. To these limits it may be restricted by a cessation of intercourse; while the disease is arrested in its progress by the dispersion of the inhabitants of the infected localities, and by preventing further access to these by artificial means.

6. It is in some instances arrested by the removal of the source of infection, or by destroying or neutralizing the poisonous effluvia through means of well-known hygienic processes, none of which effects could be produced were the disease due to causes of a different kind.

7. It observes the same laws of progression as other malarial diseases, commencing at one or more points, and while continuing to exercise its

ravages there so long as there are subjects susceptible to its morbid influence, and the weather is propitious to its development, extends in a regular manner to surrounding or adjacent parts.

8. It is worse, not only in regard to the extent of its prevalence, but also to the nature of its tendencies, in some parts than others—usually in the place of its original outbreak. It is also more malignant and fatal in its character at some periods—commonly the early period—of the same epidemic.

9. The danger of an attack in this, as in malarial diseases generally, is enhanced by exposure to the infected localities after night; while in individuals residing within those localities the morbid causes operate with more certainty and severity during the state of sleep.

10. It spares ships situated at a short distance from the shore, limiting its effects to such of the men as go ashore, particularly at night. On the other hand, vessels become the seat of the disease, without any one having landed from them, whenever they approach very near or are placed to leeward of an infected shore.

11. It is usually connected, in regard to its origin and extension, with peculiar conditions of localities favourable to the generation of malarial poisons, to some forms of which, fevers of kindred character are indubitably due, arising along the wharves and in the vicinity of other receptacles of filth or organic materials in a state of decomposition, and not unfrequently resulting from the upturning of the soil, inundations in certain spots, &c.

12. It attacks vessels at sea, far from shore, and under circumstances which leave no reason to doubt that such vessels are themselves sources of infection, containing, as they do, materials of decomposition, foul ballast or cargoes, impure and offensive bilge-water, filth, decayed timbers, green wood, shavings, &c., the deleterious effects of which have long been admitted.

13. Epidemics of yellow fever are usually, if not always, preceded or accompanied by an increase in, or aggravation of, other diseases depending on infection.

14. During the epidemic diffusion of the yellow fever, the presence of a morbid agent floating in the atmosphere of places infected, and their immediate vicinage, is made manifest by the number of persons affected, to a greater or less extent, within those limits, with premonitory symptoms or with some of the lesser phenomena of the disease.

15. The same general diffusion of a morbid cause within those boundaries is evinced by a preceding or concomitant affection of an analogous kind among some of the lower orders of animals.

16. The yellow fever is allied, by a number of its phenomena, to certain forms of malarial complaints; so closely, indeed, that it is sometimes difficult, unless to one familiar with the disease, to distinguish the one from the other, particularly in the early stage of an attack, and at the commencement of an epidemic.

Upon all these subjects I have already dwelt in detail, and need not, therefore, do more than point them out in this place. It may not be amiss to remark, however, that the dependence of the disease upon the causes con-

tended for would seem to be confirmed by the fact that its symptoms approximate to those produced by the ingestion or inhalation of various poisons of a vegetable or mineral kind, or by the introduction into the circulation of various substances calculated to deteriorate the blood, thus leading to the inference that it is itself, like malarial fevers generally, the effect of the absorption into the system of a virulent agent distinct from a specific contagious virus, and must take rank among diseases by simple infection.

In speaking of the black vomit and jaundice, I called attention to the fact that both these phenomena—especially the first—have been produced by the ingestion of certain poisons, mineral, vegetable, and animal. But, besides black vomit and jaundice, other phenomena are often found to attend on those occasions, as fever, pain, præcordial anxiety, burning heat in the stomach, red eyes, dry and furred tongue, restlessness, dark and fluid blood, &c. On this subject, the well-known and oft-mentioned experiments of Gaspard, Magendie,¹ Leuret, Hamont,² and others,³ to say nothing of the discovery by Lassaigne, in his analysis of putrid meat water, of a stinking volatile oil, the probable poisonous agent in the effects obtained by those experimentalists, and which can differ but little from the substance dissolved in the air of malarious localities, can leave no doubt.

The late Dr. Harrison, of New Orleans, who was perfectly competent to form a correct opinion on the subject, speaking of the effects obtained from injecting putrid meat water into the veins of animals, as shown in the experiments of Gaspard, already alluded to, says: “No one can, I think, fail to be struck with the extraordinary resemblance of those symptoms and post-mortem lesions to those of yellow fever. The characteristics of the disease, its rapid course, its hemorrhagic tendency, and its peculiar lesions, are all to be met with in these experiments. We have black vomit, bloody alvine discharges, redness of conjunctiva, extreme tenderness over the abdomen, great and rapid prostration of strength, burning thirst, anorexia, &c.—all so characteristic of yellow fever. In his other experiments, he speaks of other characteristic symptoms—suppression of urine, intussusception of the intestines, the existence of fetid fuliginous matter in the bowels, ecchymosis of the mucous membranes, congestion of the lungs, &c. In short, there is hardly any symptom mentioned by authors, as occurring in yellow fever, which may not be found in these experiments; and it is the same with regard to the post-mortem lesions.”⁴

¹ *Journal de Physiologie*, ii. 1, and iii. 81–85.

² *Journal des Progrès des Sci. Méd.*, vi. 181.

³ Fontana, *Traité de la Vipère*, i. 85; Celle, *Hygiène des Pays Chauds*, 89; Sauvages, *Nosologie*, iii. 112, 115; E. Miller, *Works*, pp. 52, 53; *Ibid. Med. Reposit.*, ii. 412; Waring, *Yel. Fev. of Savannah*, pp. 37, 38; John K. Mitchell, *Cryptogamic Origin of Fever*, p. 73; Hunter, *Dis. of the Army in Jamaica*, p. 156; Dewitt on *Stramonium*, *Med. Reposit.*, ii. 30; Ferguson, *Recollections*, pp. 204, 205; Caillot, *Fièvre Jaune*, p. 296; Levacher, p. 78; Chaussier, *Consultations de Méd. Légale*, p. 40; Rochoux, p. 79; Salva, *Segundo ano del Real Estudio*, &c., p. 142; Lafuente, *Observaciones Sobre la Fiebre Amarilla*, p. 201.

⁴ Boudin, pp. 128, 129; Griffin, *Lond. Med. Gaz.*

Let us also bear in mind the changes which take place in the system of those who recover from yellow or other malarial fevers, through the unaided efforts of nature; the coincidence of the sudden and successive disappearance of the symptoms with the appearance of phenomena of a critical character—dark discharges from the bowels, abundant sweats, and a copious flow of flocculent and sedimentous urine—so like what is known to attend the expulsion or elimination of some poisonous substance, it is impossible to avoid the conclusion that the above stated fevers, when they make their appearance in the localities mentioned, are the offspring not of a cause of a general character or of physical influences, consisting of some particular modification in the ordinary and sensible qualities—thermometrical, hygrometrical, electrical, &c.—of the atmosphere, since these operate in equal degree all around, without, however, producing everywhere the morbid effects in question, but of something exhaled from the soil, or from some of the various sources of decomposition adverted to. In other words, we must infer that fevers arise from the impress of a species of morbid material poison, which finds admission into the blood, and occasions peculiar changes in certain of the constituents of that fluid—conferring subsequent immunity in many cases, like other causes of a kindred nature, though in a less degree, and with less certainty; possessing, to some extent, other characteristic properties of true morbid poisons, but differing from them in not being the product of operations taking place in the living system; in its not being possessed of the power of reproducing or multiplying itself in the body; in its not converting any of the elements of the blood into its own similitude; and in its effect not being capable of propagation from one person to another.¹

Doctrine of Infection long admitted, here and elsewhere.—The doctrine of infection, in its application to the yellow fever, was early adopted in this city. It is true that, in the first epidemics, the disease was not attributed to home causes, both physicians and the public appearing to regard it as of exotic origin; for though, as we have seen, in the accounts of the fever of 1699, nothing is said of importation, neither do we find mention of its being due to local causes; and as, besides, it was then called the Barbadoes distemper, and writers who came here not many years after, and collected their information from the older inhabitants, referred it to importation, we may presume that such was the general opinion at the time. The subsequent epidemics of 1741, 1744, 1747, and 1762, were accounted for in the same way. But though the doctrine of contagion and importation was for a while in the ascendant, some leading men were found willing to admit that the efficient cause of the disease, sometimes at least, originated at home.

Dr. Thomas Bond, writing in 1766,² states that the second of the epidemics he witnessed, probably that of 1747, was of indigenous birth, that it arose from evident causes, and that it was principally confined to one square of the city. These causes, Dr. Shott, in the account of his travels through this

¹ See Simon's Lect. on General Pathology, p. 198, Am. ed.

² N. A. Med. and Surg. Journ., iv. 271.

country, says, Dr. Bond informed him, were a large quantity of putrid fish thrown into the dock.¹ Dr. Bond would appear from this to have been the first in this city to advocate the doctrine of infection. From 1752 to 1793, nothing further appears to have been said on the subject, which, from the fever not having appeared during that interval, had probably lost all interest in the minds of the profession and the public.

But on the occurrence of the memorable epidemic of the year last mentioned, the subject was revived, and soon gave rise to much discussion. By a large number of physicians the doctrine of importation was warmly advocated, and the College of Physicians, in its corporate capacity, exerted itself to the utmost to disprove the idea of local origin. Nevertheless, the latter opinion was supported with zeal, talent, and perseverance, by some of the leading members of the profession. Conspicuous among them was Dr. Rush, who was ably supported by Drs. Hutchinson, Leib, Redman, and a few others. To the celebrated physician above named credit is undoubtedly due for having been foremost in promulgating publicly the doctrine in question on that occasion. In October (3d), and consequently during the course of the epidemic, he addressed a letter to Dr. John Rodgers, of New York, which appeared shortly after in the *Federal Gazette*, of this city, and was subsequently published in Duncan's *Medical Commentaries* (xix. 345). He therein remarks: "I shall begin by observing that I have satisfactory documents to prove that the disease was generated in our city. To suppose, because the yellow fever is an endemic in the West Indies, and because it seldom occurs in North America, that it can exist among us only by importation, is as absurd as to suppose that the hurricanes which are so common in the West Indies, and which occur here only once every twenty or thirty years, are all imported from that country." Nor was this all. Scarcely had the epidemic disappeared, before the same eminent physician issued a pamphlet explanatory of the grounds upon which he based his views on the subject, and addressed letters on the subject to Dr. Redman and Mr. Swanwick, which appeared in *Dunlap's American Daily Advertiser*, for December 11 and 19. Soon after, his history of the epidemic appeared; and in this and many other publications he supported the same opinion, which he continued to advocate to the moment of his death.

Of the opinion entertained on that subject by Dr. Hutchinson, who was then the "Inspector for Sick Vessels," we have proof sufficient. We are told that, by order of the governor, he wrote letters to several physicians, for information. They replied. "This and other information collected by the doctor," says Dr. Rush, "was communicated to the health officer (of the port, Dr. Falconer), in a letter dated the 27th of August, in which he mentioned the parts of the city where the disease prevailed, and the number of persons who had died, supposed by him to be about 40, but which subsequent inquiries proved to be more than 150. He mentioned further, in addition to the damaged coffee, some putrid hides and other putrid animal and vegetable

¹ Currie, *Fev. of 1799*, p. 41 (note).

substances, as the supposed cause of the fever, and concluded by saying, as he had not heard of any foreigners or sailors being infected, nor of its being found in any lodging-houses, that 'it was not an imported disease.'¹

In 1794, Drs. Devèze and Nassy published their accounts of the disease of the preceding year, and supported the doctrine in question, which it is evident they had upheld all along. In 1797, the same view of the subject was advocated by a still larger number of physicians, and ably defended in a document issued by a number of gentlemen, who soon after became incorporated as the Academy of Medicine—Drs. Rush, Caldwell, Dewees, J. R. Coxe, P. S. Physick, Reynolds, Sayre, Otto, Boys, Cooper, Stuart, Pascalis, and Strong. All of these admitted the contagiousness of the fever under certain circumstances of weather and of predisposition in the body; but all admitted also that it was of local origin in Philadelphia during the season in question, and arose from the putrid exhalations from the gutters, streets, ponds, and marshy grounds of the suburbs. Even Dr. Caldwell, who, in 1793, besides advocating the doctrine of contagion, maintained that "to suppose the air of the streets generally is contaminated with the infection, is an outrage, or frantic presumption," and that such an opinion can originate only in a total ignorance of medical science,² now sided with Dr. Rush and others on the subject before us, and a few years after became a convert to non-contagionism, which, together with the other doctrine, he warmly and ably defended to the last.

At each repetition of the disease the number of supporters of its local origin increased; so that, at the present day, it would be difficult to find one within the limits of the city who advocates a contrary belief, while it has been ably elucidated in our medical periodicals and in separate publications.³

But although the doctrine of local origin from causes of infection was early advocated here, and Dr. Rush, from his publications in its support, has been regarded as its father, his claims to the title can only have reference to occurrences in this city; for the same views had long before received the sanction of many members of the profession in other parts of this country and elsewhere. Dr. Cadwallader Colden, once the Lieut. Governor of New York, in his account of the epidemic of that city in 1741, written in 1743, and published long after in the 14th volume of the *Medical Repository* (pp. 1, 159), advocated the origin of the disease from sources of infection—without, however, denying the possibility of its importation. Dr. Caldwell, who, at the time of his writing, could not have been aware of the views of Dr. Colden, states, that the doctrine can be traced back with certainty to the year 1746, at which time the disease prevailed in that city. The Rev. Dr. Dickenson, an enlightened clergyman, and a respectable physician of Elizabethtown, N. J., in an address published in the *New York Weekly Post Boy*, a public

¹ Rush, iii. 45.

² See article signed ARETEUS, JR., Dunlap's Am. Daily Advertiser, Aug. 30, 1793.

³ Caldwell, Accounts of Fev. of 1799, 1803, 1805; Chapman, Med. and Phys. Journ., ix. and x.; S. Jackson, Fev. of 1820; Emlen, N. A. J., vol. v.; Wood, Practice, vol. i., Monges, N. A. J., vol. ii.

print of that period, declared to the citizens of New York, "during their calamity, that the pestilence which afflicted them was neither an immediate scourge from the hand of the Deity, nor yet an evil imported from a distant country, but the offspring of their own domestic filth."¹

In Charleston, the doctrine of infection was early adopted by Moultrie, whose dissertation appeared in 1749. "The proximate cause of this disease," he says, "seems to be a diseased condition (*acrimonia*) of the fluids, and the principal of the antecedent causes, excessive heat, and too great motion or exercise, besides copious exhalations, and putrid miasmata, which, by the power of heat, are elevated (evaporated) into the air from ponds, marshes, subterraneous caverns, and especially from the extensive woods which, in America, abound in poisonous trees, are to be enumerated among the causes that tend to produce this disease. Inasmuch as the subtle particles of these miasmata and putrid exhalations, drawn together with the air into the lungs, or received into the stomach with the food, enter the pores of these viscera, as also of the external skin, they mingle with the blood, operate, perhaps, after the manner of a secret or hidden fermentation, produce in the fluids a morbid acrimony, and so affect the fluids with disease" (p. 6).

Some time after, Chalmers advocated the same views in his excellent volumes on the climate of South Carolina (i. 163); and it was universally received in 1792. The epidemics which, in 1794, 1795, and 1798 visited Baltimore, New York, Norfolk, and Boston, were referred to the same cause by the leading physicians of those cities, where, as well as in other parts of this country, it now meets with the sanction of the majority of medical men.²

¹ Caldwell, *Mem.* 1801, pp. 228-9.

² Bayley, *Fev. of 1795 in New York*, pp. 50, &c.; *Ib.*, *Letters from the Board of Health*, pp. 7, &c.; Seaman, in *Webster's Coll.*, p. 33; E. H. Smith, *Ib.*, p. 66; Taylor, Hansford, Selden, and Whitehead, of Norfolk, *Med. Rep.*, iv. 205-329, v. 129-225, vi. 247; Drysdale, i. 33; Miller, *Works*, p. 89; Dick, *Med. Reposit.*, vii. 192-3; A. Hosack, p. 21; John Warren, p. 505; Chadwell, pp. 9, 12; *Rept. of the Acad. of Med. of Philad.* 1798. p. 5; Shecut, pp. 92, 96, 117; Johnson, *Charleston Journ.*, iv. 157-8; Thomas, p. 64; Barton, *Rept.* 1851, p. 35; *Ib.*, *Rept.* 1853, p. 309; Tailleferre, p. 10; Chambery, *Med. Repos.*, xxi. 116; Gros, p. 7; Brown, *Fev. of Boston 1798*, p. 30; Vaughan, *Fev. of 1802*, p. 6; *Ib.*, *Med. Repos.*, iii. 368, iv. 242; Jamison, *Recorder*, vi. 436; Ticknor, N. A. J., iii. 216; Warren, *Boston Med. and Surg. Journ.*, ii. 38; Parsons, *An Essay on the Comparative Influence of Vegetable and Animal Decomposition as a Cause of Fever*, *Essays*, pp. 198, &c.; Revere, *Med. Record.*, iii. 216; Reese, p. 19; Waring, p. 17; Potter, p. 20; Davidge, p. 65; Smith on *Epidemics*, pp. 77, &c.; Whitridge, *Southern Med. and Surg. Journ.*, ii. 458, *New England Med. Journ.* viii. 382-3; *North Am. Rev.*, x. 400; Baxter, *Repos.*, xxi. 10; Cartwright, *Med. Recorder*, ix. 226; Watts, N. Y. *Med. and Surg. Reg.*, i. 221, &c.; Dalmas, p. 19; Valentin, p. 84; *Ib.*, *Fluxion de Poitrine*, pp. 121, 148, 150; Tooley, p. 7; *Official Statement of the Late Epidemic (1819) of the City of Baltimore*, by the District Med. Soc. of Maryland, *Med. Rep.*, xx. 347; Archer, v. 63; Hill, v. 88; De Rosset, *Rep.* ii. 143-4; Smelt, *Rep.* ix. 125; Daniel, p. 38; *Rept. on Fever of Augusta*, in 1839, p. 38; Ramsay, *Med. Rep.* iv. 218, 219; *Ib.*, vii. 234; *Ib.*, *Hist. of S. C.*, ii. 87; Harris, *Barton's Med. Journ.*, ii. 32; *A Phys. Inquiry into the Origin and Causes of the Pestilential Fevers*, N. Y. 1798, pp. 19, &c.; Fenner, *Fev. of N. O. in 1853*, pp. 73, &c.; Dowler, *Fev. of 1853*, p. 46; Irvine, p. 5; *Letters on Fev. of Baltimore in 1819*, pp. 29, 35, 43, &c.

In Europe, the same doctrine was early advocated. Chirac (*op. cit.*, i. 169), long ago referred the fever which prevailed at Rochefort in 1694, and which bore a great analogy to the yellow fever, to the miasmal exhalations for which that city has always been noted, and which, at that period, were remarkably active.¹ The doctrine has since enumerated, and continues to enumerate, many partisans;² while in tropical regions it has, from a remote period, received the support of eminent physicians and historians. Peter Martyr attributed the sickness from which the adventurers at Darien suffered, to the heat of the sun, the corrupt water, and the infectious air and venomous vapours arising from the contagion of the soil. The views of Oviedo (lib. 16, cap. 3), and Gomara (lib. 2, cap. 9), were not very different. The natives of Guadaloupe called the disease *ibomanhatina*, which Breton informs us is equivalent to the *cattiva aria* of the Italians (*Dict. Caraïbe*, p. 266). Ligon, in his *History of Barbadoes*, refers the sickness which prevailed there in 1647, to the infectious exhalations arising from the localities of the port (pp. 21, 25). Desportes remarks that the yellow fever, or *maladie de Siam*, is one of those diseases the cause of which must be sought in the "constitution of the atmosphere" (i. 191); and in describing the diseases which prevailed in St. Domingo during the summer of 1735, 1737, makes use of language which bespeaks him to have been a decided infectionist.³ Arthaud, who long practised in the same place, speaking of the septic poison (*venin septique*) which rises after the fall of the autumnal rains in that island, says, that among other morbid effects, "they bring on their plagues or malignant fevers, which, though of local origin, are generally said to have been imported in ships from the coast of Africa."⁴

¹ From the period of the year at which this remarkable epidemic broke out and spread, as well as from the character of the symptoms the disease presented, and the lesion discovered on dissection, we may justly regard it as closely allied to, if not identical with, the latter. Chirac's celebrated eulogist, Fontenelle (ii. 305), denominates it *Maladie de Siam*.

² R. Jackson, *Fev. of Spain*, vi. 47; Amiel, *Edinb. Med. and Surg. Journ.*, xxxv. 264; *Ib.*, in Johnson, pp. 254, 291; Ferrari, *Edinb. Journ.*, xix. 369; Rochoux, p. 146; De Maria and Lasso, quoted by Rochoux; O'Halloran, pp. 12, 13, 26, &c.; Gillkrest, ii. 281, &c.; *Ib.*, *Second Quarantine Rept.*, Lond. 1852, pp. 157, &c.; T. Smith, *Edinb. Journ.*, xxxv. 25, &c.; Frazier, *Med.-Chir. Rev.*, xiii. 337, &c.; P. Wilson, p. 28; Chervin, *Lettre à M. Monfalcon*; *Ib.*, *Réponse à M. Guyon*; *Ib.*, *Examen des Principes de l'Administration en Matière Sanitaire*, &c. 1827; *Ib.*, *De la Fièvre Jaune observée en Espagne*, &c. 1828; *Ib.*, *De l'Opinion des Médecins Américains*, &c. 1828, and other works; Burnett, p. 341; Wild, Martindale, Lee, Short, Playfair, Thomson, Donnet, Brady, Foot, quoted by Burnett, pp. 439, 440; Salva, quoted by Dariste, pp. 59, 140; *Ib.*, *Arch. Gén.*, v. 478; *Observateur Méd. de Marseilles*, iii. 18; Boyd, *Fev. of Minorca*, in Johnson, p. 301; Denmark, *Med.-Chir. Tr.*, vi. 317; Proudfoot, *Edinb. Med. and Surg. Journ.*, xxviii. 293; Doughty, pp. 173, 180; Paradis, *Archives*, xx. 445; Porta, *Archives*, v. 277; Pascalis, *Rep.* ix. 377; Blair, p. 38; Campbell, in *Bancroft's Seq.*, p. 421; Piguilem, *Obs. de Marseilles*, iv. 13; *Ib.*, *An. de la Méd. Physiol.*, i. 404; Alphonzo de Maria, pp. 125, &c.; Dupuytren, *Report on Costa Sicre*, p. 9; Fabbroni, *Edinb. Journ.*, i. 509; Ch. Maclean, *Evils of Quarantine*, p. 122; Costa, pp. 24, 6; Reider, *Mém. sur la Fièvre Jaune*, pp. 10, &c.

³ *Maladie de St. Domingue*, i. 87.

⁴ *Description de l'Hôpital General du Cap*, p. 12.

The same opinion was advocated by Dazille,¹ by Williams,² Makittrick,³ Lind,⁴ Hillary,⁵ and other of the older physicians. Moseley, too, though not affirming the opinion in explicit terms, says enough, particularly in his medical tracts, to indicate the nature of his tendencies on the subject.⁶ At a somewhat later period, Dr. Bancroft, in a work of great merit and universally known,⁷ enlarged on the proofs of the correctness of the doctrine of local infection, which was soon almost universally admitted, so far especially as relates to the fever as it prevails in tropical regions.⁸

From that period the same doctrine has been very generally, and is now almost universally admitted, as well by those who believe in contingent contagion as by those who reject the doctrine. Indeed, for many years past, the belief in the infectious character or local origin of the yellow fever is the one most prevalent, both in tropical and temperate climates. It is the creed of by far the largest number of non-contagionists, especially of those who believe in the identity of the disease with malarial fevers generally; and is, besides, adopted by many who, though admitting that the disease may, and does often, assume a contagious character, under particular circumstances, or possesses it always to a greater or less extent according to the degree of purity of the atmosphere, nevertheless recognize, that it usually, but not necessarily, arises

¹ *Maladies des Pays Chauds*, pp. 9, 69.

² *Essay on the Bil. Yel. Fev. of Jamaica, &c.*, p. 16.

³ P. 50.

⁴ *On Seamen*, p. 292.

⁵ P. 153.

⁶ *Medical Tracts*, p. 217; *Ib.*, *On Dis. of Trop. Cl.*, p. 435.

⁷ *An Essay on the Disease called Yellow Fever*, 1811; *Ib.*, *Sequel to an Essay, &c.*

⁸ Dr. Armstrong, *Influence of Climate on the Human Constitution*, p. 41, London, 1823, says of Dr. Bancroft: "When I had the pleasure of his acquaintance in Jamaica, in 1827, '8, '9, he was in practice as a physician at Kingston, and his opinions had undergone a change. I detailed to him certain experiments I had performed, and he stated more than once that he had spent many of the best years of his life in the West Indies, and was now more doubtful than ever whether any injurious exhalations arose from the soil"—but, he added, "We cannot dispense with them until you find something better to substitute." Dr. Magrath, of Jamaica, during his recent visit to this country, assured the author that the report respecting the change in Dr. Bancroft's views was unfounded.

Fontana, pp. 11, 76; Lind, pp. 90, &c.; J. Clark, p. 53; R. Jackson, *Sketch*, vol. i., 11, 211; *Ib.*, *Outline*, p. 102; *Ib.*, *Cold Affusions*, p. 135; Ferguson, *Med.-Ch. Trans.*, viii. 110; *Ib.*, *Recollections*, p. 143; J. Wilson, pp. 138, 139, 151; Macmillan, *Edinb. Journ.*, x. 38; Osgood, p. 18; Davidson, *Repos.*, viii. 248; Ralph, ii. 55; Frost, xii. 209; Parson, *Edinb. Journ.*, viii. 388; Gillespie, pp. 61, 131; Trotter, i. 328, ii. 91; Lempriere, ii. 4, 35, 48; Jones (of Barbadoes), *Archives*, xix. 619; J. H. Dickson, *Edinb. Journ.*, xiii. 35; Wallace, *Ib.*, xlv. 271-277; Mortimer, *Med.-Ch. Rev. and Journ.*, iii. 9, 93, 182; Thomas Clark, p. 14; Lefort, p. 14; Pugnet, pp. 331, 334-5; Bally, pp. 22, 345, 392, 410, 413; Hunter, p. 147; H. McLean, pp. 24, 35; Rufz, *Exam.*, iii. 108; Humboldt, pp. 762-3; Savarésy, p. 227, 239; François, p. 7; Chervin, *op. cit.*; Henderson, p. 8; Gilbert, p. 93; Burnett, *Fever of Ascension*, p. 52; Arnold, pp. 27, 157; Dariste, pp. x. 31, 34; Maher, p. 830; *Dict. des Sci.*, xii. 204; Evans, p. 12; Pinkard, iii. 415, 416, &c.; Furlong, *Med.-Ch. Rev.*, xxv. 289; Belcher, *Edinb. Journ.*, xxiii. 248-9; Dyott, *Med.-Ch. Rev. and Journ.*, iv. 1003; Hartle, *Ib.*, 996-7; Chambole, *An. de la Méd. Physiol.*, xiii. 200; Blair, pp. 52-3; Musgrave, *Med.-Chir. Trans.*, ix. 130; Lefoulon, p. 359; J. Davy, *Edinb. Journ.*, lxxii. 281, 282; Bone, p. 18.

from the operation of local causes of infection. Nay more, we find zealous importationists¹ acknowledging the occasional production of the disease from such causes. We also find some who, though regarding the fever as the offspring of the impression of tropical heat on a system unaccustomed to its effects,² make similar acknowledgments; while by not a few contagionists and anti-contagionists such local agencies are regarded, not as the efficient, but as one of the exciting causes of the disease.³

The Poison of Yellow Fever not precisely similar to that of other Malarial Fevers.—While entertaining these views respecting the origin of the yellow fever from local sources of infection, I do not wish to be understood as meaning that the poison producing the disease is precisely similar to that giving rise to ordinary remittent and intermittent fevers, and that it differs from it only in regard to degree of intensity. In a preceding chapter, reasons were assigned in support of the opinion that the yellow fever, though belonging to the same class or family of diseases as the other febrile complaints mentioned, nevertheless differs from them to a sufficient degree in regard to phenomena to justify us in regarding it as constituting a separate and distinct complaint. If such be the case, the effluvia giving rise to it may reasonably be viewed as constituted of different elements—produced by the decomposition of different materials—or, perhaps, of different proportions of the same elements proceeding from the decomposition of similar materials, but under the influence of peculiar external agencies. That the poison of remittent and

¹ Dr. Chisholm, whose attachment to the doctrine of contagion and importation is proverbial, and who, deriving the fever Grenada from Bulam, would not allow it to have been developed in that island, or any other place where it subsequently appeared, somewhat inconsistently admits its springing up in the Hankey—not from a morbid virus derived from the Africans of that island, but from the foul condition of the vessel itself—which foulness, owing to some peculiarity in the African atmosphere—gave rise to a new pestilence (*Letter to Haygarth*, pp. 217, 218). In his account, also, of the fever of Demerara in 1800, he traces, as we have seen, the origin of the disease to a ship which arrived there from *Liverpool*, after touching at Surinam, and the filth of which “occasioned by a cargo of horses, and the extreme neglect of the officers and crew, was such as beggars description” (*Letter to Dr. Davidson*, in *Med. Repos.*, v. 229). Finally, he regards the common yellow fever of the West Indies, which he erroneously views as a different disease from his Bulam pestilence, as arising from the miasm of putrefaction. If the disease—a new pestilence—can arise from the effluvia of a ship—especially one from *Liverpool*—for nothing is said of fever existing at Surinam—there certainly can be no reason for denying that the same cause may produce it ashore.

² Dr. Rochoux, though advocating the doctrine of infection, so far as regards the fever of Barcelona, and other places in temperate climates, denies its applicability to the production of the yellow fever of tropical climates, which he regards as a totally different disease, and as being invariably the product of excessive heat, light, &c. Yet he describes an occurrence and prevalence of the fever on board of a French war brig—the *Messenger*—which he attributes to effluvia arising from a quantity of mangrove bushes, which had been imprudently placed in the hold. In another case, he attributes the disease to exhalations arising from green timbers used in the construction of the vessel (pp. 60, 61).

³ Tommasini, § 84; Dickson, *Med. and Phys. Journ.*, iii. 265; *Ib.*, *Eclectic Journ.*, iv. 112; Simons, p. 18.

intermittent fevers is the same, we must admit; for these diseases are daily converted into each other in the same individual—intermittent into remittent, remittent into intermittent; and when a number of individuals are exposed to the same paludal effluvium, it not unfrequently happens that while some are affected with the one, others are stricken down with the other, form of the disease—the difference depending on the degree of energy of the cause, on the length, period, and place of exposure, or on individual peculiarities. But it is not the less certain, that diseases by infection in different or the same country—all arising from miasmatic emanations—present differences of a more or less notable character.

The jungle fever of India is not exactly the same in form as the remittent of Africa—the fever of Batavia differs in some respects from the remittent of this country, or of the Gambia. The febrile diseases of Rome are not precisely similar to those of the West Indies or Batavia, and the Walcheren fever differs in like manner from the like complaint of both, or from that of Breskau. In France, the fevers of Rocheford do not present exactly the same characters as those of Bresse and the plain of Forez. The morbus Hungaricus¹ of some centuries back, bore but a faint resemblance to the Athenian plague described by Thucydides; to the oriental plague of the present day, or to the sweating sickness and black death of the fourteenth century. The Mediterranean remittent, described by Sir W. Barnett, is somewhat unlike the periodic fevers of England, or of northern Europe. It differs, too, in some particulars from the fever of Greece, an account of which is found in the justly celebrated books of epidemics of Hippocrates; and from that of Algeria, or the Morea. Again, the typhus of London, or typhoid of Paris, and other places, would not easily be mistaken for that form of disease which prevailed among our negroes in 1820 and 1821, and in Banker Street, New York.

Sources of the Effluvia different.—These fevers all differ from each other; while none of them resemble exactly the form of disease which constitutes the object of our present inquiries. These must all be regarded as the offsprings of miasmatic effluvia of some sort. Nevertheless, the differences between them are, in some instances, quite considerable; affecting their physiognomy principally, and depending on the nature of the organs implicated, and the degree and manner of that implication; and though this difference may, in part, be ascribed to a difference in the constitution and temperament of those attacks, or to peculiarities in the epidemic constitution of the atmosphere, the explanation cannot be of universal application, and we are obliged to refer them to variations in the composition of the effluvia to which those diseases are due. And if such is the case in regard to the etiology of the fevers mentioned, the admission of a difference between the cause of yellow fever and the various forms of miasmatic disease is still more proper, inasmuch as the former differs from the others in a more marked manner than they differ from each other.

¹ Pringle, p. 188; Sennertus de Morbo Hungarico; see Smith's Transl. i. 166; Med. Reposit., i. 15.

This probable diversity in the nature and composition of the miasms in question, as exhibited by the diversity of the febrile phenomena they produce, has called the attention, and met with the assent of many writers of former and present times. It is referred to by Lancisi;¹ approved of by Rochoux,² Ferrus,³ Desland,⁴ Littré,⁵ and many others of equal note; and particularly insisted upon by Twining,⁶ J. M. Smith,⁷ and Monfalcon.⁸

Be this, however, as it may, certain is it, that the yellow fever, though of miasmal origin, is not the offspring of the effluvia of ordinary marshes, or issuing from the margin of streams, ponds, lakes, open flat countries or savannas, or of swampy or humid soil generally, to some or all of which remittents and intermittents are usually traced. For, in many places where such localities abound, yellow fever is unknown; while, on the contrary, the latter prevails where such characters of localities are absent. In a word, it will be found that the several forms of fevers do not necessarily manifest themselves in the same situations. Thus, in this country, while the yellow fever is a disease of cities and towns—generally of only a portion of these—common remittents, and especially intermittents, spread on the skirts of these, and more particularly in the open country, and seldom, if ever, penetrate, to any extent at least, within thickly built and populous habitations; and when they do so, we very generally discover that they spare those peculiar locations where the yellow fever is rife. This is true, whether we turn to the southern, southwestern, or middle sections of this country. In the first, the yellow fever, when epidemic, generally diffuses itself over a large surface of the city or town; and in some portions of this surface remittents sometimes prevail. But when they do, it is to a limited extent, most generally their main habitation being beyond the limits of the infected place, where the yellow fever never extends. Take the city of Charleston as an example: the inhabitants of the surrounding country, whether on the main land or in the neighbouring islands, are sorely afflicted in certain seasons with remittent and intermittent fevers. To escape from these, they take shelter in the city where such complaints do not prevail. But here they encounter the yellow fever, which, in its turn, never extends beyond the limits of the city. As a general rule, the same results are obtained in Mobile, New Orleans, &c. In regard to the latter city, it has been found that the unacclimated may reside in the midst of swamps, marshes, pools, or standing water of any kind, and enjoy perfect exemption from the yellow fever, while the city is ravaged with this disease. Let them go into town, and they are attacked. It is only in times of a widely diffused epidemic constitution of atmosphere, when the disease extends far and wide, that it passes beyond the barriers here pointed out.

¹ Op. cit. cap. xi. 34–36.

² Recherches sur les Différentes Maladies, &c., p. 135.

³ Dict. de Médecine, 1st ed. viii. 68.

⁴ Dict. de Méd. Pratique, vii. 73.

⁵ Œuvres d'Hippocrates, ii. 578.

⁶ Diseases of Bengal, ii. 288.

⁷ On Epidemics, pp. 48, 67, &c.

⁸ Op. cit. pp. 65, 69. See also Rev. of Devèze, Med. Repos., xxi. 187; Forry, op. cit., pp. 285, 291; Desportes, Bulletin de l'Acad., v. 380.

In the cities of our Middle States, Philadelphia, New York, Baltimore, and Boston, the yellow fever, when it prevails, is more restricted in regard to the extent of the surface it occupies, and there common remittents are but seldom, and intermittent scarcely ever seen.

The history of the disease in the West Indies furnishes many illustrations of this fact—remittent fever prevailing extensively where the yellow fever never shows itself; and conversely, the latter spreading extensively and with great malignancy where the former are scarcely known.¹ There, also, the yellow fever is usually found to be a disease of cities and garrison barracks, while remittent fever is a disease of the open country. Ships at sea are not ordinarily subject to remittents—seldom, if ever, to intermittents—yet instances of yellow fever infection on board are of common occurrence.

The island of Barbadoes is remarkably free from marshy grounds, and is thereby preserved from febrile diseases generally; but the yellow fever has there often prevailed, from the days of Warren to our own, very extensively and fatally. Brimstone Hill (St. Kitt's) and Stony Hill² (Jamaica) are far from marshy, and are exempt from remittents; nevertheless, they have been the seats of fatal visitations of yellow fever, while the surrounding marshy localities were free. On the other hand, the islands of St. Lucia, Dominica, and Demerara, where the soil is miasmatic, and consequently autumnal fevers common, the yellow fever prevails only in the towns or barracks, and, so far, has generally done so at long intervals of time. In some islands, during certain seasons, remittents and intermittents spread widely without being in any way commingled with the other disease, as was the case in Dominica in 1843; while at other seasons the yellow fever prevails extensively and alone.³ In the course of eighty-eight years, the yellow fever has only prevailed three times in Cayenne—in 1762, 1802, and 1850—though the soil is marshy to a proverb, and periodic fevers prevail to a considerable extent. At Martinique, while the yellow fever reigns at Fort Royal, it does not show itself at Port Marie, at the southern extremity of the island, which is much more marshy than the former, and the seat of periodic fevers.⁴

The same thing has been observed in Spain and Italy; and it is a well-known fact that bilious remittent and intermittent fevers are very common in some countries—in Asia, Eastern Europe, Eastern Africa, and South America—where, notwithstanding the existence of ample sources of paludal exhalations, the yellow fever is seldom, if ever, heard of.⁵

¹ Imray, lxiv. 332, 335; Wilson, p. 89; Ferguson, p. 148; Doughty, pp. 66, 67; Moreau de Jonnes, p. 158.

² Arnold, p. 172.

³ Imray, lxiv. 334.

⁴ Moreau de Jonnes, p. 158.

⁵ That facts present themselves, militating apparently against the propriety of referring miasmatic fevers to the exhalations of marshy soils, must be familiar to every medical inquirer; for endemic fevers are found to prevail as well in Europe as in America and Africa, not only under very diversified conditions of meteoric phenomena, and during diversified periods and varieties of seasons, but also in places possessing a geological character and peculiarities of soil different from those mentioned; and conversely, they are found to fail under circumstances which are usually regarded as most conducive to their develop-

The malarial effluvia giving rise to the yellow fever may be inferred to differ from those occasioning remitting and intermitting fevers, not only from the difference existing in the phenomena of those diseases and the character of the localities they visit, but from the fact that they are apparently governed, on many points, by different laws. It is admitted by Dr. Chervin, a strong and uncompromising advocate of the unity of all miasmal fevers, that although the effluvia occasioning the yellow fever are, like those giving rise to remittents and intermittents, wafted by the wind, still, as regards the former, the deleterious effects they produce do not extend as far as is the case with the latter. But it may be doubted whether the extension is even as great as this gifted writer was disposed to believe; for while the malaria of common remittent and intermittent fevers is wafted to a considerable distance—even several miles—over land, and has been asserted by some, though denied by others, as regards some localities at least, to extend to a greater distance over the surface of water, facts show that the poison of the yellow fever extends but little beyond the source of its origin—attacking one or a few streets, or one side of a street, ship, &c.—and not unfrequently progress in a contrary direction to the wind. Again, while the miasm of remittents and intermittents ascends to a considerable height—sometimes 1,000 feet—and exhibits a tendency to that effect, the poison of the yellow fever creeps along the surface of the ground, selecting the lower stories of houses and hospitals, and leaving the upper uncontaminated, and does not manifest a tendency to be wafted to any great elevation above the surface of the place where it is evolved.

ment.* On the other hand, the yellow fever, though generally occurring in situations such as I have described, has appeared under circumstances of contradictory character. Nevertheless, the experience of ages has shown that the domain of intermittents and remittents is to be found in countries overspread with marshes, abounding in alluvial deposits and a luxuriant vegetation, and subject to periodical inundations. From the days of Hippocrates, too, it has been found that by drainage places may be rendered healthy which before were insalubrious, while the yellow fever appears in marshy situations so seldom, that we are justified in the conclusion that the ordinary exhalations from these do not necessarily contain the cause of the disease, which, when it occurs under such circumstances, arises from something adventitiously added, but not essentially belonging to them.†

* McWilliams, p. 156; Bryson, p. 196; Edinb. Journ., lxi. 132; Rep. on Sickness of British America, p. 102.

† Wilson, p. 89.

CHAPTER XXXII.

INFECTION—CONTINUED. NATURE OF MATERIALS GIVING RISE TO THE YELLOW FEVER POISON.

Animal Decomposition.—As to the materials which, by their decomposition, give rise to the malarial poison constituting the efficient cause of the yellow fever, different opinions have been and continue to be entertained. Like the poison of ordinary and other pestilential and malignant fevers, the one in question has on some occasions been ascribed to the decomposition of dead animal matter. Dr. Cartwright attributes the fever of Washington (Miss.) to a mass of putrid bacon. The same author refers the epidemic of Natchez, in great measure, to an analogous cause. Dr. Caldwell states that he witnessed once, in Philadelphia, several cases of yellow fever which seemed fairly referable to a mass of putrid fish.¹ He has also seen the disease produced by putrid oysters and hides.² Other instances of the kind might be gathered; and we should bear in mind that the results, already adverted to, obtained from the injection of putrid animal substances in the blood, as well as the experiments performed by Magendie, with a view of ascertaining the effects produced by inhaling the effluvia springing from animal substances in a state of putrefaction, lend support to the opinion in question.³

By many, however, the agency of animal decomposition in the production of this effect has been controverted. Ferriar⁴ long ago denied that agency, so far as regards pestilential diseases generally; and Drs. Warren⁵ and Dunglison (p. 75) in this country, and Bancroft in Europe,⁶ have taken pains to accumulate facts and testimonies in favour of the same opinion, mostly in its application to ordinary and yellow fevers. Dr. Revere,⁷ in his account of the epidemic of Baltimore in 1819, shows conclusively that such decomposition exercised no agency in the production of the disease. No less positive on the subject is Dr. Merrill, formerly of Natchez, who makes the following statement relative to the epidemic of Pensacola in 1822. The disease on this occasion, as remarked before, “was attributed to the importation of damaged codfish, and circumstances went far in support of the opinion.” “But it is a remarkable fact, that came under my own observation, that the United States troops stationed at Fort Barrancas, consisting of about

¹ Thoughts on Febrile Miasms, &c., pp. 491, 492.

² Malaria, p. 69. See Fever of 1805, p. 60. Dr. Rush (iv. 96) also attributes the same fever to a like cause.

³ Journal de Physiol., iii. 85.

⁴ Works, Origin of Contagious and New Diseases, i. 274.

⁵ Boston Med. and Surg. Journ., ii. 1, 17, 32. 1829.

⁶ Hygiene, p. 93.

⁷ Recorder, iii. 232.

one hundred persons, were exposed, in very close, uncomfortable quarters, almost in the midst of nearly a whole cargo of this putrid fish, which had been wrecked upon the beach, and yet not a single case of fever occurred among them during the four weeks they were thus situated, and not until three weeks after the fish had been removed. At the same time, a battalion of infantry that was cantoned one mile distant, in a dry, airy situation, was sorely afflicted with violent bilious fever. The sailors, who had been living on board this fish vessel for a number of months, in the West India seas and harbours, and in the midst of putrid exhalations that were almost suffocating to a stranger, all arrived in Pensacola in good health, and only suffered, in common with the other inhabitants, after a residence of three or four weeks in the city.”¹

When we take these facts into consideration—when we bear in mind, at the same time, that the miasm of the yellow fever has often, as we shall see presently, been generated under circumstances, and in situations, where there did not exist any evidence of animal decomposition—on a sufficient scale, at least, to account for the effect—and advert, besides, to the recognized innocuousness of dissecting-rooms everywhere, of slaughter-houses, of cemeteries, of the large knacker’s establishment of Paris (Montfaucon), and other places, as attested by Parent du Chatelet,² as well as to the impunity which attended the disinterment in Paris of a large number of decayed bodies, under circumstances usually conducive to the generation of febrile diseases; when, I say, we take these facts into consideration, we cannot withhold the conclusion that, whatever may be the deleterious effects of animal putrefaction—and these cannot be denied, certified as they are by the highest authorities—however true it may be that it has produced, among other effects, diarrhœa, dysentery, and even continued fevers,³ febrile malaria cannot be regarded as necessarily dependent upon that putrefaction singly. We may infer that the latter can exist, even under the most favourable meteorological conditions, without giving rise to malarial fevers; that the yellow fever, in particular, is not, usually at least, the offspring of the miasm thus produced; and that if, in regard to the cases mentioned above, supposing no error to have crept in the narrative of their causation, they are not due to circumstances different from those to which they are ascribed, they constitute the exception, and not the rule.

Vegetable Decomposition.—Much more generally the production of morbid poisons, giving rise to yellow and other fevers, has been referred to the decomposition of vegetable matter. It has been the received creed of the pro-

¹ Phil. Med. and Phys. Journ., ix. 238.

² Parent du Chatelet, Des chantiers d’Ecarissage de la Ville de Paris, Ann. d’Hyg., viii. 139–147; Ibid., Rap. sur l’Enfouissement des animaux morts de Maladies Contagieuses, Ann. d’Hyg., ix. 109; Chrystison on Poisons, Am. ed., p. 490.

³ Des divers accidents graves occasionnés par les miasmes d’animaux en putréfaction, Mém. de la Soc. de Méd., i. 97; Chevallier, Lèttre adressée à M. —, au sujet de l’accident arrivé à M. Olivier dans un magasin de chiffon, Ann. d’Hyg., vii. 116; see Report to Parliament on Health of Towns; Animal Putrefaction capable of producing Fever, Med.-Chir. Rev., ii. 202; Vicq d’Azyr, vi. 320, 339, &c.; Ambrose Paré, lib. 22, c. 3; Haller, Physiologia, lib. 8, sect. 3, § 12; Warren, op. cit.

fession on the subject since the days of Laneisi, and is founded on the universal prevalence of the vegetable kingdom; the abundance of vegetable substances, in all varieties of conditions, in places where malaria exists; the known evolution of effluvia, recognizable to the sense of smell, from the decomposition of vegetable matter; the insalubrity of places where this decomposition is known to be going on; the salutary change occurring in that respect from the removal of the materials undergoing such decomposition (witness the jungles of the East, the rice and hemp plantations of various parts, and artificial collections of dead plants, trees, wood, &c.); and the difficulty of discovering another cause to account for the phenomenon in question.

But to this origin of the yellow fever poison much opposition has been made. "That the marsh poison," says Dr. Ferguson (who under that title places the poison of endemic, remittent, and yellow fevers), "cannot emanate from vegetable putrefaction, I think, must be evident from the fact that it is found most violent and abundant on the driest surfaces, often where vegetation never existed or could exist for the torrents, such as the deep ravine of a dried-up watercourse; and that it is never found in savannas or plains that have been flooded in the rainy season, till their surface has been thoroughly exsiccated, vegetation burnt up, and its putrefaction rendered as impossible as the putrefaction of an Egyptian mummy."¹ The facts adduced by Dr. Ferguson, in his paper on Marsh Poison, from which the above is taken,² are numerous, and go far towards supporting the views therein expressed.³

¹ Recollections, p. 196.

² Originally published in the Transactions of the Royal Society of Edinburgh, and since inserted in the volume of his Recollections, &c., and in the seventh volume of the Philadelphia Medical Journal.

³ Many of them, so far at least as regards those derived from observations made in Spain and Portugal, are corroborated by respectable authorities,* and by some are regarded as disproving conclusively the agency of vegetable decomposition in the production of the effect in question. Other facts and reasons are adduced†—the insalubrity of places, in Europe and this country, where no vegetable decomposition is visible; the renewed unhealthiness of places which, through the effects of draining, had been freed from fever; the prevalence of malaria in places formerly free from it; and, on the other hand, the innocuousness of certain hemp plantations in Virginia, and of the tarro in Polynesia. "If," says Dr. Dunglison, who in this matter follows Dr. Ferguson very closely, "vegetable decomposition, singly, were capable of producing malarious diseases in the cases already given, where no sensible evidence of vegetable matter existed, how much more strongly ought we to be exposed to them in situations where the vegetable kingdom flourishes in the utmost exuberance, and where the decomposition in question must be perpetually going on, in spite of every effort to the contrary? In country situations, we ought to be in the *foyers* of malarious emanations, as the very grass around us is suffered to go through its stages of growth and decay without interference; whilst the settlers of the forest are surrounded with dead vegetable matter, necessarily undergoing more or less decomposition. In the West India sugar ships, the drainings of the sugar, mixed with the bilge-water of the hold, create a stench that is absolutely suffocating to those unaccustomed to it, yet it is denied that malaria and malarious diseases are generated even from this combination" (p. 73).‡

* Dr. Brown, Cyclop., iii. 62, art. Malaria and Miasm.

† Dunglison, pp. 69, 71.

‡ Ferguson, p. 196.

But these facts, satisfactory as they may appear to the writers just cited, will not be viewed in the same light by all. They cannot counterbalance the evidence we possess of the morbid agency in question. Many of them admit of an explanation different from that offered, and they are opposed by others of a similar kind, having reference not to ordinary remittent only, but also to the yellow fever. Indeed, a survey of the whole subject must lead to the conclusion that the opinion which ascribes febrile emanations to the decomposition of dead organic matter, producing new compounds by the combination of the elements of these, is one justified by the strongest evidence short of demonstration. Some few writers in this country, and Parent du Chatelet,¹ Fourcroy,² and Girardet,³ in France, may deny the noxious effects of the decomposition of hemp; the first of these writers—who, with all his cleverness, was somewhat too fond of paradox—may have come to such a conclusion because experiments made in his own rooms did not produce fever in his wife, children, or himself, but the fact is too well ascertained to be denied.⁴ While everywhere experience has demonstrated the injurious effects arising from the ordinary mode of cultivating rice—effects which have called forth in several places the protective interference of governments⁵—the physicians of Georgia are ready to tell us what has been the result of the dry-culture system on the health of localities heretofore proverbial for their sickness.⁶ Putrid cabbages,⁷ the detritus of indigo,⁸ and other such substances, have not unfrequently been cited, on good authority, as having caused serious febrile complaints.

Infection from the decomposition of vegetable matter in ships is notorious. The case of the *Messenger*, related by Rochoux, in which the disease was evidently produced by a cargo of mangrove trees, is in point. Here there was no other cause to account for the morbid effect, unless we refer it to the vessel itself; but this would not alter the conclusion, as this vessel was made of vegetable matter. That the yellow fever is frequently caused by effluvia arising from ship-timber, has been fully ascertained. Rochoux instances a French war vessel, built of green wood, which was long prolific of fever. The epidemic which occurred on board of the *Priamus* frigate was evidently due to the action of the bilge-water on a quantity of chips and shavings that had been left in the hold.⁹

Dr. Bryson is not friendly to the idea of the fever arising from the decay of the timbers and planks of a ship, stating that it stands on the same

¹ Ann. d'Hyg., vii. 332.

² Cited by Monfaleon, p. 162.

³ Ann. d'Hyg., vii. 558.

⁴ Monfaleon, pp. 91, 161; Coutumes de Normandie, ib. of Amiens, see Ann. d'Hyg., vii. 250; Ord. of the King of Spain, July, 1627, ib., p. 251; Boudrillard, Eaux et Forêts, article Roussage; Ramazzini, pp. 244, 589; Bielt, Diet. des Sci. Méd., iv. 533; Rozier, Diet. d'Agriculture, ii. 435.

⁵ Monfalcon, p. 160; Bourelly, Gaz. Méd. de Montpellier, Oct. 1849, p. 99; Ib., Annales d'Hyg., xliii. pp. 328, 332; Cyel. of Praet. Med., iii. 60; Williams on Morbid Poisons, ii. 431; Boileau Castelnau, Annales d'Hyg., xliii. 331; Delongehamp, Diet. des Sci. Méd., xlix. 56, 57; Zimmerman, de l'Experience, ii. 402; Fodéré, Méd. Légale, v. 153.

⁶ Daniel, p. 29.

⁷ Rogers, An Essay on Epid. Diseases, p. 41.

⁸ Williams on Morbid Poisons, ii. 426.

⁹ Burnett, Rep., p. 34.

basis with the doctrine which attributed the fever of Hong Kong to the laying bare of a few square yards of solid rock. But he adds: "That green firewood is favourable to the production of disease on board of vessels, when closely stowed, seems, however, to be an opinion somewhat more consonant with the rules of legitimate induction; it becomes heated, and emits an effluvium strongly perceptible to at least one of the senses. The *Vestal*, in the year 1839, had a quantity of green wood on board, from which there arose so disagreeable a smell as to annoy the men who were at work in the hold; and when it was got upon deck, to be stripped of its bark, the fever then prevalent on board seemed to become virulent and fatal, while the men who were employed in stripping off the bark were all within two, or at most three, days seized with the disease in an aggravated form."¹ The disease on board of the British frigate *Sybil*, in 1829 and 1830, was thought by the surgeon, Dr. McKinnel, to be probably caused by the decomposition of the wood from the long-continued action of heat and moisture, aided perhaps by an accumulation of different substances under the lining or limber boards of the hold.² To similar causes—decomposition of the timber, or the contents of the hold—we must attribute the infection on board of the *Macedonia* frigate and other vessels to which reference has been made in another chapter.

Nor has the great opponent of vegetable decomposition been consistent in his views on the subject, for in the case of the *Regalia* transport, which suffered considerably from the yellow fever, he ascribes the disease to the foulness of the ballast (small stones, with a considerable mixture of mud and other impurities), and to the operation of sea-water, resulting from leakage, on that ballast, as also on a quantity of green wood with which that vessel was loaded. His words are explicit: "The cause of the disease was, therefore, I am clearly of opinion, to be ascribed to the green wood laid in at Sierra Leone, operating, along with the foul ballast, to furnish, when impregnated with the gases arising from putrid sea-water, morbid miasmata, similar to those that on land arise from marshes, when exposed to the influence of the higher degrees of atmospheric heat."³ To the same end might be cited the febrile effects of the decomposition of coffee, potatoes, pepper, &c., instances of which are not rare in medical works, and to some of which cases of yellow fever have been justly ascribed. The epidemics of this city, in 1793 and 1820, furnish illustrations of the deleterious effects—the first, of damaged coffee; the latter, of damaged potatoes. The fever of Boston, in 1819, was traced to a cargo of *corn*.

These facts, and others of similar kind which might, if necessary, be adduced, illustrate beyond doubt the agency of vegetable decomposition in the production of the poison of yellow fever; and if the disease, in the instances mentioned, was the offspring of such exhalation, we risk nothing in inferring that it is due to the same cause in other cases, although the materials of that decomposition may not be evident to the senses. This inference is the more natural, because, if in places visited by fever—and more particularly by that

¹ Bryson, p. 229.

² *Ibid.*, p. 54.

³ *Med.-Chir. Trans.*, viii. 117.

form of it which constitutes the object of our present inquiries—vegetable matter is not discovered on the surface, it assuredly exists beneath, in a more or less perfect combination with the soil.¹ We know that the mud on the margin of lakes and ponds—the theatre, often, of many febrile complaints—contains a large portion of vegetable excrements. Rich meadow land, also a frequent seat of fever, is in a great measure composed of such excrements; and though the surface may often become parched and dry, it is found in a different condition at short distances below, where miasma may be evolved. What takes place in relation to fevers generally, takes place also as regards the yellow fever, the occurrence of which is usually associated with the existence of masses of filth or mud, and has been traced occasionally to exhalations from beneath the surface of the soil. The reader need not be reminded that most soils, even though showing no traces of vegetable matter, may yet abound in seed of various kinds, even at considerable depths. Let the etiological inquirer test the fact with the assistance of the microscope, and he will not be long in perceiving the truth of the statement, and certainly he will then admit that these organic materials must be susceptible of decomposition, and give issue to exhalations which, though not cognizant to the senses, may nevertheless contaminate the air in a sufficient degree to produce fever.

In a previous chapter, I have dwelt at some length on the injurious effect of the upturning of earth during the prevalence of certain meteoric conditions of atmosphere, by which the organic matter lying beneath the surface is brought under the action of decomposing agencies. We have seen, also, the effects of large conflagrations, by which extensive surfaces containing masses of organic substances are exposed to the decomposing influence of heat and humidity. We have seen, in like manner, the frequent occurrence of febrile developments from the exhalation issuing from made ground, by which an abundance of organic matter is exposed to the same influence. Nor have we failed to see a similar effect from inundations, &c. That cultivation, by cleaning, washing, and removing all organic refuse, renders a locality more salubrious, is a fact well understood; but every one knows, also—and experience in this country confirms the observation—that endemic fevers often become, during several years after the soil is cleared of its more bulky vegetable products, more severe and prevalent than they were previously; and assume, at times, the epidemic garb. We cannot err in attributing these effects to the evolution of a greater quantity of more noxious effluvia from the surface of the soil, arising from the more direct exposure of the latter to the sun's rays than could be produced when the same surface was protected by a dense and exuberant vegetation—from the upturning, for purposes of culture, of a portion of rich and fresh vegetable mould, and from the decomposition of the materials—branches, leaves, chips, wood, &c.—left on the surface.²

¹ Brown, *Cycl.*, p. 2; Smith, pp. 45–6; Forrey, pp. 312, 313; Eberle, *Practice*, i. 42.

² Heustis, *Fev. of Alabama*, *Am. Journ.*, viii. 93; La Roche on *Pneumonia and Malaria*, p. 221; Stratton, *Edinb. Med. Journ.*, lv. 348; Pendleton, *Charleston Journ.*, vii. 451; Fort, *Southern Med. and Surg. Journ.*, iv. No. 11; *Ib.*, *Med. Practice*, p. 67; Williamson,

The production of fever from terrestrial exhalations would, by itself—and had we no other proof of the fact—afford evidence of the agency of organic, and especially vegetable decomposition in the generation of febrile miasms; for soil is a compound of a stratum of comminuted mineral substances and organic remains; and as effluvia cannot be supposed to arise from the former, we must refer them to the latter, knowing as we do, besides, that these are liable, under various circumstances, to decomposition, and that during the process of this decomposition they give rise to injurious effects. The conclusion will appear still more natural if we bear in mind that in all sections of this continent, and probably everywhere else, febrile diseases are more particularly rife in alluvial soils, consisting mostly of a rich vegetable mould—sometimes many feet thick. In other words, while sandy and sterile districts, with undulating surfaces—of primitive formation, and destitute altogether, or in great measure, of organic remains and of moisture—are usually free from fever, however favourable the climate may be to its production; localities of the other kind—of alluvial and level character—especially those presenting a geological formation of tertiary and cretaceous secondary deposits, with plenty of organic remains and ample humidity, are—the climate often being the same—remarkable for the development of the morbid poison.¹

These facts, which have reference to malarious fevers generally, have a bearing also on the form of it under consideration; for we find that in this country the disease is, in great measure, confined to the Atlantic plain or tide-water region extending from Boston to Texas, the geological character of which is of the kind mentioned—that its existence elsewhere is easily connected with a soil of similar structure, and that there—but more particularly when it occurs in situations of a different kind—it is in almost every instance traceable to effluvia arising from additional sources of organic, and principally of vegetable decomposition, docks, sewers, masses of filth, &c., and under circumstances favouring the accumulation and concentration of the morbid product—ill-ventilated streets, alleys, courts, and the like.

That the effect is not the result of aqueous decomposition many circumstances lead us, as already stated, to presume, and what is true in that respect, as to paludal fevers generally, will be found especially so in relation to the yellow fever. The latter appears in its worst forms in situations where, and under circumstances when, from the absence of humidity on the surface—and at some distance below—either entirely, or in any notable degree, such a decomposition cannot be looked for at all, or to a sufficient extent to account for the phenomenon. The prevalence of the fever is also retarded, or even

Med. and Philos. Reg., iii. 344; *Ib.*, Hist. N. C., ii. 193; Volney, *Climat des Etats Unis*, p. 309; Forry, *Climate of the U. S.*, p. 313; Drake, vol. i. 249, 311, 396, 404, 710, 717; U. Parsons, *Dissert.*, p. 206; J. M. Smith on *Epids.*, p. 23; Evans, pp. 15–23; Williams, ii. 422; Macculloch, pp. 126, 138, 172; Monfalcon, pp. 180, 181; *Cyclopedia*, iii. 61–2–4–82; Ludlow, *N. Y. Med. and Phys. Journ.*, ii. 83; Leblond, p. 23; Lefoulon, p. 23; Julia, p. 129; Boussingault, *An. de Chimie*, lvii. 151; Copland, ii. 758, Fenner's *Rep.*, ii. 932; *Transaction of Penns. State Med. Soc.*, ii. 42–65.

¹ Forry, pp. 280, 284.

arrested, by an addition of water to the localities infected, which by increasing the material of decomposition would, if these views were correct, favour the evolution of the poison, while in no instance has it been possible to refer the disease simply to exposure to large sheets or masses of water, however putrid and offensive the latter may be. That the fever seems at times to prefer rainy weather—when aqueous decomposition must be greater—and ceases its reign of destruction during dry and droughty spells, are facts which none can deny. But more frequently, as has already been seen, the reverse is the case; and when it so occurs, the effect is due to the aid which the humidity thus produced lends to the decomposition of the organic components of the soil, or of the materials by which the surface may chance to be supplied, and which by continued drought had been dried beyond the degree required for the chemical changes elicited in the process. Hence, we find that in some yellow fever regions the disease is more rife at the commencement and close of the rainy season, when the soil and its organic coating, after having been dried up, acquire moisture, or when, after having been deluged, they begin to dry through the desiccating power of a tropical sun.

With these facts in view, we must coincide with Dr. Ferguson and others in rejecting the agency of aqueous decomposition singly; but, while doing so and while recognizing the fact, long ago pointed out by Dr. H. McLean, that the longer water has been incorporated with the soil without fresh rain, the more virulent and dangerous the miasm becomes, we can only subscribe conditionally to the theory of the first of these writers when discarding vegetable decomposition alone or combined with the aqueous, he remarks, that one condition only “seems to be requisite to the production of the marsh poison (that of yellow fever included), on all surfaces capable of absorption, and that is paucity of water, where it had previously and recently abounded. To this, he says, there is no exception in climates of high temperature, and from thence we may justly infer that the poison is produced at a highly advanced stage of the drying process.”¹

These facts and inferences are correct; but from all that precedes, from the many instances of the fever produced by evident vegetable decomposition, and from the known effects of the introduction of putrid vegetable matter into the veins, we are bound to admit that, without the organic or vegetable remains contained in the soil or on its surface, no hurtful decomposition would follow; that it is these remains which (after being very wet, dry up by high atmospheric heat) eliminate during their desiccation, and under certain meteorological conditions, the morbid poison in question. The drying process insisted upon by Dr. Ferguson must apply to something tangible. If, as he affirms, it is neither animal nor vegetable matter, nor water, I am at a loss to know what it can be.

Nor can we admit the theory proposed by the late Dr. R. Jackson,² and subsequently adopted with slight modifications by Mr. Doughty.³ Accord-

¹ Notes and Recollections, p. 198.

² Outlines, pp. 105, 106; Strobel, i. 211.

³ On Yellow Fever, pp. 3, 77, 207.

ing to this theory, miasmatic effluvia consists of an emanation from living vegetable, through excess of organic life—*i. e.*, the cause of fever is analogous to the cause that moves vegetation. “It would appear,” says Dr. Jackson, “that the materials of vegetation abounding in excess, acted upon by a powerful cause, give out a principle which, not being expended in the growth and nourishment of plants, is diffused to a certain extent in the atmosphere, occasioning a derangement of such bodies as come within the sphere of its action” (p. 105). “In spring, the principle of vegetation is extricated in great quantity, while the capacity of plants is still small; an excess is consequently generated, and this excess extends its influence to a certain distance around. In summer, the extrication of the principle still increases; but the capacities of plants being extended in a greater proportion, the means are more adequate and the excess is actually less. In autumn, the growth of plants being complete, while causes still continue to produce a great extrication of the principle of vegetation, the excess abounds and occupies a wide circle. Upon this principle, also, may perhaps be understood the occasional unhealthiness of lands dry and rocky, bare and barren, or that produce only shrubs and trees of stunted growth” (p. 106).

This theory is not to me very intelligible, and has not been expressed in terms calculated to enable us to comprehend it, while the modification of it offered by Mr. Doughty is not less ambiguous. Dr. Wilson has justly remarked (p. 114), that the expression used must refer either to the principle of vegetation, or to its products, most probably to the former. If we speak of the principle of vegetation in the abstract, we speak of what we do not understand. If the principle of vegetation imply something analogous to vitality in animal bodies, it must mean, in all probability, a mode of being, not a distinct essence, and cannot, therefore, be a cause of fever. It may be presumed, then, that the cause which moves vegetation, if applied to the production of yellow fever, must mean something given out by vegetable matter, either in growth or decay. Doughty, on the other hand, would seem to derive it from the earth itself—calling it “a principle in nature,” from which vegetation is produced, and which, through the effect of continued high temperature, become concentrated and accumulated, and engender a power which in the extrication of that principle is capable of producing fever (p. 207). But whether it means one thing or another, it is objectionable, and must have appeared so to Mr. Doughty himself; for he acknowledges that marsh and other miasma resulting from the decomposition of vegetable substances may produce the same order of fever. If by principle of vegetation is meant something given out by vegetable matter, the theory is at fault, if applied to paludal fevers generally, and more particularly to the yellow fever, for the extent of prevalence and severity of the disease is not proportioned to the extent and luxuriance of vegetation in any place—fever occurring, even severely—where there is little apparent vegetation, or where the season of vegetation has passed. The explanation offered of the occurrence of the disease in such localities is far from satisfactory; for if the place is rocky, barren, and with-

out vegetation, whence can the principle be derived? On the other hand, the fever in some places is not very rife—it is even absent—in localities where vegetation is luxuriant, or after having been so, has reached the period of decay.

The history of our yellow fever affords us numerous examples of the disease breaking out and prevailing in localities where there was no vegetation at all, or where there could be none, and where, therefore, there could not exist anything capable of eliminating the principle in question. If the latter is supposed to proceed from the earth, we cannot explain the production of fever in barren soils and in such localities as are usually visited by the yellow fever—in ships, for example; for in these there is no vegetation, or the possibility of it, and it is impossible to suppose that nature would place the principle of vegetation, and set it in operation in places where she has withheld or removed the elements required for the development of the process; while, in that supposition, all places would be equally liable to fever.

To affirm, as has just been done, that the yellow and paludal fevers generally are all the products of organic, and especially vegetable decomposition, at the same time to disbelieve the identity of the former with the latter, and to regard them as the offspring of different causes, would seem contradictory, did we not bear in mind the probability that the decomposition of different vegetable material must give rise to products of different character, and that these, when applied to the living system, must produce different though kindred morbid effects. Many facts, indeed, lend support to this belief, and justify the conclusion that the yellow fever, properly so called, is due to the influence of ligneous decomposition, aided in many places, though not necessarily, by peculiar characters of soil; while remittent and intermittent fevers are ascribable to the decomposition of herbaceous matter, the ordinary component of marshes and paludal localities.

Dr. John Wilson,¹ who has more particularly called attention to this circumstance, remarks that the yellow fever is peculiarly rife, in the West Indies, in places where lime of secondary formation is a predominating ingredient—provided the more palpable cause of the disease exists in the vicinity. This cause he views to be furnished by wood, being a gaseous product of trees and shrubs in a state of decomposition. It is generally given out by them in a cut or dried state, but may arise from a living forest; trees being capable, in different parts of their frame, of simultaneous growth and decay (p. 139). Indeed, wood, after it has passed from the green to the dry state, is still capable of generating the poison; certain degrees of heat, and a certainty quantity of moisture being supplied. These views may be illustrated by a reference to those instances in which the disease has spontaneously appeared and prevailed on board of ships, often far beyond the influence of any agency derived from land, or the introduction of a contagious principle (p. 139).

Dr. Wilson refers to the case of the *Rattlesnake*, of the *Lively*, *Isis*, *Pylades*, *Ferret*, *Iphigenia*, *Seont*, *Bustard*, and concludes that the disease in them arose from the decomposition of the wooden materials of the ships

¹ On West Indian Fever, pp. 127, 138, &c.

themselves (p. 152), and from such loose timbers as they contained. The conclusion may be further sustained by a reference to other facts. The instance of the *Regalia*, cited by Dr. Ferguson himself as an example of the production of the disease by the effluvia of green wood—that of the *Vestal*, described by Dr. Bryson, in which the people on board were taken with the disease in an aggravated form from exposure to the same cause—that of the *Messenger*, cited by Roehoux, and in which the mischief was caused by a cargo of mangroves—of the *Priamus*, in which it arose from a quantity of chips and shavings; and many others of similar kind, some of which have been already cited, are in point. So are instances of the fever produced by the decomposition of spars, logs, and trees on shore. The fever of Gallipolis, in 1796, was satisfactorily traced to the decomposition of a quantity of trees piled in a ditch near the infant settlement, and only partially covered over with earth.

Dr. Wilson has called attention to the character of our wharves, and very justly attributes to them a considerable share in the production of the fever among us. “Those wharves, &c. are wooden structures, and their materials, in seaport towns, are therefore undergoing decomposition. The rise and fall of the tides, and great heat, will hasten the process; and as those structures fail, and are repaired, timber in various conditions of greenness and dryness, health and decay, will be employed in their composition; hence, gaseous products, capable of inducing fever, will be disengaged. The materials are always in existence, ready to be acted on by high temperature, and other agents, perhaps, of which we are ignorant” (p. 169).

In various localities in tropical climates where the fever prevails extensively, ample sources of ligneous exhalation are easily discovered in mangrove marshes or woods by which they are surrounded. “These plants grow in sand, either covered to some depth, or completely wet with salt water. Where they terminate seawards, they are immersed some way up the stems, and there they are most vigorous, and acquire the largest size; as they recede towards the dry sand, they diminish in bulk, and they soon die when the water fails.” They monopolize the soil on which they grow, scarcely any other plant existing over the whole. “The mangrove grows and runs to decay rapidly. The branches, after rising a few feet, bend towards the root, on which they engraft themselves; from the bow thus formed other branches spring up, which in like manner insert their extremities into different parts of that from which they grow, and so on till an impenetrable forest is spread over the surface of the water. While the lower part of this *water forest* is undergoing rapid decomposition, the upper part is in a state of luxuriant growth and beautiful verdure, so that the appearance of the whole is singular and striking; it looks like a piece of basket-work supporting a shrubbery. From its structure and habits, the mangrove appears destined to speedy decay, to which surrounding circumstances are highly favourable. In consequence of the rise and fall of the tides, parts of it are alternately wet and dry, and the whole is exposed to the influence of high temperature; hence, whatever deleterious product is furnished by decomposing wood, must be

furnished abundantly here." In all this there is nothing like a marsh, in the common sense of the word—nothing by which the exhalations of marsh can be produced (*Ib.*, pp. 163–4).

The views here set forth recommend themselves in many ways. The facts adduced in their support furnish evidence amounting, so far as they go, to prove that ligneous decomposition is, if not the invariable, at least a frequent, and, in many places, the only cause of the disease. It will be found, besides, that the material of the ligneous origin is uniform and constant—differing, in this respect, from other assignable causes; for whenever, as Dr. Wilson remarks, "West Indian fever has appeared, wood, in some condition, has existed either in its natural soil, or in the structure of ships, wharves, houses, or otherwise; and, therefore, if to the other causes of decomposition there be added the peculiar influence known only by its effects, which is here assumed to be essential, in some way, to the production of the disease, the disease may be expected, as the source of its more palpable cause will not be defective." And he adds, "that if, as has been proved in some instances in the West Indies, the fever is occasioned by the invisible products of wood alone, and it is found that wood, and nothing else, constantly coexists with the disease, such constancy of coexistence is at least presumptive evidence of its operation" (pp. 170–1).

The ligneous theory, which has been adopted, with more or less reserve, by other West Indian writers,¹ had been to some extent sustained in this country, and illustrated by facts long before the appearance of the interesting volume of Dr. Wilson. In his *Medical and Physical Memoirs*, published a half century ago (1801), and in works of a more recent date,² Dr. Caldwell, in enumerating the sources of morbid effluvia in this city, allots a conspicuous place to our docks and wharves, and remarks that the timber of which they are built, running constantly into a state of dissolution, is itself an abundant source of pestilential exhalation (p. 105). The epidemic of Baltimore, in 1819, was not improperly attributed to a similar source of infection. Of Fell's Point, where the fever prevailed, it is said that a large quantity of vegetable matter had been used for filling in the wharves. This consisted of pine cord-wood, pine tops, old barrels, chips, shavings, &c. "The pine wood constitutes the great body of this article, and will be liable to generate miasma for a series of years, whenever a season such as favours its generation occurs. There is no prospect of this wood becoming innocent until it is all dissolved and its place supplied by earth."³ This view of the source of the miasm was adopted not by one of the physicians consulted on the subject, but by many. Wharves, timber and wood, in large or small fragments, have been recognized as the cause of similar effects in other cities of this country, and especially at Mobile during the severe epidemic of 1819.⁴ Dr. Dickson in like manner

¹ Wallace, *Edinb. Journ.*, xlv. 274; McKinnel, in Bryson, p. 54; *Ibid.*, *Med.-Chir. Rev.*, xiv. 40.

² Malaria, p. 96.

³ Letters on the Yellow Fever of 1819, pp. 101, 107, 109, 132, 193.

⁴ See *ibid.*, p. 200, for report on that fever.

calls attention to the fact that the wharves of Charleston are built chiefly of wood; palmetto and other logs forming the framework or outline, which is filled up with rubbish and mud drawn by machinery from the shallow docks which they embank.¹

The epidemic of Savannah was attributed in a measure to the number of wooden houses, unpainted and in a state of decomposition.² And although at New Orleans wharves cannot be said to furnish the materials for the elimination of miasm, the swamps and woods which adjoin the city are evidently the seat of ample ligneous decomposition; while the humid soil upon which the city is built contains masses of wooden materials, in the form of planks, beams, piles, &c., which serve as support to the foundation of the houses. Nay more, long before the time of Dr. Wilson, Baron de Humboldt invested the theory in question with no small share of importance, by calling attention to the large masses of mangroves and avicennas with which the sea-coast and the river banks in the tropics are covered, and ascribing the fever of those localities to the rapid alternate growth and decay which those plants undergo. To this cause, in great measure, he refers the insalubrity of Vera Cruz, where are to be found a number of marshes or lagunes filled with those plants, leaves, roots, fruits, &c., all of which, after being covered with water during the rainy season, become subsequently exposed to the action of the sun. The experiments made by him at Cumana, on the action exercised on the ambient atmosphere by the roots of the mangrove, when, while slightly humid, they remain exposed to the light, confirm the observations made in the two Indies, that, in all places where the mangrove thrives luxurantly, the most insalubrious are those where these plants are not constantly covered with water.³

With regard to the agency of a lime soil in aiding the development of the miasmatic poison, I need not say much. That such a soil is not essentially necessary to the production of that poison, is a fact which admits of no doubt; for instances might easily be found of the fever appearing—in its worst form, too—where lime did not exist; and whenever the disease breaks out and prevails on board of ship, the agency in question cannot be referred to. Nevertheless, when we find, on examining the geological structure of the West India Islands, that lime, of secondary formation, is a predominant ingredient in the soil, mixed with much volcanic matter—covered, in the cultivated parts, with a stratum of clay, forming a chalky marl; when we learn that the Campagna di Roma, Gibraltar, many parts of Spain, and the Ionian Islands present much the same character; and recollect, at the same time, what has been said of the structure of our Atlantic plain—when, I repeat, we notice the existence of a lime soil in so many places where fever is most rife, and its absence in places where the latter does not prevail, it is difficult to withhold the conclusion that a calcareous soil, combined with moisture, which the insalubrious spots possess either on the surface or beneath, must, when present,

¹ Am. Journ., ii. 67.

² Waring, p. 40.

³ De la Nouvelle Espagne, p. 763; Personal Narrative, iii. 190, 371, 392.

exercise the agency contended for. This is consistent with the well-known power of such soil to attract moisture, of its higher degree of temperature, and of its consequent greater tendency to hasten the decomposition of vegetable and other organic matter.

To these circumstances must be referred a fact well known to all farmers, and to which Dr. Williams points, that "carbonate of lime, mixed with the ordinary matters of a compost, greatly forwards the process of putrefaction, so that the mass thus prepared is fit in much shorter time for the purposes of manure" (ii. 434). The greater power of absorption possessed by a lime soil has been illustrated by Sir Humphrey Davy, who found that pulverized chalk absorbed eight-tenths (times?), and clay two and a half times their weight of water. The difference of that absorption, and its effects on vegetable products, are visible in various counties of England, in Derbyshire, North Wales, as, indeed, in many parts of this country. Such being the case in localities where the atmosphere contains a limited quantity of vapour, it must be greater in tropical regions, where the quantity is three times as large; and during the rainy seasons of other localities, when it is also very considerable. This moisture, aided by the greater heat imparted by lime to the soil, and the high additional heat it receives from the sun, accounts for the more certain and rapid decomposition of the vegetable remains contained under or on the surface of such soils, and consequently for the more abundant extrication of miasm—paludal in marshy localities, and concentrated in places containing the material furnishing the poison of the yellow fever.

Whether animal effluvia are ever conjoined to vegetable in the composition of the poison giving rise to the yellow fever in this and other cities of this country, in Europe, and in tropical regions, and whether the former, when present, adds virulence to the latter, are questions to which I am not prepared to offer a definite answer. That, alone, the effluvia arising from animal decomposition, though doubtless productive of disease, and, among those, of some forms of febrile complaints, has, nevertheless, never been positively shown to be adequate to the production of the poison of yellow fever, I have already pointed out; while the disease has often appeared under circumstances which indicate its origin from vegetable decomposition singly. But from this it does not follow that the former, when present, may not increase the virulence of the products of the latter. It is a fact too generally known to need illustration, that in most localities visited by the yellow fever, either habitually or at distant periods of time, animal remains are found in greater or less abundance, and exhibit, under peculiar meteorological conditions, evident signs of decomposition. This is found to occur along the whole of that part of the coast of Mexico where the fever is rife. It is seen in St. Domingo, the West India Islands generally—in fact, all over the insalubrious sections of tropical regions. In the yellow fever districts of this city, as in those of New York and other American cities, from Boston to Galveston, the materials of decomposition to which the disease is ascribed consist in part of, and are mixed up with, animal remains of various kinds. And from what we know of the deleterious effects on the human system of the effluvia arising from these

when unmixed, as well as from a recollection of the phenomena produced by the introduction of putrid animal substances into the circulation, and of the effects of confining animals in an atmosphere highly impregnated with animal effluvia, we can with difficulty suppose that, when mixed with the products of vegetable decomposition, such effluvia can remain perfectly innocuous, and leave the morbid agency exclusively to the latter. The propriety of this supposition will appear the more doubtful when we bear in mind that, however widely animal and vegetable effluvia may differ in many respects, they approximate—some of them, at least—on other points, and especially in regard to some of the elemental principles of their composition. These, being the same in both classes, must give rise, during their decomposition, to results of an analogous kind—all other circumstances being similar; and we are thence justified in supposing that the effluvium furnished by animal substances, though not adequate alone, from peculiarity and proportion of combination, to the production of the poison, may lend force to the action of that furnished by the other class of materials.

Epidemic Constitution of Atmosphere.—Whatever be the nature of the morbid agent producing the yellow fever, or the materials from which it is derived, and however effective this agent may be, when the requisite meteorological conditions are present to generate the disease, certain is it that its existence cannot alone suffice to account for the epidemic diffusion exhibited by the latter at certain periods. Every one who has investigated the subject of the history of the yellow fever—and the same remark applies equally well to other diseases which take on the epidemic garb, whether contagious or otherwise—knows that while, in some seasons, the number of cases is limited, the disease prevailing within very circumscribed boundaries; making slow, if any progress, and assuming either a sporadic character or one scarcely deserving the name of epidemic; in other seasons, marked by meteorological conditions and peculiarities of locality differing but little, if at all, from those exhibited in the former instances, the fever originating from seemingly small beginnings in one or few spots of circumscribed extent, soon spreads far and wide—rages fiercely and fatally—covers in a short time an enlarged surface of ground, and there attacks a large number of individuals—in a word, assumes the character of a wide-spreading pestilence. The history of the disease shows, as has been pointed out in various parts of the present work, that in most of the cities of this country epidemics of the disease have appeared at intervals of greater or less extent—breaking out violently and spreading widely in some seasons, and sparing the same places for several and even many successive seasons—the meteorological phenomena and sources of infection remaining the same.

At some periods the disease is not limited to one city, but appears in several or many. This was the case in 1839, when it prevailed in most of the seaports of the Mexican Gulf. In 1853, it invaded most, if not all, the West India islands, the coast of Mexico, and a large extent of the gulf coast. It extended far up the Mississippi and its tributaries, attacking villages and plantations which heretofore had been complete strangers to it; visited the

village of Brandywine in the State of Delaware, and showed itself in this city. We have seen that the same results have been frequently observed in the West Indies and other parts of tropical regions. In 1798, it prevailed in Boston, New York, Portsmouth (N. H.), Newport (R. I.), New London (Conn.), Philadelphia, Wilmington (Del.), Charleston, &c. In 1819, it showed itself in Leon, Cadiz, Seville, Xeres, Port Sta. Maria, and other parts of Andalusia, in Minorca, Cuba, Jamaica, Martinique, Guadaloupe, New Orleans, Charleston, Baltimore, Philadelphia, New York, and Boston.

We have seen, also, that summer and autumnal diseases, generally, in whatever section of country they have appeared, have, with few exceptions, assumed a more severe character, or prevailed more extensively in the vicinity of the infected locality—or over an extensive tract of country—at the time of, or prior to the epidemic visitations of the disease in commercial and other cities of this country, Europe, or the West Indies. We have seen, besides, that the character of the disease varies in different seasons—being inflammatory one year and typhoid another; and, finally, we have noticed that not unfrequently epidemics of yellow fever have been preceded by a prevalence of disease among the lower orders of animals, plants, &c., and that at distinct periods the disease is modified by the implication of various organs.

This diversity in the power of diffusion and in the character of the phenomena is noticed in smallpox, scarlatina, and other kindred complaints; in remittent and intermittent fevers; in dysentery, typhoid, and typhus fevers, &c. At certain periods, the cases are scattered, and comparatively few in number; at others, they are quickly multiplied, and become more or less generally diffused. Like the yellow fever, other epidemic diseases are at times of an inflammatory, at others of a typhoid character; while, during the existence of a wide-spreading epidemic, all reigning diseases receive a peculiar imprint from the latter and assume some of the pathological characters which appertain to it. Nor is it less true that certain diseases, including, under peculiar circumstances, some of the exanthemata mentioned, while to a certain extent under the controlling influence of particular conditions of localities or vicissitudes of temperature, appear to derive their origin from a general atmospheric influence, the existence of which can only be inferred by its effects. But this influence, which has received the name of epidemic meteoration, must be distinguished from that above mentioned, inasmuch as the former conveys the poison of the disease—as of cholera, influenza, mumps, hooping-cough, &c.; whereas, the latter consists of a particular influencing agency, which facilitates the diffusion, or increases the severity of a local cause or contagious virus, either by promoting their generation and aggravating their virulence, or by increasing the susceptibility of individuals exposed to their morbid impression; and which, besides, modifies the character of the prevailing disease.

This modifying agency is invisible, and widely spread, and owing to its depending apparently on some peculiar alteration of the surrounding air, has received the name of epidemic constitution of the atmosphere. In what it consists, and to what it is due, are points which so far have not been positively

ascertained. Its existence, like that of epidemic meteorations just alluded to, is known by its effects. It has been noted from the days of Hippocrates, who attributed pestilential diseases to some unknown or divine principle—*quid divinum*. The same attention to these effects was paid by Diemerbroeck, who, apparently, derived his *seminarium e cælo dimissum*, from the divine principle of the Coan physician. Van Swieten, Stoll, in Germany; Lapecq de la Cloture, in France; Sydenham, Mead, Huxham, Hancock, in England; Rush, Webster, Caldwell, Chapman, Smith, Jackson, Barton, in this country; Dariste, Arnold, Wilson, R. Jackson, in the West Indies, not to mention many other authorities of more or less note, have dwelled upon and illustrated this modifying agency—some attributing it to a mysterious influence of the heavenly bodies, especially comets or the moon; to earthquakes and volcanoes; to an altered condition of the atmosphere; to a rapid propagation and migration of animalculæ or fungi; to some hidden or occult qualities derived from exhalations, however produced, from the bowels of the earth.

One of the latest writers on the subject informs us that what is meant by an epidemic atmosphere is the presence of certain elementary constituents, or their combination differently from the habitual or normal condition. We have, according to him, no proof of anything *specific* beyond the combination, and this is twofold: the meteorological part, probably forming the predisponent, is innocuous without the other—it is but one blade of the “shears;” the second is the local circumstances and influences—the true localizing power.¹

But most of these explanations are purely hypothetical—some are fanciful; and though the effects in question may probably be referable to the perversion or distempered condition of certain unknown influences—atmospherical, terrestrial, or both—the sound condition of which is essential to insure a healthy state of the vital functions; though they are probably, in some measure, connected or associated with a peculiar condition of the electrical element of the atmosphere—which imparts to the latter a tendency to hasten the decomposition of the materials of infection, or dead animal and vegetable matter generally, recognized in all epidemic times—we must rest satisfied, so far as regards the disease before us, and every other of an epidemic kind, with the fact of the existence of a secret power residing in the atmosphere, and which, under certain contingencies, operates injuriously on all individuals exposed to its influence, promotes the generation of morbid poisons, and imparts a special character to the prevailing disease. With respect to Dr. Barton’s explanation, we may remark that it applies only to an infected atmosphere, rendered so by exhalations evolved from local sources of impurities, aided by certain meteoric conditions of atmosphere; in other words, simply to the cause of the disease, and not to the peculiar something which promotes the diffusion and influences the degree of force and malignancy of that cause, and the susceptibility of the system to receive its impression.

¹ Barton, Rep. of the Sanit. Commiss. of New Orleans, 1853, p. 263.

CHAPTER XXXIII.

TREATMENT.

As may very readily be presumed, the method suggested for, and pursued in, the treatment of the yellow fever has greatly varied, not only in the different regions where the disease has made its appearance, and at different epochs of its history, but also in the same locality, at different seasons and in the same season, according to the views entertained respecting its etiology, pathology, and tendencies, and the efficacy and mode of operation of therapeutic agents. By those who advocate the opinion of the inflammatory character of the fever, or in places where and seasons when that character manifests itself, a treatment appropriate to the removal of that morbid condition is recommended—often to the exclusion of every other. Another class of physicians, who can see in the disease nothing but, or little else than, the manifestation of a typhoid, or putrid, morbid element, an opposite course is preferred and lauded. Again, others who believe in the bilious character of the fever, and in the identity of the latter with bilious remittents, turn their attention principally to means suited to correct the deranged condition of the liver, and to evacuate the morbid secretions of that organ; while the exclusive partisans of the gastro-enteritic doctrine have almost solely an eye to the employment of remedies useful in eradicating the local complaint, and to the avoidance of medicinal and other substances which they suppose are calculated to keep up or aggravate the inflammatory irritation of the diseased mucous membrane.

But whatever be the views adopted in relation to the nature of the fever and the plan of treatment pursued, experience, here and elsewhere, has clearly shown that, at all periods of the epidemical recurrence of the disease, individuals labouring under it may, in a therapeutical point of view, be divided into three distinct categories: 1. Those in whom the poison has produced a deadly impression—who, to use the expression attributed to Magendie, begin to die from the first moment of their attack, and in whom no treatment can avail; the few that recover, doing so by the power of nature, not through the instrumentality of art. 2. Those who are so mildly affected as to recover spontaneously, without any treatment, or under the influence of any rational, and even often irrational, management. 3. Those in whom the disease assumes an intermediate grade, whose chances of life or death are equally or nearly balanced, and in whom, consequently, it becomes an object of the utmost importance to apply means calculated to produce a favourable impression on the train of morbid actions, and thereby arrest their dangerous tendencies.

From this it naturally follows that, in all that has reference to the *methodus medendi* of the yellow fever, the physician must have in view the discovery of means appropriate to assist the efforts of nature in the cases composing the

first category, and of remedies likely to prove useful in the management of those of the second and third. In other words, he must seek the discovery of means which, if not strictly necessary in the first class of cases, may at least prevent them from assuming a dangerous aspect; and of remedial agencies, which, in other cases, will, by inclining the scales in a favourable direction, assist in rescuing the patient from his perilous condition.

I scarcely need remark, after what has been said in preceding chapters relative to the several forms assumed by the disease under various contingencies, to the opposite morbid conditions of the system at large, as also to the individual organs and tissues implicated in each of those several forms, and to the various degrees, in point of violence, observable in these, that the treatment appropriate to each will vary considerably. As a natural consequence, it must follow, also, that no inconsiderable divergence, in the same respect, will arise from the age, constitution, sex, and race of the individual attacked; from the nature of prevailing diathesis and epidemic constitution; in a word, from all the diversified contingencies, general and personal, which tend to modify the force and character of the disease. It is evident that if the treatment must, from such causes, undergo marked modifications, the decision of the physician respecting the appropriateness of a particular plan in the treatment of the fever during certain seasons, or in the management of some individual cases, cannot always be based upon the results obtained from the same plans under all other circumstances, inasmuch as what has suited in the one may prove unsuccessful in others. The same diversity is observed in other diseases. A distinguished writer, after remarking that there is no doubt a material difference in the type of the fevers of France compared with those of Britain—a greater tendency to gastric irritation, and to those changes in the mucous membrane of the bowels which result from inflammation—adds: “We do not, therefore, wonder at the proscription of purgatives by the French physicians, and their extravagant denunciation of the British treatment of fevers. The fact seems to be, that if British physicians were called upon to undertake the treatment of fevers in France, they would be less lavish of their purgatives; and, on the other hand, after the French physician had seen the character of the fever in this country (England), and thrown aside his preconceived notions and scholastic prejudices, he would acknowledge the utility of the judicious administration of purgatives in the treatment of the fevers of Great Britain.”¹

To come nearer home: the common inflammatory diseases of our country are subject to like modifications respecting the indication of cure, as they appear in different places, and at different times in the same localities. Our physicians know full well that the autumnal and winter diseases, with which they are so familiar, call in some years for more active depletion than in others; and that bleeding, emetics, and the like, which are indispensable at one time, cannot be carried far, or are even inadmissible, at another. It is fresh in the recollection of us all, that, a few years back, a great mortality ensued

¹ Tweedie, *Cyclop. of Pract. Med.*, ii. 200.

from what was denominated typhus pneumonia, in which, though the lungs were engorged and inflamed, depletory means could only be used very sparingly; that in many cases they could not at all be resorted to; and that not unfrequently the powers of life were obliged to be sustained, from the outset, by cordial remedies. Some forms of gastro-enteritic inflammation, complicated with meningitis, sometimes call for the same mode of treatment; and the difference, in the same respect, presented by scarlatina, measles, and other eruptive diseases, influenza, &c.—requiring active depletion in one year, and a milder, or even a tonic or stimulant treatment the next—is familiar to the profession.

Nor would it be difficult to show, from the records of the epidemics of yellow fever which have prevailed in this and other cities of the United States, as well as in foreign countries, within and beyond the tropics, that a similar necessity for a modification in the method of treating the disease has existed at various periods.¹ The famous epidemic of Grenada, in 1793, described by Chisholm; that of Brimstone Hill (St. Christopher), mentioned by R. Jackson (*Sketch*, i. 17); that of Trinidad in 1816 (McCabb, *Ed. Journ.*, xv. 33); of the Childers, an account of which has been given by Birnie (*Ed. Journ.*, xiii. 337, &c.); of Dominica in 1838 (Imray, *Ib.*, liii. 85), required evidently a treatment differing from that found beneficial on other occasions. According to Rochoux, the fever of Guadaloupe called for a mode of treatment which subsequent experience taught him was inappropriate to the epidemic he witnessed at Barcelona; and though, for reasons of his own, he chose to regard the two diseases as specifically distinct from each other, the better informed and clear-sighted reader will be disposed to attribute the result to a different cause.

The same may be said of Chisholm, Pym, Stevens, and others, who, basing their views on a difference of therapeutical indications, and on circumstances connected with the supposed mode of propagation of the fever, under the control of peculiar influencing contingencies, established a line of demarcation between the yellow fever of the African coast and the ordinary form of the disease as it prevails in the West Indies.

In fact, after all that has been said on the subject of the modifications which the yellow fever undergoes under the influence of various circumstances, we ought to be prepared to find it calling, at times, for a treatment more or less different from that appropriate to its management at other periods and in other cases. The only thing calculated to astonish the prudent, judicious, and clear-sighted inquirer in this matter is, that a fact so plain and so positive should not have been borne in mind and acted upon by medical writers generally, a number of whom are but too much disposed to make the application of their own personal experience, obtained often in a circumscribed

¹ Rush, Fever of 1793, 1798; Rogers, Fever of New Orleans, in 1824; New York Med. Journ., iii. 384; Chatard, Med. Rep., iv. 254; Selden and Whitehead, Med. Rep., vi. 251; C. Drake, Repos., xxi. 137; Ramsay, *ib.*, xi. 234, 405; Johnson's Oration before Med. Society of South Carolina, in 1807; *Ib.*, xi. 405; Fever of New Orleans, in 1839, p. 233.

sphere of observation, to the disease generally—regarding their favourite mode of treatment as suitable in all cases everywhere, and under all circumstances—casting censure, or showering ridicule on all who happen to view things in a different light, or expressing doubts as to the genuine nature of the cases noticed by their opponents, or even to the truth of their statement.¹

The one advocates active depletion by the lancet, and measures the blood abstracted not by ounces, but by pounds, looking with contempt on all who prefer a less energetic method. Another dreams of nothing but mercury, and would salivate all cases. A different writer preaches the necessity of free and profuse purging, and attributes the large mortality of the disease to a neglect of that indispensable means. Another, again, holds that neither bleeding, purging, nor mercury has ever done, or can ever do, good, and strongly insists on the propriety, in all cases and under all circumstances, of administering the Peruvian bark in large doses. Some insist on the necessity and possibility of cutting short the fever by means of sulphate of quinia, and accuse those who call for proof of the success of the *abortive* method with being behind the times. Some discard every method heretofore suggested, and aver that the true plan of treating the yellow fever is to oxygenate the blood by means of neutral salts. These various plans, and others equally exclusive, on the saying of their promulgators and advocates, are applicable to the disease at all times and in all climes—they seldom fail.

The impropriety of such exclusiveness has been more than once pointed out and insisted upon, not only by practitioners whose experience in the disease enabled them to perceive its unfavourable tendency; but also by others who, though not conversant with the disease from personal observation, have examined the subject extensively, and with due attention. “On a cursory glance,” says Dr. Good, “these diversified modes of treatment appear in many respects to be hostile to each other, and to establish an utter absence of any one therapeutic principle common to the whole; but a closer attention to the subject will show us that there is not necessarily any *opprobrium medicorum* in the discrepancy, except what results from becoming so exclusively the champion of any one of those respective modes of treatment as to bend every case to its own limits, and thus convert it into a bed of Procrustes; for there seems to be abundant reasons for believing that in different situations, or under different circumstances, each of those plans have proved equally judicious and successful.”²

It is evident, then, that experience teaches the necessity of modifying the treatment of the yellow fever according to a variety of circumstances. But, while the propriety of such modifications is placed beyond doubt, experience teaches, also, that, notwithstanding all that has been written on the subject, and the ample opportunities for observation afforded by innumerable epidemics, our progress within the last three-quarters of a century, to-

¹ Dubreuil, Journ. Univ., viii. 323; Bally, 556; Wilson, p. 6; H. McLean, p. 113; Wallace, xlv. 277.

² Study of Medicine, ii. 184–85.

wards anything like a satisfactory treatment of the disease in its various formidable shapes, has been far from gratifying. The fever, when severe, continues to produce its usual havoc; and on comparing the remedial plans in vogue now-a-days with those suggested by our forefathers, we do not find them to differ materially; or if they do, to be attended with much better success. The disease being the product of a poisonous contamination, it might be expected that the object of the physician would be the discovery of an agent calculated to neutralize the morbid operation of the cause and to act in the capacity of an antidote—thus endeavouring to realize in relation to this fever the prediction long ago hazarded by Sydenham, and often since repeated, that the day would come when a specific remedy would be found for every disease.

That individuals, in and out of the profession, have entertained a hope to that effect, or have even fancied they had made the discovery, is true; but I presume it is unnecessary to remark that such expectations have not as yet been, and are not likely to be, realized, in our days at least; and that in encountering this formidable disease, we must content ourselves with endeavouring, not to neutralize the poison circulating in the system, but to correct the morbid effects it occasions on both solids and fluids. We must, while watching carefully the course of the disease, prevent undue mischief from being done, especially to organs essential to life. We must keep these organs in as healthy a condition as possible—restore, if possible, equilibrium in the play of the functions—reduce undue and dangerous excitement, general and local, and sustain the powers of life when these threaten to become impaired, or are already reduced beyond the point of safety. But we are forced to confess that, beyond this, art is of little avail. The idea of *curing* the disease, or greatly abridging its course, is entitled to little confidence. To nature must be left the chief management of the case; time must be allowed for the elimination of the poison; and the physician must be impressed with the conviction that, in cases where no marked organic mischief has been done, or is likely to occur, he must keep his hands off as much as possible, and restrict his agency to the employment only of such means as are strictly necessary to fulfil particular indications. He must not attempt to do what is more safely done by the recuperative powers of the system, and rest assured that in these, and indeed in all instances, more danger is to be apprehended from too great than too little interference on the part of the medical attendant.

No one can doubt the propriety of basing our practical method on broad pathological principles; but in fevers—and in the yellow fever, perhaps, more particularly—the process is beset with difficulties: for though the disease expends its force on particular and important organs and tissues, and though the main object would appear to consist in restoring these to their pristine condition, or to place them beyond the reach of disorganization, yet, in doing so, we do not attack the other elements of the disease—we do not eradicate the immediate cause of mischief, which, if sought at all, will be found placed beyond a condition of parts which, however frequently it may present itself, does not do so invariably, and is sometimes the effect of in-

flammation, at other times of congestion, combined or not with nervous irritation. Even were this not the case—even were we, while combating these local derangements, to strike at the root of the disease, and ignore phenomena of secondary import in a pathological point of view, and which sometimes are absent—it would be found that the case is often so rapid in its course, the affections of particular organs so obscure, the life-force of the system so deeply involved, and the strength of the patient so exhausted, that we have nothing tangible to seize upon, or if we have, the opportunity of producing a proper remedial impression has escaped, and we may well hesitate to make the attempt, from the fear of robbing the system, without adequate compensation, of the small amount of resisting force it continues to possess. Such being the case, we are forced to content ourselves with treating the prominent symptoms as they present themselves, and leave the rest to the recuperative powers of the system.

It has been properly remarked by an intelligent writer, whose residence in a city frequently visited by the yellow fever gave him ample opportunities of observation, that of all the diseases which afflict the human race, there is none that requires more unremitting care and attention on the part of both physician and nurse, than the one before us. “Accidents, or acts of imprudence, which, in other diseases, are mere trifles, are of tremendous importance in this. The mere getting out of bed has cost many a man his life. Exposure to cold currents of air, or negligence to take the requisite precautions against a change of the weather, has been equally fatal. A man in this disease, however safe the physician may think him, is hovering between life and death—a trifle may decide his fate.”¹

To guard as much as possible against all such disturbing influences must be an object of solicitude on the part of the physician. He must see that the patient is confined effectually to his bed, and prevented from rising. He must give proper directions for the free ventilation of the apartment, and the preservation of cleanliness; while, whatever be the form the disease assumes, he should proceed to the medical treatment of the case with the least delay possible. The great rapidity with which the dangerous symptoms make their appearance, the little time afforded for preventing their onset, and the great importance existing of effecting that object—besides the difficulty of their removal when they do appear—render such promptness imperative. At the same time, a knowledge of the insidious and treacherous nature of the disease, the great difficulty of predicting whether symptoms indicative generally of a mild attack are not soon to be succeeded by those of an opposite kind, and whether changes calculated to make us hope for a favourable issue are not to be followed—perhaps in a few hours—by others portending approaching death, should make him constantly attentive to the nature and succession of every phenomenon.

Attention being paid to these details, the medical treatment will necessarily vary according to the particular form which the disease assumes. In the

¹ Harrison, N. O. Med. and Surg. Journ., ii. 322.

several varieties of the inflammatory form, recourse must be had to antiphlogistics, sedatives, and evacuants, graduating the energy of these to the degree of violence of the reaction, the force of the circulation, the heat of the skin, and the extent of the local inflammations or congestions. Of the indispensable necessity of the antiphlogistic and evacuant treatment, which in this, as in other fevers of kindred nature, consists in sanguine evacuations, sedatives—internal and external—and purgatives, there can be no doubt. It is based on the evident character of the complaint, and the success which has attended its application; and comes to us under the sanction of innumerable and high authorities. In this city, after a short period, when the tonic and stimulant plan was pursued by many practitioners—from the then existing idea of putridity, and at the suggestion of Dr. Stevens of St. Croix, but at the time residing here—the antiphlogistic method was adopted by Dr. Rush, and almost every one else. By the resident French physicians—who, let it be said, had never adopted the tonic treatment—depleting, sedative, and evacuant means were employed; and from that day to this, the same mode of treatment has been regarded as the most appropriate, under certain circumstances, and with certain modifications.

In other parts of this country the antiphlogistic treatment, in greater or less purity, was early adopted, and continues to be pursued by a large number of physicians. In tropical regions, too, antiphlogistics have no less generally been used, from the days of Towne to our own; and in Europe, though they have experienced more opposition—particularly on the part of Spanish physicians—they nevertheless can boast of numerous and highly respectable advocates.¹

¹ Rush, iii. 170–71; Currie, *Fev. of 1793*, p. 40; *Ib.*, 1799, p. 86; Barnwell, pp. 392, 399; Beugnot, *N. O. Journ.*, i. 23; Lawson, *Stat. Rept. U. S. Army*, p. 267; McCormack; Monges, *N. Am. Journ.*, ii. 63; Caldwell, *Epid. of 1803*, p. 189; *Ib.*, *Epid. of 1805*, p. 103; *Ib.*, *Mem. (1800)*, p. 216; *Ib.*, *Mem. (1826)*, p. 154; Drysdale, *Med. Mus.*, i. 130, 241; S. Jackson, *Epid. of 1820*; *Ib.*, *Am. Journ.*, xiv. 41; Hosack, *Med. Essays*, iii. 437; *Ib.*, *Practice*, pp. 391, 392; Miller, *Works*, p. 74; Rand, *Med. Repos.*, ii. 474; Vaughan, *Med. Reposit.*, iv. 241; *Ib.*, *Fev. of 1802*; Seaman, *Repos.* iv. 250; Selden and Whitehead, *ib.*, iv. 336; *Ib.*, vi. 251; Davidge, *ib.*, ii. 89; *Ib.*, *Essay*; S. Brown, *Med. Reposit.*, iv. 62; Chatard, *Reposit.*, iv. 254; Jameson, *Med. Recorder*, vi. 435; Thomas, *Fièvre Jaune*, pp. 82, 92, 101; Barton, *Am. Journ.*, xv. 41; *Ib.*, xi. 50, 51; Harris (of N. O.), *Am.* xiv. 46, 47; Archer, *Med. Record.*, v. 70; Bayley (Rich.), p. 113; Ffirth, p. 29; Warren, in Tytler, p. 503; Drake (C.), *Med. Repos.*, xxi. 137; Smelt, *ib.*, ix. 127–28; Baxter, *ib.*, xxi. 13, 14; White, *Med. Repos.*, ix. 147; Hall, *ib.*, viii. 20; Wille, *ib.*, vi. 127; E. T. Waring, *ib.*, iv. 237; Barrington, *Am. Journ.*, xii. 313; Gros, p. 20; Girardin, p. 41; Waring, *Report*, p. 63; *Ib.*, *N. A. Journ.*, iii. 1; Townsend, p. 206; Merrill, *Med. and Phys. Journ.*, ix. 248; *Ib.*, *N. A. Journ.*, ii. 225; Cartwright, *Recorder*, ix. 19; Hogg, *West. Journ.*, i. 414; Perlee, *Med. and Phys. Journ.*, iii. 1; Irvine, p. 41; Dickson, *Med. and Phys. J.*, iii. 258–59; Coffin, *Med. Repos.*, i. 504; Washington, *Med. and Phys. Journ.*, vi. 316; Ticknor, *N. A. Journ.*, iv. 1; Potter, *Notes to Gregory's Practice*, i. 151; *Rept. of Fev. of N. O.*, in 1839, pp. 337–38; A. Hosack, p. 35; Duprés, *Am. Journ.*, ii. 381, *N. S.*; Seaman, in Webster's *Collect.*, p. 42; E. H. Smith, *ib.* p. 141; Monson, *ib.*, p. 186; Ffirth, p. 29; Towne, p. 25; Bruce (in Lind), p. 280; Pouppé Desportes, i. 207; Desperrière, pp. 58, 73; Hillary, p. 157; Moseley, p. 445; R. Jackson, *Treatise*, p. 268, and other works; H. McLean, p. 164; Bancroft, pp. 60, &c.; Lefort, *De*

That everywhere great diversity of sentiment exists as to the extent to which these means—the depletory especially—are to be carried; that some are in favour of pushing them to great extent; that others are more sparing in their use; that others, again, discard them almost entirely, and that, while some use the lancet freely—others resort only to topical bleeding, are facts familiar to all medical readers. But, however this may be, as regards such depletory means, it is not less certain that it may safely be affirmed that, among the large majority of physicians in all parts of the yellow fever zone, even among those who discard sanguine evacuations, the treatment, at least at the outset, is more or less antiphlogistic, sedative, and evacuant. The very course pursued by the coloured women in the West Indies, and from which they have acquired some degree of celebrity, is, though feeble, strictly speaking, of the nature in question;¹ while by many of those who regard the disease as always from the outset of a typhoid or asthenic character, the use of tonics and stimuli is usually preceded, if not by depletory, at least by evacuant means (*Clark*).

These remarks apply exclusively to the first stage of the yellow fever; for experience in every part of the world visited by that disease has demonstratively proved that it is only during that stage, and, indeed, during the early portion of it, that depletories, sedatives, and evacuants can be used with any chance of success. Experience has proved also that, to be productive of most benefit, these means, the first particularly, must, if resorted to at all, be so without loss of time—success being proportioned to the earliness of their application. Nor is this all. Experience has, moreover, proved that, though antiphlogistics may evidently be called for in the treatment of

la Saignée, &c., p. 553; Th. Clark, p. 18; Musgrave, *Med.-Chir. Tr.*, ix. 125, 135; Ralph, *Edin. Med.-Chir. Tr.*, ii. 82; J. H. Dickson, *Edin. Journ.*, ix. 53; *Ib.*, xiii. 50; McArthur, in Johnson, p. 351; Blane, on Seamen, p. 427; Osgood, pp. 42, 43; Stevens, p. 366, *Rufz, Med. Exam.*, iii. 118, 120; *Ib.*, Chervin's Rept. pp. 37, 54; Rochoux, *Tr. de la Fièvre Jaune*, p. 423; *Ib.*, Typhus Amaril, p. 582; Caillot, p. 301; Rouppe, p. 311; Wallace, *Edin. Journ.*, xlv. 278, 281; Frost, *Med. Repos.*, xiii. 253; Blicke, in Zecchinelli, p. 207; J. Clark, p. 23; Belcher, *Edin. Journ.*, xxiii. 254; Birnie, *ib.*, xiii. 339; Douglass, in Doughty, p. 23; Macmillan, *ib.*, xx. 31; Parson, *ib.*, viii. 388; Ferguson, *Med.-Chir. Rev.*, ii. 190; Comrie, *Edin. Journ.*, xiii. 167, 177; Mortimer, *Med.-Chir. Rev.*, iii. 186; Bally, p. 516; Hunter, p. 86; Imray, *Edin. Journ.*, liii. 86; *Ib.*, lxiv. 330; Gillespie, p. 84; Maher, p. 896; Chevalier, p. 10; Stewart, *Med. Regist.*, iii. 189; Chambolle, *Ann. de la Méd. Phys.*, xii. 202; Veitch, p. 132; Dickinson, p. 151; Le Riverend, *Ann. de la Méd. Phys.*, xii. 543; Wilson, pp. 9, 26, &c.; Dubreuil, *Journ. Univ.*, viii. 323; Curtin, *Duncan Com.*, ix. 237; Grant, p. 21; Arnold, p. 38; Catel, p. 14; Dariste, p. 189; Makittrick, p. 135; Dyott, pp. 1005–6; Manson, p. 28; Dancer, p. 88 (note); Palloni, p. 13; Burnett, pp. 19, 20, 25, 131, 352, 402; *Ib.*, *Edin. Journ.*, viii. 397; Robert, pp. 122, 384, 731; Barrel, p. 15; Ferrari, *Edin. Journ.*, xix. 368; *Ib.*, *Ann. de la Méd. Phys.*, vii. 298; Amiel, *Edin. Journ.*, xxxv. 279; T. Smith, *ib.*, xxxv. 43; Gilpin, *Med.-Chir. Tr.*, v. 323–24; Proudfoot, *Edin. Journ.*, xxviii. 9, 290; Jourdain, *Ann. de la Méd. Phys.*, iv. 505; Lasso, in Rochoux, p. 647; Doughty, p. 218; Tommasini, i. 223, &c.; Rubini, p. 120.

¹ Savarésy, p. 517; Caillot, pp. 312, 314, 317; Bally, p. 541; Gillespie, p. 74; Rochoux, p. 581; Gilbert, p. 89; Bonnen and Sulpicy, p. 59.

the inflammatory form of the yellow fever, they are not, as a general rule, to be used with the same freedom as in ordinary inflammations; that, except in cases where the reaction is inordinately high, where important organs are seriously inflamed or congested, and where the patient is vigorous, plethoric, and young, and the recuperative powers of the system are energetic, it is safer to avoid the use of agents calculated to debilitate suddenly and considerably, and to trust to milder means; not only because a heroic course—too long in vogue among us and elsewhere—has not been found to succeed any better than one of a different kind in arresting the course of the disease, and to do more than afford temporary relief; but because, whatever be the means employed, a remission is generally obtained after a paroxysm of two or three days, and the natural tendency of the case is then to pass to an adynamic or collapsed condition. In a word, while endeavouring to relieve inflammation or congestion, we should not lose sight of the imperative necessity of husbanding the strength of the patient, and of avoiding everything calculated to depress the powers of life, and thereby foster the tendency in question.

While these means, upon each of which I shall descant somewhat in detail presently, are resorted to; while general excitement, local inflammations, and congestions, if they exist, are reduced by suitable depletives and sedatives, and the bowels, when costive, opened by mercurial and other purgatives, attention must be paid to tranquillizing the stomach, by internal and external agencies to be specified hereafter; and at this period, and later, the physician must watch the efforts of nature, and promote any critical movement she may indicate. At this early stage of the disease, a large number of physicians, in this country and in the British colonies, have had recourse to special remedies—to mercury, particularly, with a view to revolutionize the system, and by its sialagogue effects to destroy the poisonous tendency of the disease, or to cure inflammation. Others endeavour by means of particular stimulants, as turpentine, to produce a specific and counteracting impression on the diseased gastric organs, with a view, by substituting a new and different kind of irritation, to remove the one already existing there. Others, again, after proper depletion, or without any depletion at all, have recourse to the Peruvian bark, or its salts, in large doses, as tonics or antiperiodics, alone or combined with calomel. Again in cases, especially those of a mild character, in which no special organ shows signs of being seriously implicated, and in which there exists a disposition to diaphoresis, the disease is often treated by internal and external remedies calculated to bring on and keep up, from the outset, profuse perspiration.

To what extent we are to rely on these and similar agencies in the management of the disease, I shall inquire presently. When, by their means, aided by others suited to relieve local irritation or congestion, a check has been put to the violence of the disease, and a remission has been obtained; or when the latter or metaptoxis has occurred at the regular time by the spontaneous subsidence of the febrile excitement, little remains to be done beyond keeping up the strength of the patient by mild tonics and light nourishment, and preventing, if possible, the onset of further and malignant symptoms by the

use of antiperiodic, tonic, and astringent remedies. But when the remission is incomplete, or is succeeded by a train of symptoms indicating a continuance and exasperation of the disease, another and different course must be pursued. The irritability of the stomach and the vomiting, as also the inflammation of that organ, if it occur, must be combated by sedatives and antacids internally, emollients and revulsives externally; the cerebral organs, if implicated, must be attended to, and their irritation or congestion treated by ordinary means; the failing powers of the system must be sustained by nourishment, tonics, and stimuli, either by the stomach, if it will bear them, or, in the contrary event, by the rectum; while the hemorrhagic tendency must be arrested by astringents or other suitable remedies.

In the congestive form of the disease, when the reaction is deficient or altogether wanting, recourse must be had to means calculated to arouse and sustain the dormant energies of the system, and, at the same time, to relieve the sufferings of the overloaded organs. External stimulation, by means of rubefacients, hot baths, sinapisms, vesicatories, &c.; the internal use of stimuli, tonics, &c., by the stomach or bowels, and, in the milder cases, stimulating and mercurial cathartics, must be resorted to; and, whenever it can be done with safety, the congested vessels of important organs, and the inflammation of some parts, which sometimes is combined with the congestion of others, must be relieved by general and topical bleeding.

Such is a brief outline of the plan upon which the treatment of both forms of the yellow fever may be usually conducted. It remains now to examine more in detail the agents appropriate to fulfil the indications, pointing out the extent to which they may be carried, and the period at which they may be used most advantageously.

Bleeding.—It has been said that the inflammatory form of the disease calls for the employment of antiphlogistics. Of these, the most powerful and efficient is bloodletting, general and local. The lancet was early used in this city, and by many regarded, during the epidemic of 1793, as one of the chief, if not the chief, means of relief. Dr. Rush, who was its principal advocate, says, in his account of that epidemic, that it was in great measure owing to the almost universal use of the lancet that the mortality of the disease diminished in proportion as the number of persons who were affected by it increased. About the middle of October the loss of life was scarcely double what it was in the middle of September, and yet six times the number of persons were probably at that time confined by the disease. The success of copious bleeding (aided by other means to be soon mentioned) was, he thinks, not confined to the city of Philadelphia. Several persons who were infected in town and sickened in the country were cured by the lancet; and Dr. Rush does not hesitate to state that, could a comparison be made of the number of patients who died of the yellow fever in 1793, after having been plentifully bled and purged, with those who died of the same disease in the years 1699, 1741, 1747, and 1762, the proportion would be very small in the year 1793, compared with the former years. Including all who died, under every mode of treatment, he suspected the mortality to be less, in proportion to the popu-

lation of the city and the number of persons who were affected, than it was in any of the years that have been mentioned (iii. 170, 171).

Dr. Rush found the lancet beneficial in various ways: 1. It raised the pulse when depressed, and quickened it when preternaturally slow or subject to intermissions. 2. It reduced its force and frequency. 3. It checked in many cases the vomiting which occurred in the beginning of the disease, and thereby enabled the stomach to retain purgatives. It likewise assisted these in preventing the fatal vomiting of a later period. 4. It lessened the difficulty of opening the bowels. 5. It removed delirium, coma, and wakefulness. 6. It disposed in some cases to a gentle perspiration. 7. It lessened the muscular debility of the system. 8. It frequently removed in a few hours the redness of the eyes. 9. It eased pain. 10. Though sometimes, at first, increasing it in every part of the body, more especially in the head, it subsequently relieved it. 11. When used early on the first day, it frequently strangled the disease in its birth, and generally rendered it more light, and the convalescence more speedy and perfect. 12. In those cases which ended fatally, bloodletting restored or preserved the use of reason, rendered death easy, and retarded the putrefaction of the body after death (iii. 143-145).

The use of bloodletting was adopted by Dr. Rush's pupils and followers—Drs. Griffiths, Physick, Penington, &c.; by all, in fact, who at that time and subsequently were designated under the appellation of Rushites. Dr. Currie, who, in many respects, was far from being included in that category, was nevertheless an advocate of bloodletting, which, according to him, generally afforded relief in all cases where the activity of the arterial system was evident, and the head and epigastrium were at the same time much affected.¹ Dr. Barnwell, too, who, in like manner, practised in the city in 1793, was a warm advocate of bloodletting, which he used in the majority of cases, and repeated, as the urgency of the symptoms required, in eight, twelve, or twenty-four hours, from one to four times.

So convinced, indeed, were the physicians of that period of the advantage to be derived from bloodletting, carried even to a great extent, that some of them were disposed to attribute much credit to themselves for having originated the practice. Dr. Rush states that he was led to use the lancet from the theory he soon formed of the fever, leaving us to infer that no one had thought of it before; and Dr. Barnwell devotes some pages of his work to the defence of his claim as the originator of the practice (pp. 392, 399). They forgot that bleeding was used in this country, under certain circumstances, by Moultrie, who states that it was extensively resorted to by what he calls the empirics; and that in tropical climates it was highly recommended by Towne (p. 25), Desportes (p. 207), John Williams (p. 37), Poissonnier (pp. 56, 57), Dazille (p. 250), R. Jackson (p. 268), and others; and spoken of as sometimes useful, or as frequently employed in their time, by Makittrick, Warren, and many other practitioners, several of whom, let it be remarked, are referred to by Dr. Rush himself.²

¹ Fever of 1793, pp. 40, 41; On Bilious Remitting Fever, Appendix, p. 221.

² The claimants for the practice of free bleeding might indeed have found that they had

Be this as it may, the use of venesection has been commended, in the early stage of the disease, by a large number of the physicians of this country, from Boston to Texas, as well as by those of Europe and tropical regions. But, among those who advocate the antiphlogistic treatment, opinion differs as to the extent to which bloodletting may be carried—even as to the propriety of employing it at all. By one class it is resorted to freely, largely, and without fear of consequences. Among these, Dr. Rush, as we have seen, stands pre-eminent in this country; and equally positive on the subject are Currie, Barnwell (already cited), Merrill, Ffirth, Davidge, Cartwright, E. H. Smith, Caldwell, Monson, Lawson, and Beugnot; and those who are conversant with the doings of West India, and of some European practitioners—Towne, Jackson, Wilson, Rochoux, Dickinson, Lefort, Moseley, &c.—need not be told that these highly clever physicians were not less bold in respect to the use of the lancet. Dr. Rush, in 1793, could not conceive that any inconvenience would ensue from the loss of a pint or even twenty ounces of blood at a time, and drew from many persons seventy or eighty ounces in five days, and from a few a much larger quantity. He repeated the operation as often as ten times (iii. 247, 248). In 1794, he often took 100 ounces, sometimes 140 and 150 (iii. 221). In 1797, he again often bled to 100 ounces, and records cases, in the hands of other physicians, in which 150 and even 176 ounces were abstracted.¹

Dr. R. Jackson speaks of the abstraction of three pounds and upwards of blood, at one time, as an event of frequent occurrence.² Moseley (p. 445) and Catel (p. 14) bled *ad deliquium*, and repeated the process, if required. Towne (p. 25) was far from sparing in the use of the lancet. Dickinson (p. 155) bled, regardless of quantity, until the excitement was reduced and the predominant symptoms were removed. Dr. Comrie, in the treatment of the fever which prevailed among the crew of the *Raven*, in 1815, bled sometimes

been anticipated not only as to the employment of the lancet, but to the frequent repetition of it. Dazille informs us that during nearly a century, bleeding was almost the only means employed in the West Indies. It was very customary to have recourse to it twelve, fifteen, and even twenty times or more in an attack.* “Usually, in Europe,” says Chanvalon, “even in acute diseases, nature does not progress so rapidly as to afford us no time to notice and follow the course she adopts. In the islands she proceeds so rapidly, that if we fail to seize the disease at the very outset of the attack, it assumes so violent a character that the physician can no longer control it. The patient is then treated like a house on fire, part of which must be sacrificed in order to save the rest. He is, therefore, bled fifteen or eighteen times in the twenty-four hours, and in the intervals other remedies are used.”† This was about the year 1750. The same practice was pursued during the epidemic which prevailed in that island in 1770 and 1771.‡ It would seem, indeed, that copious and reiterated bleeding was the favourite practice, at Martinique and other islands, so early as the days of Father Labat.§

¹ Rush, *Inquiries*, &c., iv. 22.

² Sketch, i. 227. See also *Outline*, &c., p. 264; *Fev. of Spain*, p. 136, 140, &c.

* *Observations Gén. sur les Mal. des Pays Chauds*, p. 250.

† Thibault de Chanvalon, *Voyage à la Martinique*, 4to. p. 76.

‡ Romanet, *Voy. à la Martinique*, 8vo. p. 169.

§ Nouveau, *Voy. en Am.*, ii. 3.

to the amount of 200 ounces or more in the course of three or four days. Among the men on board the *Niobe*, the same heroic plan was pursued; they were bled sometimes to the amount of 100 ounces in the space of twelve hours, and the lancet was "repeated until the urgent symptoms were removed, such as headache, pain in the thorax and abdomen, heat of skin, &c. The first bleeding was generally to the amount of 60 ounces or more, and was always continued until syncope was induced."¹ The same bold practice is recommended by not a few writers within and beyond the tropics.²

By another class of physicians—and among these must be classed those who advocate a tonic and stimulating treatment—the lancet is used sparingly and cautiously, or even discarded altogether. More than a century ago, Dr. Warren, of Barbadoes, expressed himself decidedly against free and repeated bleeding in the disease, which then prevailed in that island. "For the truth of this observation," he says, "I appeal to all who have been conversant in the business of medicine here during this reigning constitutional disorder, who, I am persuaded, will readily own that taking away blood in a large quantity, or often, and especially after the first day, has always aggravated the disease and exasperated all the symptoms, and laid a sure foundation for inevitable ruin" (p. 30).

This estimable physician, however, acknowledged that when called in early, "if the patient had been a very hard drinker, or gross feeder," or was of a sanguine complexion and full habits, or the like, he ordered a vein to be opened, and a few ounces—more or less, as circumstances seemed to him to require—to be taken away (*Ib.*). Nevertheless, he avers that he often very happily succeeded where the persons had not been bled at all, though of the grossest and most plethoric habits (p. 31). Sir G. Blane was of opinion that, considering the tendency to gangrene, and the rapid supervention of debility, "bloodletting ought to be practised with discrimination and limitation;" and that the "subjects to whom bloodletting is most likely to be beneficial, are those of a robust and plethoric constitution, newly arrived from Europe" (p. 428). Dr. J. Clark, of Dominica, entertained a similar opinion on the subject, and states that, from the remarkable flushing of the face, great inflammation of the eyes, and full pulse in the first stage of this disease, young practitioners might be induced to use the lancet freely; and the French surgeons, whose chief remedy in almost all disorders of these islands is venesection, very readily fell into this error. But experience showed the danger of the practice (p. 24). In some new comers immediately from Europe—robust and sanguine—who had never been in the West Indies, the lancet was used with seeming advantage; but as it failed often, it was laid aside. "There

¹ Edinburgh Journal, xiii. 167, 170.

² O'Halloran, *Fev. of Andalusia*, p. 87; Ralph, ii. 83; Brown, p. 65; Lefort, *De la S. et du Q.*, p. 576; Ruz, pp. 37–8; Pinkard, iii. 154; Burnett, *Edinb. Journ.*, viii. 397; *Ibid.*, *Fev. of Medit.*, p. 393; Cartwright, *Recorder*, p. 9; Ffirth, p. 29; Merrill, ix. 248, 249; Wallace, *Edinb. Journ.*, xlv. 278; Veitch, p. 132; Musgrave, *Med.-Chir. Trans.*, ix. 136; Ferguson, *Med.-Chir. Trans.*, ii. 191; H. McLean, pp. 137, 162; *Fever of Cayenne in 1850*, p. 232; Lawson, *Stat. Rep. of Sickness of the Army of the United States*, p. 267.

was not a single instance of an emigrant recovering who had been bled" (pp. 24, 5). Dr. Todd, of Jamaica, speaks in the same way of venesection, stating that it was nearly abandoned after the most complete proofs of its fatal effects.¹ Chisholm, as it is well known, entertained like sentiments as to the employment of the lancet;² and so did many other competent authorities,³ some of whom used it moderately, while others discarded it altogether. "Without any intention," says Dr. Gillkrest, "to impugn the statements respecting the advantages derived from liberal venesection on particular occasions in the West Indies, it must be declared that the weight of evidence is against its general adoption in yellow fever, even where, *primâ facie*, it would seem to be indicated." Dr. G. further cites the authority of Mr. Linton, who has long been resident in the West Indies, and who states that in the records of the Naval Hospital of Jamaica (of which he had the charge), extending back for many years, the mortality was very great from the depletory system, which, from the seeming inflammatory nature, had been acted upon (*Cycl.*, ii.).

Dr. Imray, to whose interesting accounts of the epidemic of Dominica in 1838 I have often had occasion to refer, informs us that it was soon discovered, after the occurrence of several cases, that the usual mode of treating fevers by sanguineous depletion—so generally necessary with Europeans when attacked with fever on their arrival, or after a short residence—was not only inefficacious, but often highly injurious."⁴ These observations were made in 1838. The disease having reappeared in Dominica in 1841, the lancet was again tried, and once more failed. "It was found, as a general plan of treatment, not to succeed. It only tended to bring on sooner that debility which supervened so early, and diminished the prospect of the patient's recovery."⁵

In the hands of Dr. Blair, the lancet does not seem to have succeeded better; bloodletting was injurious as the basis of any system of treatment, either in large, or small and repeated quantities. Of course, the more inflammatory or dynamic the type, the less mischief ensued.⁶ Dr. Blair's annotator, Dr. John Davy, states that in a few cases during the epi-

¹ Annals of Med., ii. 341.

² Vol. i. p. 334, 5.

³ Levaeher, p. 97; Bally, p. 493; Boyle, p. 235; Osgood, p. 43; Arnold, pp. 42-3; Baneroft, p. 53; Rouppe, p. 375; Vatable, p. 349; Dazille, Mal. des Nègres, p. 49; Makittrick, p. 135; Holliday, p. 17; Madrid, pp. 17, 18; Paton, Laneet 1853, ii. 288; Joubert, p. 972; Hillary, p. 157; Savarésy, pp. 44, 311, 312; Pugnet, p. 365; Cassan, v. 54; Lind, pp. 177-8; Wright, Rept. of his practice to the Army Med. Board, in Annals of Med. ii. 344; Ib., Faets and Obs., vii. 10, 11; Bruee, in Lind (note), p. 280; John Hunter, pp. 118, 119; Gillespie, pp. 72, 133-4, 136; Danecr, p. 88; Furlong, p. 291; Gonzales, p. 324; Caillot, p. 301; Gilbert, pp. 83-4, 91; Pym, pp. 11, 14; Redman (in Pym), p. 76; Robertson, of Barbadoes (ib.), p. 243; Thomas Clark, p. 104; Lempriere, ii. 118; Wallae, xlvi. 278; Imray, liii. 83; Ib. lxiv. 320; Stevens, pp. 306-7; Hume, pp. 209, 219, 238; Curtin, Med. Comment., ix. 237; Stewart, Med. and Philos. Reg., iii. 190; Poissonnier, pp. 56-7, 74.

⁴ Edinb. Med. and Surg. Journ., liii. 85.

⁵ Imray, Edinb. Journ., lxiv. 329.

⁶ Yellow Fever Epidemic of British Guiana, p. 104.

demic of Barbadoes in 1842, the abstraction of blood from the arm had a trial, but the results were not encouraging.¹ Dr. Archibald Smith, in his account of the rise and progress of yellow fever in Peru, tells us that general depletion by the lancet was tried in 1854 with confidence—probably founded on European opinions—by a celebrated Spanish physician just arrived from the Peninsula; but scarcely was the arm bound up in these cases, when the strong and full pulse of the preceding hour began to sink with extraordinary rapidity—especially if the bleeding took place after the second day of the fever. Physicians soon learned to be cautious on the subject, and it became a settled opinion among them—both Europeans and natives—“that bleeding could not be resorted to without incurring thereby the utmost danger to the patient.”² Even local bleeding was injurious. The abstraction of blood was disapproved of during the fever which prevailed in Lima in 1818.³ Dr. Lallemant, whose name has frequently appeared in these pages, and who encountered the disease in Rio Janeiro in 1850, does not speak of the use of the lancet in much more flattering terms.³

In Europe and this country, the limited and cautious use of the lancet has been inculcated by competent authorities;⁴ while, by others, bloodletting is seldom if ever resorted to, or thought advisable; and by some, again, its use is severely condemned, or passed by in total silence,⁵ on the plea that it does not put a stop to the disease, or arrest its dangerous tendencies; that the management of the disease may in the very large majority of cases, if not in all, be effected just as well, and with much greater safety, without it; that it seldom does any good, and often does much harm, increasing the prostration without any compensating effect, and depriving thereby the system of a large share of that vital force she is so greatly in need of in the struggle which occurs during the latter stages of the attack. In a word, practitioners have either never forgotten the danger, or if otherwise, have again learned to be timid about running the body under bare poles. They think it advisable, continuing the figure of Dr. Rush, to keep enough sail to lay to during a gale, but not to be altogether at its mercy.

¹ Blair, p. 104 (note).

² Edinb. Med. and Surg. Journ., lxxxii. 199.

³ *Observações Acerca da Epidemia, &c.*, p. 140.

⁴ Griffith, Frazer, Lafuente in Pym, pp. 238–9, 241; A. Hosack, p. 27; Harrison, ii. 325; Thomas, pp. 120, 132; Fev. of N. O. in 1839, pp. 337–8; Davidge, p. 116; Monges, ii. 67; Bayley, Fev. of 1795, p. 113; J. Warren, p. 503; Louis, pp. 333–4; Seaman, p. 44; Heustis, p. 115; Copland, iii. 182–3; Nassy, p. 34; Pym, p. 237; D. Hosack, *Essays*, iii. 310; Monson, p. 188; Townsend, p. 207.

⁵ Palloni, p. 13; Robert, *Guide Sanitaire*, pp. 122, 384, 731; Waring, pp. 72, 5; Valentin, p. 199; Dalmas, p. 122; Seagrove, *Med. and Philos. Register*, ii. 420; Perlee, *Phil. Med. and Phys. Journ.*, iii. 13; Nott, *Am. Journ.*, N. S., ix. 292; Kelly, *ib.*, xiv. 389; Dickson, *Phil. Med. and Phys. Journ.*, iii. 261–2; *Ib.*, *Am. Journ.*, ii. 76; *Ib.*, *Eclectic Journ.*, iv. 114; *Ib.*, *Elements of Medicine*, p. 275; Maclean on *Epid.*, i. 77, 91, ii. 412; Shecut, p. 133; Arejula, p. 180; Pariset, p. 578; Irvine, p. 30; Gros, p. 20; Chatard, *Med. Repos.*, iv. 354; Mitchell, *Med. and Philos. Register*, iv. 385; Wragg, *Report*, p. 20; J. J. Chisholm, *Fev. of Charleston in 1854*, *Charleston Journ.*, x. 451; Humphreys, *Edinb. Journ.*, xii. 177; Wedderburn, *N. O. J.*, v. 209.

By those who are not wedded to any particular theory on the subject, and will take an extended survey of it, both these extreme views as to the employment of bloodletting will be avoided; and while refusing to discard the remedy in all cases, under every possible contingency, and whatever may be the symptoms of active reaction or local inflammation presented, they will be careful to shun that lavish expenditure of blood—that heroic system of depletion in which some have indulged. If, in the inflammatory form of the disease, they avoid the lancet altogether, it will be only in the milder cases, where reaction is moderate, or the important organs but slightly implicated, for experience has conclusively shown that, in those of a more severe character, venesection is not only harmless, but decidedly useful; and if, on the contrary, they bleed very actively and amply, it will be only in such instances of a contrary kind, when the inflammation and congestion run inordinately high, and the danger of disorganization is imminent. But they will find that in the generality of cases they need not place their principal reliance on the lancet—that in very many bleeding is useless, and may do much mischief—and that therefore, as a general rule, the milder and less debilitating method will be the safer, and the one to be preferred; never forgetting the tendency of the disease to run into a collapse, and the danger, by a rapid and copious abstraction of blood, of favouring that tendency, and thereby placing the system in a condition from which recovery is always doubtful.

The propriety of this course is the more evident, as the superiority or success of the heroic employment of the lancet—except under rare and peculiar circumstances, is more than problematical—has never, notwithstanding all that has been said to the contrary, been established beyond the possibility of doubt; and has often met, in this city and elsewhere, with a flat denial on the part of those who, from their position, had ample opportunities of judging of its merits. It may be remarked, that the diminished mortality of the fever which occurred in 1793, towards the middle and close of the epidemic, and which was ascribed by Dr. Rush to the use of the lancet having become more general about that time, was doubtless due to another and different cause—the lessened severity of the disease; a change which, as we have seen, takes place in the large majority of epidemics.

The idea of *curing* or strangling the disease by bloodletting is erroneous; and the testimony of the profession may be appealed to for the fact, that the object to be obtained from it is principally to guard against local inflammation or congestion—to prevent these from running too high—and by removing the excess of excitement, to allow the system to bear, to the end, the force of the disease. For these reasons, bloodletting, when such symptoms present themselves, should be used carefully and cautiously, and limited principally to robust and plethoric constitutions, and to cases in which the pain in the head is very severe, the face and eyes red, the pulse hard—either full or small—the skin hot; in a word, to cases attended with inordinate excitement.

But even under such circumstances, though the lancet may sometimes be used freely and repeated several times; though it may prove eminently

serviceable and even be indispensably required, the very copious abstraction of blood at a single bleeding should be avoided, for fear of its hastening on a state of collapse, and placing the system in a condition whence reaction is with difficulty obtained. I am aware that the opinion here expressed is opposed to that of respectable authorities. Dr. Barton, for example, in reference to the epidemic of New Orleans in 1833, found one efficient bleeding preferable to several smaller ones. He drew sometimes 70 ounces of blood at one operation; and twelve to eighteen hours after, 10 to 20 ounces more (p. 15). But it may be doubted whether this plan has been generally adopted in New Orleans, and whether he himself found it as useful in 1853 and 1854 as in 1833, when the disease appears to have been of a more inflammatory character. In cases in which the pulse is full but compressible, and the skin soft and perspiring, the lancet has seldom, if ever, been useful in the hands of eminent physicians (*Monges*, p. 64), however strongly other symptoms may seem to indicate the propriety of the practice.

Much will depend, of course, on the peculiar medical constitution of atmosphere existing at the time, for, as every one knows, and has already been stated, the utility of antiphlogistic means, and the extent to which they can be pushed, differ considerably at different epidemic times, and even during different periods of the same season. Dr. Rush states that copious depletion was not as well borne in 1798 as it had been in 1793. Desportes, who was a free bleeder, states that in the winter of 1746 the sick did not in general bear bleeding well; especially during the month of November. This he attributes to the humidity of the atmosphere. He bled more in dry weather.¹

At Pensacola, in 1823, Dr. McMahon, of the army, found that before and after the 9th of October the disease presented a character diametrically opposite. Prior to the above period, there was an excess of arterial action requiring a free and liberal use of the lancet; but subsequently the vital powers manifested such a loss of energy that the strongest stimulants were imperiously demanded.²

Careful as the physician must be as to the selection of the cases in which the lancet can be used safely and advantageously, and as to the extent to which the loss of blood may be carried, he must be not less so as to the period at which the operation is to be had recourse to. It is a fact agreed upon in this city, and in every place visited by the yellow fever, that bleeding—and the remark applies to any mode of treatment we may institute—to be most successful must be resorted to with the least delay possible after the outset of the attack. The sooner it is used the better. It should be limited to the first stage, or that of reaction; and, if possible, within the first twenty-four or forty-eight hours.³ Such was found to be the case by Rush (iii. 145, iv. 22), Devèze (p. 268), Currie (p. 41), Ffirth (p. 29), Caldwell (*Essays, Fever of 1805*, p. 101), Warren (*Tytler*, p. 503), Rouppe (p. 311), Leblond (p. 111), Caillot (p. 301), Vincent (p. 31), Townsend

¹ *Maladies de St. Domingue*, pp. 169, 321.

² *Stat. Rept. of the Sickness and Mort. in the Army of U. S.*, p. 40.

³ *Dariste*, p. 190; *Chatard, Marseilles Journ.*, v. 339.

(p. 207-8), Barton (p. 42), Louis (p. 333), J. Clark (p. 25), Blane (p. 449, 2d ed.), Bancroft (p. 60), Wilson (p. 9), Moseley (p. 447), Rochoux (p. 585), Gilbert (p. 91), Dariste (p. 190), Jackson (*Sketch*, i. 230), Catel (p. 14), Thomas Clark (p. 18), Hume (p. 219), Williams (p. 39), Wood (i. 310), Heustis (p. 115), Barton (p. 42), Merrill (*op. cit.*, p. 249). Dr. Rufz, in his essay on the yellow fever of Martinique, published in the *Medical Examiner* of this city, has inserted the following table, furnished to him by Dr. Catel, who, being at the head of the medical department of one of the military hospitals of that island, had ample opportunities of testing the subject in question:—

Of 176 patients treated during the first twenty-four hours, 5 only died, or 1 in 35.

Of 108 treated on the second day, 11 died, or 1 in 9; and of 143 treated on the third day, 40 died, or 1 in 3.¹

Another physician of eminence of the same place—the late Dr. Lefort—remarks, to the same effect, that of the patients he had an opportunity of bleeding once, twice, or even three times, immediately after the attack, four out of five recovered; the disease terminating on the second or third day, and convalescence setting in on the fourth or fifth day. “Bloodletting performed on the second day of the attack affords six times less chances of success; and on the third and fourth days, it is too late to have recourse to it.”²

It is not in inflammatory cases only that bloodletting has been used. By not a few high and competent authorities it is resorted to in those of a congestive kind. That it should have been very generally employed in that form of the disease, is not to be expected. The symptoms of prostration by which it is characterized are such as to be readily supposed calculated to deter many from having recourse to a measure likely to increase the existing danger. Hence, among those who regard bloodletting as a remedy of equivocal utility and of dangerous tendency in the inflammatory form of the disease, we could scarcely expect to find advocates of its use in the form in question. But it is disapproved of by many of those who are not inimical, and by some who resort to it largely in the inflammatory form of the disease. On this subject, the following remarks by Dr. Cartwright, of Natchez, will be responded to by numbers of experienced physicians in this and other countries: “In such cases (congestive), if blood be taken away, even should the patient not immediately sink under it, so far from reaction being produced, the blood-vessels lose more and more of their contractile power; stimulants impart no strength; the warm bath and rubefacients produce no more effect on the skin than if applied to so much leather; the organs become paralyzed; the sympathies more deranged; and the whole system soon appears as if it were divided into different parts, one not depending on another, and each having the principle of life diminished in it.”³

By others, however, who hold that in cases of this kind the vital energies are oppressed by an excess of stimulus, and not labouring under a state of

¹ Med. Exam., iii. 118.

² De la Saigné, p. 574.

³ Med. Recorder, ix. 22.

direct prostration, the lancet is used freely, to remove this oppression and thereby elicit reaction. Dr. Rush bled in cases of the kind, when the pulse was preternaturally slow, provided, however, it was tense. He was led to this not only by the theory he entertained of the disease, but by the success he had derived, in a slow and depressed state of the pulse, in apoplexy and pneumonia. He, moreover, fortified himself with the authority of Sydenham, who, in the account he gives us of a new fever which appeared in 1685,¹ advocates the use of the lancet in what we would now denominate a congestive state of the system.² Dr. Rush was, moreover, supported by the more apposite authority of Moseley, who, as may be recollected, was not deterred from bleeding his patients, "from the depression of the pulse and from the faintness which sometimes accompany the very onset of the fever;" because "here the pulse always rises and the faintness disappears as the heart is relieved from its oppression by the loss of blood."³

Dr. Caldwell, while recommending powerful stimulants, both external and internal, in cases of the kind, says: "But as the mass of blood is too weighty for the action of the enfeebled heart to maintain in motion, to diminish and lighten that mass by means of venesection, cautiously employed in the midst of the use of awakening and strengthening stimulants, is a mode of treatment which the perilous nature of the complaints justifies, which reason and analogy seem to approve, and which no established principle forbids."⁴

Dr. R. Jackson was an advocate of this mode of practice, and carried the abstraction of blood (aided, of course, by other means) to a very great extent (ii. 234, 236); and the same practice has found an able advocate in an intelligent physician of our own country, Dr. Merrill. In speaking of congestive cases, in his account of the epidemic of 1825, at Natchez, he remarks that, "when treated with bloodletting, to the necessary extent, in the commencement, they almost invariably assume the character of inflammatory cases,

¹ Sydenham, ii. 351, Wallis ed.

² "Some persons may infer the malignity of a fever not so much from the purple spots as from the fact of its having proved fatal in a manner disproportioned to the intensity of its symptoms and the time of its duration. To these I would answer that the process has been as follows: On the first onset of the disease, nature was, as it were, so much dejected and oppressed as to be incompetent to exhibit the natural and regular phenomena consonant to the disease, so that all the phenomena which have shown themselves were anomalous. The animal economy being disturbed and thrown down, the fever becomes depressed. Had nature taken its own course, the fever would have been developed.

"A rare proof of this happened many years ago, in the case of a young man to whom I was called in. Although he seemed dying, the temperature in the external parts of the body was so natural to the touch, that when I said he was suffering from fever, his friends disbelieved me. However, he *was* so suffering; the fever being unable to show itself visibly, from the great oppression of the blood, by which it was forbidden an exit, and so was smothered internally. I said that if he were bled the fever would show itself. He was bled freely, and the fever *did* show itself. I never saw a worse case. It yielded only to the third or fourth bleeding."*

³ On Tropical Diseases, p. 447.

⁴ Med. and Phys. Mem. (1826), p. 166.

* Sydenham, On the Appearance of a New Fever, 1685, Syd. Soc. edit., ii. 210, 211.

and are afterwards quite as easily managed.”¹ In all such cases it is particularly insisted upon that, in order to obtain this desirable effect, the treatment must be applied cautiously and watchfully—more so, indeed, than in the inflammatory form; and by every one who has resorted to it, stress is laid on the necessity of a gradual, not a sudden, abstraction of blood. “The danger of collapse from a too sudden abstraction of stimulus,” says Dr. Merrill,² “when the system was labouring under an oppressed circulation, rendered it important not only that the vital powers should be cautiously sustained during the operation, but that the loss of blood should be more gradual, and effected in small quantity, at short intervals. This was found to be more feasible on account of the greater length of the exacerbation of fever in the congestive cases, and the shorter and less distinct remission, which admitted of the repeated application of the remedy during the same paroxysm.” It is remarked by Dr. Merrill, in another place,³ that the loss of a large quantity of blood is required in the congestive variety of the disease, but that it must be taken by small bleedings, frequently repeated. “Eight or ten pounds of blood have sometimes been drawn during the first twenty-four or thirty-six hours, by as many different bleedings, and a favourable reaction was generally the consequence of such practice, when properly conducted.” In 1825, Dr. Merrill drew, in one case recorded, four pounds in six hours, with decidedly good effect (ii. 226). Dr. R. Jackson drew from two to four pounds (i. 235, 236).

Useful as such an abstraction of blood, in the cases in question—obtained in the gradual and cautious way mentioned—has proved in the hands of these and other practitioners, it has been found necessary to employ, at the same time, other means, internal as well as external, calculated to promote the desired reaction, and, in cases marked by symptoms of considerable exhaustion—weak pulse, cold and pallid skin, &c.—to elicit somewhat that reaction by those means, before resorting to the lancet.⁴

That such a mode of treatment may be, and has often been, usefully resorted to, we have no reason to doubt. It has been found advantageous in the congestive form of other varieties of febrile affections; and we might, *à priori*, conclude from this that it would be equally so in the like form of yellow fever, while its success is attested by authorities which it were not safe to controvert. Nevertheless, it cannot be denied that, as a general rule, the lancet must be regarded as of problematical utility in the majority of congestive cases, and will, especially if carried to a great extent, prove an instrument of no small mischief, producing an augmentation of prostration, and even inducing collapse, as well as the effects depicted by Dr. Cartwright. In many such cases the poison has produced a condition of direct, not indirect, debility; the congestion in the viscera is a secondary, not primary, link in the chain of morbid phenomena; and the main object to be attained is to reanimate the prostrate powers of life, not to unload the engorged vessels.

Dazille, long ago, recommended the bleeding to be effected from the foot

¹ N. A. Med. and Surg. Journ., ii. 226.

² Ibid., ii. 225, 226.

³ Med. and Phys. Journ., ix. 249.

⁴ Merrill, ii. 226; R. Jackson, i. 236.

when the head was affected.¹ Caillot seems to prefer that mode of blood-letting in all cases (p. 303). Dariste (p. 190) and Vatable (p. 342) are in favour of the practice, under the circumstances mentioned. At a later period, Louis was of opinion that, although we cannot propose bleeding as a means to prevent inflammation of the brain, no traces of which were found by him at the autopsies, nor to remedy any serious mental disorder, which did not exist, still, on account of the headache, the blood had better, perhaps, be taken from the foot than from the arm (p. 333). I am not aware that the practice here suggested has been tried among us.

Local Depletion.—When, in cases of much inflammatory violence, the pulse and other prominent symptoms of congestion or local irritation have been somewhat reduced by the lancet; or in cases in which the latter has been judged inadmissible, but in which, nevertheless, symptoms of gastric inflammation or cerebral derangement demand special attention; or, again, when the disease has already progressed too far to justify the hope of employing the lancet with advantage or safety—the object may be, and is, often attained by local depletion. Scarified or dry cups and leeches, in such cases, often produce an advantageous effect. By many, indeed, they are almost invariably substituted for the lancet; or are used with safety when, from the state of the pulse and other signs, we discover an evidence of feebler reaction than comports with the safe use of general depletion, emptying the engorged capillaries of the organs or tissues affected, without abstracting an undue amount of blood from the general circulation.

This practice of local depletion, which was resorted to with decided advantage during our early and subsequent epidemics, by Drs. Monges (p. 64), Caldwell (p. 102), and others, has been recommended by competent authorities in other cities of the United States, in Europe, and in tropical climates.² Leeches, when easily procured, must be preferred to cups, unless a counter-irritant effect on the surface is at the same time desirable. They, as well as cups, must be applied over, or as near as possible to, the part affected—on the epigastrium, when the affection of the stomach is to be relieved; on the temples, along the course of the jugular veins, inside of the nostrils, or at the back of the neck, when the condition of the brain or of its membranes calls for assistance. But to a certain, though more limited, extent, local means of depletion must, like general depletion, be resorted to at as early a period of the disease as possible. When used late, they are of no utility, and

¹ *Maladies de Nègres*, p. 189.

² Paton, *Lancet*, 1853, ii. 288; Blair, p. 105; Dickson, i. 361, 369; Makittrick, p. 142; McArthur, p. 351; J. Williams, p. 40; Thomas, p. 122; Barton, p. 22; Ffirth, p. 29; Harris, p. 14, 46, &c.; Wood, i. 310, 312; *Fever of N. O. in 1839*, p. 338; Dickinson, p. 159; Jourdain, *Ann. de la Méd. Physiol.*, iv. 505; Stone, pp. 198, 199; Boyd, in *Johnson on Trop. Clim.*, p. 304; Dariste, p. 190; Evans, pp. 303–305; Catel, p. 14; *Ib.* *Fev. of 1844*, p. 232; Levacher, p. 97; Le Reverend, *Ann. de la Méd. Phys.*, xii. 543; Louis, p. 333; Osgood, p. 45; Beguerie, p. 53; Vatable, p. 349; Townsend, p. 207; Imray, liii. 87; *Ib.*, lxiv. 329; Arnold, p. 42; Hosack, iii. 439; Joubert, p. 974; *Fev. of Cayenne in 1850*, p. 189.

may even act disadvantageously by increasing the prostration of the powers of life.

It is proper to remark that, in the advanced stages of the disease, it is often difficult to stop the flow of blood resulting from leech-bites and scarifications, and that care should always be taken to watch at that time the surface on which such wounds had been made at an early stage, as they are apt to re-open and give issue to more blood than can well be afforded. When at such a period local engorgements or irritations are to be relieved, the above objections can sometimes be obviated by substituting dry cupping. The same effect from leeching is experienced sometimes in the early stage of the disease, when the hemorrhagic tendency is well marked.

Many years ago, Dr. John Williams, of Jamaica, recommended the use of arteriotomy to the temporal region; particularly for the relief of delirium (p. 40). It has since been used by others;¹ but though by that means a more prompt abstraction of blood may be obtained, it is doubtful whether greater benefit is therefrom derived than from the means already mentioned, while the operation is of a disagreeable kind, and the bandaging required often produces more compression of the head than most patients can bear.

CHAPTER XXXIV.

TREATMENT—CONTINUED.

Emetics.—At the breaking out of the fever in this city, in 1793, some physicians—influenced by the belief that the disease against which they had to contend was nothing more than an aggravated form of the common bilious remittent fever of the country, in the early stage of which active emesis is often beneficial—resorted to emetics. The late Dr. Hodge went so far as to affirm that he used with advantage a solution of tartarized antimony after one copious bleeding, with a view to produce an artificial cholera morbus.² In 1797, as we are told, the same physician employed full vomiting in the early stage of the fever, after moderate bloodletting, with remarkable success; of seventy patients he treated in this manner, he lost but three.³ Dr. Rush also, in 1798, in a communication to Drs. Physick and Cooper, Physicians of the Fever Hospital, who had sought his advice as to the best mode of treating the disease, states, for sundry theoretical reasons he enumerates, that “he became satisfied of the necessity of producing an artificial cholera morbus about the fourth day of the disease. He began the

¹ Merrill, ix. 250; Bryson, pp. 60, 61 (on Bd. of Eden); Blair, p. 108.

² Currie, A View of the Diseases most prevalent in the United States of America, &c., p. 76.

³ Currie, Fever of 1799, p. 86.

practice about the 5th of August. His solicitude for the issue of it was very great; but it succeeded to the full extent of his wishes, and thereby lessened, in a great degree, the anxiety and distress which accompany our attendance upon this ferocious disease. The medicines he used for *shaking the gall-bladder and bowels*, and discharging their contents, were tartar emetic, gamboge, jalap and calomel combined, or given separately, and in small or large doses according to circumstances. He thought that, perhaps, turpeth mineral might be added to this list of vomiting and purging medicines with advantage.¹

Emetics were also administered and recommended by Towne (p. 30), Leblond (p. 111), Gonzales (p. 320), and others.² Among these, Dr. Hacket, of the British Army, states that during a recent visitation of the disease at Trinidad, his "practice commenced in almost every case by an emetic of sulphate of zinc," and it would seem that in the employment of this, in addition to his other means, he found reason for being satisfied.³ Dr. R. Jackson prescribed them sometimes, after the abstraction of blood and due preparation of the stomach—without which precaution he thought they did harm (i. 271, &c.). Dr. Osgood, too, while, under ordinary circumstances, he objects to emetics, recommends them when, after purging, there remains frequent retching of phlegm or bile, indicating that foul substances remain adhering to the upper intestines (p. 47).

Respectable, however, as such authorities may be, we hazard nothing in expressing the opinion that the weight of testimony in modern times is not, and, indeed, has not been for years back, in favour of the general employment of emetics in the treatment of the yellow fever. That by some physicians in this country and elsewhere the administration of an emetic dose of ipecacuanha or sulphate of zinc, under peculiar circumstances, has been spoken of in terms of commendation; and that such remedies, or large draughts of hot water or warm infusion of chamomile or boneset, may find a useful application in the early stage of the disease to bring on reaction, and especially when the attack comes on after a full meal of which the patient cannot disgorge himself, is doubtless true.⁴ But it is not the less certain that articles of a more irritating nature—tartarized antimony in particular—and the very substances already mentioned, except when resorted to for the purposes, and at the period referred to, have so generally proved hurtful as to justify their almost complete proscription.

They aggravate the irritation and inflammation of the gastric organs, if these morbid states exist, or tend to create them if they do not, by their acrid or irritating qualities and the contractions of the parts concerned in the act

¹ Currie, *Fever of 1798*, p. 21.

² Addoms, p. 29; Chatard, *Med. Reposit.*, iv. 356; *Ib.*, *Marseilles Journ.*, v. 337; O'Halloran, p. 133; Ffirth, p. 30; Palloni, p. 13; Heustis, p. 115; Irvine, p. 39; Caldwell (*Fev. of 1805*), p. 107.

³ *Gaz. of Health*, No. 16, quoted by Gilkrest, p. 281.

⁴ Valentin, p. 201; Harrison, ii. 334; Wood, i. 309; Denmark, vi. 309; Hosack, iii. 431; Dickson, i. 360; *Ib.*, *Elements of Medicine*, p. 276.

of vomiting, and foster the disposition to vomit, which it is, on the contrary, a primary object to relieve, and which when once elicited it is difficult, if not impossible, to control. More than a century ago, Dr. Warren, of Barbadoes, said of emetics, "they enrage the distemper, raise an unappeasable hurry and tumult in the spirits, grievously affect and irritate the nerves of the *plexus cardiacus*, and all the nervous membranes about the *præcordia*, give a further dissolution of the blood; and in short (if I may use the phrase), raise a demon in the system, that is scarce ever after to be laid but by the death of the patient" (p. 36). "Quoad emetica. Cadaverum incisio"—says Moultrie—"ventriculum maxime inflammatum monstrat, et emetica inflammationem intendunt, et febris vehementiam a vomendi motu violenti augent, ergo lædunt: atque ægros vidi, quibus morbus mihi mitis esse videbatur, post emeticum, nausæ et vomitu perpetuo laborare, et febrem vehementem accendi, morte solâ extinguentam" (p. 17).

The late Dr. Monges, of this city, who probably saw more of yellow fever during our various epidemics from 1793 to 1820, inclusive, than any other physician, positively affirms that he never saw a patient recover from the disease to whom tartarized antimony, or any other active remedy of the same class, had been administered. Of the impropriety and danger of the practice he was early impressed, from noticing that of the first family in which his professional services were solicited, five members, to whom emetics had been prescribed, died; whereas three others, who were otherwise treated, recovered (ii. 65). Subsequent experience convinced him still more positively of the necessity of shunning such remedies—which have, likewise, been condemned by other physicians of our city,¹ as well as by those of other parts of this country, of Europe, and tropical climates.²

"How often," says Dr. Moseley, "have I seen and lamented the effects of emetic tartar, given to remove the supposed cause of the treacherous symptom of vomiting! Even in slight degrees of fever in the West Indies in young plethoric subjects and newly arrived, the stomach has been sometimes destroyed by it. Instead of removing the irritating sickness in this fever, or

¹ Cathrall, p. 50; Devèze, pp. 269, 283; Barnwell, pp. 277–78; Currie, p. 49; Wood, i. 309.

² D. Hosack, iii. 331, 431; *Ib.*, Practice, p. 392; A. Hosack, p. 30; Valentin, p. 201; Waring, p. 75; Davidge, p. 120; Dalmas, p. 122; Manson, p. 58; N. O. Fever, 1839, p. 338; N. O. Fever, 1820, p. 13; Cartwright, p. 23; Dickson, *Eclectic J.*, iv. 114; Thomas, p. 120; Merrill, ix. 254; *Ib.*, ii. 231; Townsend, p. 213; Tully, p. 323; Girardin, p. 41; Gros, p. 20; Gillespie, p. 72; Lemprière, ii. 120; Poissonnier, pp. 57, 75; Hunter, p. 120; Stevens, p. 285; Madrid, pp. 19, 20, 21; Hillary, p. 159; Bancroft, p. 66; J. H. Dickson, in Johnson on Tropical Climates, pp. 267–68; Musgrave, p. 139; Guyon (see Lefort, p. 550, and Réponse, p. 37); Blicke, in Zecchinelli, p. 206; Frost, xiii. 345, 347; Rufz, pp. 119; Gilbert, pp. 30, 86, 91; J. Johnson, p. 248; Fellowes, p. 407; Dufour, iv. 53; Arnold, p. 45; Powell, p. 8; Hume, p. 210; Holliday, pp. 16, 18; Dariste, p. 201; Pinckard, iii. 21; Williams, p. 36; Bally, p. 480; Ralph, ii. 87; Audouard, p. 313; Caillot, p. 291; Burnett, pp. 26, 33; Chevalier, p. 16; Pym, pp. 124, 125; Savarésy, p. 313; Robert, i. 121; Osgood, p. 47; R. Jackson, i. 271; Rochoux, p. 643; Blair, p. 105; Seaman, p. 45.

exciting diaphoresis, a spasm has been produced in the stomach; incessant vomiting; inflammation; the vessels of the thorax and head have been stifled with blood; and the patient has vomited away his life" (p. 451). Bruce is equally severe in his censure of emetics (p. 280).

Nearly the same reasons that induce us to proscribe the use of tartarized antimony, and other medicines of like import, in puking doses, lead us to condemn them when given in fractional and repeated portions for sedative, diaphoretic, and other purposes. They arouse or increase irritation of the stomach, and, by awakening the irritability of that organ, produce an effect which it is often next to impossible to mitigate, and which might otherwise, perhaps, have been averted. I am aware that by Palloni, in Europe (p. 31), Dr. S. Brown, in this country (p. 68), Dr. Wallace, in the West Indies (p. 278), and others, the practice here referred to has been recommended. I am aware, also, that the sedative, counter-stimulant, and diaphoretic effects thus sought to be obtained may have been, in some instances, advantageous, and would generally prove so if it were possible to obtain them without risk of ulterior injury; but it is impossible to shut our eyes to the fact that, in attempting to attain the objects in question, we pave the way to the production of another effect, the danger of which is far from being compensated by the good resulting from the other, while in many instances the latter is not obtained at all, and the medicines thus administered, instead of producing the desired benefit, aggravate the existing symptoms, and bring on others of serious import.

So far I have spoken of the use of emetics in the inflammatory form of the disease. The same remarks will apply to most cases of the congestive form, especially when there exists a gleam of reaction, and when the stomach exhibits a disposition to a state of irritation. In the more marked cases of congestion, however, emetics in the hands of some physicians appear to have proved highly serviceable. In circumstances of the kind, their effects have been highly eulogized by Dr. O'Halloran (p. 135). They were used successfully by Dr. Rush in cases attended with a languid pulse on the first day.

Dr. Dickson, of Charleston, thinks well of emetics in this form of the disease, to rouse the system from the state of torpor in which it is thrown. In such cases he has no faith in tartar emetic or ipecacuanha, as they require to be given in large doses, and are therefore not unlikely to depress the vital powers seriously if they fail to procure their own expulsion. He prefers some quicker emetic, as sulphate of zinc, or sulphate of copper—or the more stimulating, as salt and mustard (i. 374).

Very different are the views of Dr. Cartwright, formerly of Natchez, as to the efficacy of large doses of tartar emetic in this, and indeed in other forms of the disease. "In the first stage of the yellow fever," he says, "or in that which consists of an ataxic or crippled reaction, when the blood is unequally determined, the heat unequally diffused, sensation impaired, and secretions suspended, I found no other remedy, or combination of remedies, which produced such decided effects as tartar emetic in full doses. It, however, seemed sometimes inadequate to make a sufficient impression on the torpid system.

When given in this stage of the disease, its effects were not *so soon* apparent as in the healthy state of the system, or in less violent diseases. Very often it would be an *hour* or *more* after a full dose had been taken, before the system appeared to feel it. At length, that peculiar sensation of heat and cold at the same time would somewhat yield to a sensation of heat only, the temperature of the skin would become more uniform, and, as the excitement was brought out, great distress would ensue, and the system appeared to arouse from its torpor, and to regain in some measure its organic sensibility. Some one or more of the great organs of secretion now took on a secretory action. The nausea, the retching, and anxiety soon gave way to full vomiting—first of phlegm, and then of bile. Soon, a reaction took place, and the disease passed into the second stage; but the reaction was general,¹ and lost its ataxic or broken character; it was accompanied with a hot skin, violent pain, and a full, strong, tense pulse.”

Tartar emetic did not always produce vomiting. “In those cases in the first stage, in which the skin was cold, and even when the reaction was ataxic and scarcely perceptible, the organs as if palsied, secretion entirely suspended or strangely vitiated, the stomach irritable, and little or no pain complained of, tartar emetic in full doses might be given without producing vomiting. Strange as it may appear, tartar emetic, in such cases, was a most powerful stimulus; it brought out the excitement, heated the skin, raised the pulse, allayed the irritability of the stomach, restored sensibility to the organs, and finally awakened one or more of them—the kidneys, skin, &c.—to active secretion.”

Dr. Cartwright gave the remedy in doses of from three to ten grains every one, two, or three hours, in water or pills. I need scarcely remark that, so far, Dr. C. does not appear to have made many proselytes.

Purgatives.—We have seen that the use of emetics has, by an almost unanimous consent, been discarded from the treatment of yellow fever, and is only resorted to in a few cases to meet peculiar contingencies. Very different will be found to be the state of opinion relatively to the utility of purgatives. From an early period, they have been used in the fever of this city, and found advantageous under particular circumstances. The same favourable results have been obtained from the time of the first outbreak of this disease, and continue to be so to this day in other parts of this country, and in every other place where the disease has manifested itself. That by a few physicians among us, though principally among the French, these² medicines have been neglected or condemned as useless or hurtful, is true. But it is not less certain that their condemnation is, in great measure, based on purely theoretical grounds.

In no one instance do we find that an opinion adverse to them is founded on positive observation of any injurious effects that may have accrued from them when properly administered. According to some, they are likely to

¹ Medical Recorder, ix. 20, 21.

² Barton, p. 18; Warren, p. 38; Poissonnier, p. 57; Caillot, p. 294; Dalmas, p. 122; Rufz, p. 119; Rochoux, pp. 599, 643.

produce or aggravate irritation or inflammation of the bowels or stomach, and must therefore be set aside. Agreeably to others, the disease is of a nature forbidding the idea of its being benefited by the promotion of alvine evacuations. By another set, it is thought that the fever, being of an adynamic or typhoid nature, must necessarily be aggravated by the depletory action of purgatives; while by others, again, these remedies are proscribed without any assigned reason. Entitled, however, as some of those who condemn purgatives may be, and doubtless are, to our regard, their opposition, founded, as it is, on such unsatisfactory reasons, and not on the result of positive experience, cannot be adopted as a rule of practice; the more especially as the success of such physicians is not found to be superior, if equal, to that of others who follow a different course.¹ Their opposition to purgatives might have some show of foundation if it were possible to adduce in its support reasons such as are urged against the use of emetics. But these reasons are not applicable to the former. The stomach, we know, is often, in the early stage of the disease, in a state of irritation—or if not irritated, is easily made so. Such being the case, it may, *à priori*, be conceived that an effect of the kind will naturally be brought about by the acrid and stimulating nature of the emetic articles used, by the contraction of that organ itself, and by the pressure it experiences from the parts concerned in the act of emesis. Purgatives, on the contrary, produce comparatively little irritation in the stomach. They expend their action principally on the intestines, which are but seldom in a state of irritation or inflammation, while some of them at least, being found useful in cases in which such irritation exists, may readily be presumed to be admissible in the yellow fever, even were the intestines more inflamed in that disease than we know them to be. But as to these remedies, experience has led to very opposite conclusions. While it has pronounced emetics to be hurtful, it has shown that purgatives, if judiciously used, may prove highly advantageous. Hence, we find they have been employed, more or less extensively, and commended by the almost entire body of physicians in both temperate and hot latitudes.

In this country, purgatives were found highly useful by Dr. Mitchell, of Virginia, during the epidemic of fever which prevailed in his vicinity in 1741, and which by some has been regarded as closely allied to the yellow fever. From his communication it was that Dr. Rush acknowledges having derived the idea of resorting to the free use of active cathartics, which subsequently occupied a prominent place in the catalogue of means employed by him for the cure of the fever (iii. 128, 133). According to this eminent physician, the effects of purging, during the epidemic of 1793, were as follows: 1. It raised the pulse when low, and reduced it when it was preternaturally tense or full. 2. It revived and strengthened the patient. This was evident in many cases, in the facility with which patients who had staggered to a close stool, walked back again to their beds after a copious evacuation. It acted by abstracting excess of stimulus, and thereby removing the depression of the system.

¹ Museum, i. 12; Register, iv. 200, &c.

3. It abated the paroxysm of the fever. 4. It frequently produced sweats when given on the first or second day of the fever, after the most powerful sudorifics had been taken to no purpose. 5. It sometimes checked that vomiting which occurs in the beginning of the disease, and it always assisted in preventing the more alarming occurrence of that symptom about the fourth or fifth day. 6. It removed obstructions in the lymphatic system. 7. By discharging the bile through the bowels as soon and as fast as it was secreted, it prevented, in most cases, a yellowness of the skin.¹

Whatever we may think of some of the effects here assigned to purgatives, it is not the less a fact that much benefit was derived from this class of medicines by Dr. Rush during our various epidemics. By the late Dr. Monges, whose experience in the disease was most ample, and whose practical tact was unsurpassed, it is positively affirmed that, during the whole course of his long practice, he seldom saw a patient die of this disease whose bowels had been well evacuated within the first twenty-four hours after the attack.² Purgatives were also used by Currie (pp. 41, 42), Nassy (p. 34), Devèze (pp. 272, 284), Ffirth (p. 30), Caldwell, 1805, (p. 102), Barnwell (p. 379), Cathrall (p. 48).

In other cities of this country they have been found more or less beneficial, not only by Dr. Mitchell, already cited, but by almost every subsequent physician,³ and in the list of those who have encountered the disease in Europe and in tropical climates it is almost impossible to find one (except those I have mentioned) who entertain a different sentiment on the subject.⁴ It is not to be denied that by not a few in this and other countries too exclusive a reliance has been placed on purgatives, to the neglect of other means equally if not more important. Nor is it less probable that, on many occasions, purgatives have, by these and other physicians, been resorted to empirically, and when not required, that they have even been carried further than is warranted by the rules of prudence, and that they have thereby been productive of unnecessary annoyance and fatigue to the patient, or of irremediable mis-

¹ Vol. iii. 135, 223. See also iv. 22, 48.

² North American Med. and Surg. Journ., ii. 66.

³ Seaman, *Fev. of 1795*, in Webster, p. 43; E. H. Smith, *ib.* p. 132; Monson, *ib.* p. 186; Bayley, p. 98; Davidge, p. 121; Warren, in Tytler, p. 503; Shecut, p. 123; Valentin, p. 196; Heustis, p. 116; Harrison, p. 334; Dickson, *Eclectic Journ.*, iv. 114; *Ib.*, *Am. Journ.*, ii. 76; *Ib.*, *Elements of Medicine*, p. 276; Gros, p. 21; Townsend, p. 209; Hosack, *Essays*, iii. 430, 433; S. Brown, p. 65; A. Hosack, p. 28; Merrill, *Med. and Phys. Journ.*, ix. 251; *Ib.*, *North American Med. and Surg. Journ.*, ii. 227; Perlee, *Med. and Phys. Journ.*, iii. 15; Cartwright, *Recorder*, ix. 29, 31; Chatard, *Med. Repos.*, iv. 255; *Ib.*, *Marseilles Journ.*, v. 337.

⁴ Wallace, xlv. 278, 279; Arnold, p. 47; J. Clark, p. 25; Dickinson, pp. 151, 157; Hillary, p. 161; Holliday, pp. 9, 10; Moseley, p. 452; Hume, pp. 209, 238; Mackintosh, p. 66; Williams, p. 41; Musgrave, ix. 137; Bally, p. 485, &c.; Imray, liii. 86; *Ib.*, liv. 329; Dariste, p. 195; Osgood, p. 45-6; Ralph, ii. 85; Gilbert, p. 83; Bancroft, p. 64; Bruce, p. 280; McArthur, pp. 351-52; Gilkrest, ii. 281; O'Halloran, p. 90; Pariset, p. 581; Tagart and Hackett, in Gilkrest, p. 282, &c.; R. Jackson, i. 276, &c.; H. McLean, p. 137; Macmillan, *Edin. Journ.*, x. 31; Copland, iii. 184.

chief. The medical annals of this city might, I doubt not, furnish facts to justify the above admission; and others equally strong might easily be pointed out elsewhere.

Among some extraordinary instances I could mention, I select the following from Dr. Dickson: "The absurd extent to which it was carried (at one time at Charleston), even by medical men, will be illustrated by a single fact. I was called to see a patient, in consultation, in 1824, on the third day of his illness, and but a few hours before his death, who had been purged by this mixture (solution of Glauber or Epsom salts in infusion of rad. seneka), as a careful attendant assured me, one hundred and forty times. The poor fellow himself affirmed that he had not been off the chair five minutes. These statements were fully confirmed by his physician."¹

In regard to these, as, indeed, to other remedial means, experience has taught the necessity of avoiding all idea of exclusiveness—of regarding them as useful, to meet certain indications; not as the sole instruments of relief, and, at the same time, of resorting to them, not in a blind, empirical, and unlimited way, but with moderation and prudence, and under particular and suitable circumstances. When so employed, purgatives will very generally be found to fulfil useful indications. When, on the contrary, attention is not paid to these particulars, the expectations of the physician will, in all probability, be foiled.

As a general rule, it may be regarded as safe and proper to administer purgatives every day until copious and proper evacuations have been procured, or until it is seen what effect the latter are likely to produce on the disease. Care must be taken, in the inflammatory form, not to administer them before reaction is established in the system; as they are otherwise apt to produce watery stools, and to do harm by preventing that reaction, and thereby endangering the life of the patient. When, on the contrary, they are used during reaction, they aid in subduing arterial excitement and in enabling the powers of the system to do their share in the restoration of health.

Nevertheless, though the period at which cathartics are most advantageously employed is that at which bleeding, general or local, is most appropriately used; and though, in some cases, these two means may go hand in hand, yet in all instances in which arterial excitement runs high, or in which, with or without energetic action of the heart and arteries, symptoms of gastric irritation are evident, it is both safer and more advantageous to precede the use of purgative medicines by venesection, when it is called for, or by the application of a sufficient number of leeches or scarified cups to the epigastric regions, as also by other sedative and tranquillizing means, to which attention will be called presently. In other words, it is necessary in such cases to bring back the over-irritating organs, by depletory and sedative agencies, to what has been called the secreting point; as otherwise an effect very different from that looked for will likely be produced. Instead of diminishing, we might aggravate the excitement of the system and the irritation of the gas-

¹ Dickson, i, 368.

trie organs; and in lieu of obtaining healthy, we might promote the discharges of bloody and watery, matter, from which more harm than benefit would accrue.

In many cases, on the contrary, when vascular excitement is not sufficiently energetic to require bloodletting; in those also in which topical depletion alone is required, or where both can or must be dispensed with; in other words, in the milder cases of the inflammatory form, purgatives, when properly and cautiously administered, will be found an excellent substitute, and sufficient, by themselves, to produce the desired effect of removing or moderating the excitement.

By not a few physicians—of experience, too—purgatives are especially used at the decline of the stage of reaction when the irritation of the system begins to subside.¹ The object, with some of them is, while avoiding the irritating effects such articles are supposed likely to produce during the existence of that stage, to promote the establishment of a critical movement, which not unfrequently takes place by the intestines, or to establish, as it were, a crisis of an artificial character. There is no doubt that much advantage may be gained by aiding nature in establishing such a critical movement where signs of her intentions to that effect show themselves, and with that view to excite and keep up—within proper bounds, however—discharges from the intestinal tube. But there can be no reason to limit the use of purgatives to such a time and to such purposes; for experience, as we have seen, has fully shown that there is nothing to fear and much to gain from a judicious administration of them at an earlier period of the attack. It is not certain, indeed, that, as a general rule, it is not safer to have recourse to such medicines earlier than at the time mentioned; for, except in very mild cases, when the disease ends at the close of the stage of reaction, and when, as a consequence, further medication is seldom necessary, the irritation and inflammation of the stomach and upper intestines are, in the large majority of cases, much more severe after the third or fourth day than they were before; and those organs must be in a less proper condition to bear with impunity the impression of purgatives.

Be this as it may, however, after the inflammatory stage has subsided, the bowels must be kept open by purgatives, if they are admissible, or, if otherwise, by other means. For this purpose, enemata are highly useful, and should be resorted to freely. They are recommended by most writers.² They prove useful in the first stage by promoting the operation of purgatives, and are of especial advantage when, from irritability of the stomach, alvine evacuations cannot be obtained through means of cathartics. Indeed, by some physicians, they are preferred in almost all cases to purgatives, and used to the complete exclusion of these. It is more than doubtful, however, from what has already been seen, whether such ultra views can be admitted as correct.

¹ Devèze, p. 272; Dariste, p. 195.

² A. Hosack, p. 37; Wood, i. 310; Ralph, ii. 85; Monges, ii. 66; J. Clark, p. 256; Blair, p. 111; Rochoux, p. 599; Maher, p. 904; Gilbert, p. 89; Bruce, p. 280; Caillot, p. 312; Brown, p. 67; Barton, McCormack, &c.

Purgatives have also been found useful in the congestive form of the disease—acting powerfully in relieving the system from the oppression under which it labours, and in equalizing the excitement. In such cases, they have been administered till the secretions assume a healthy appearance, or, at least, approach to that condition.¹

The objects to be attained from the use of purgatives are various. They act by clearing the intestines of the depraved secretions and irritating matters they contain; they are useful by the depletory effects they produce; as also by the counter-irritation they occasion; by their agency in correcting the secretory process in the intestines and annexed organs, and especially by their influence in promoting the formation of a plentiful supply of bile, which, in that disease, is usually more or less suspended.

By many practitioners, cathartics are given, principally with the view to elicit the evacuant, depletory, and counter-irritant effects these articles tend to produce. Hence, they prescribe mild purgatives, or those which, while producing free evacuations, do so without causing much intestinal irritation.² Dr. Monges states that senna tea, acidulated with lemon-juice or tamarinds, answers sometimes remarkably well, when the stomach can retain the mixture. Castor oil, manna, salts, or magnesia are also found advantageous, and were by him and others freely employed. If by this class of physicians mercurial purgatives are resorted to, it is principally on account of the smallness of the bulk in which they produce their effects, and the greater chance there is of their being retained in the stomach.

With other physicians, the main object, after emptying the bowels, is to modify the secretory process in these, and to promote the flow of bile, the presence of which they justly hail as an almost sure evidence of returning health. For this purpose they have recourse to calomel, either alone or united with or followed by other medicines calculated to expedite its purging effects. Dr. Rush employed this remedy during the epidemic of 1793. He gave it in combination with jalap, in doses of fifteen grains of the latter to ten of the former, repeated several times a day until four or five large evacuations from the bowels had been procured, and then followed it up by milder purgatives (iii. 128, 129). When the bowels were very sluggish, he gave the calomel in combination with gamboge, two and a half grains of each every six hours, until they produced five or six stools.

As every one knows, the practice of resorting to calomel for purging purposes was not original with Dr. Rush. Besides its being used, in fevers of kindred character, by Dr. John Clark, Sir J. Pringle, Dr. Cleghorn, Dr. Balfour, and some of the older physicians of this country, it was resorted to, in yellow fever itself, by Dr. Lawrence, of New York, in 1791, as also by Chisholm, Wright, and Smith, of the West Indies, &c. But, however this

¹ Wallace, p. 279; Merrill, ix. 252; *Ib.*, ii. 232; Wilson, Cartwright.

² Devèze, p. 284; Pariset, p. 581; Gilbert, p. 83; Bruce, p. 280; Hosack, iii. 433; Bally, p. 485; Barnwell, p. 379; Valentin, pp. 196, 204; Gros, p. 21; Monges, ii. 66.

may be, the practice subsequently became very general, both in this country and in tropical climates.¹ By many, calomel has been administered in very large doses, and persevered in until an action on the liver, and green, glairy, curdled or jelly-like stools, or dark-yellow bile has been procured. Dr. Merrill gave it in scruple doses, more or less frequently repeated; Dr. Wurdeman gave ten grains every two hours; Perlee, ten grains every hour. Dr. Blair used it in combination with sulphate of quinia, in the proportion of twenty grains of the former and twenty-four of the latter, every six hours, as long as the stage of reaction lasted.

Dr. Stone, of Natchez, and formerly of Woodville (Miss.), believing calomel to produce a sedative effect when given in large doses, used it with this view, as well as to regulate the secretion of bile, during the epidemics of the latter place in 1844, and of the former in 1848. He administered from forty to sixty grains at a time. "The quantity," he says, "should be sufficient to be *sedative*; and if one hundred grains should be required for this, then that would be the proper quantity." "It allayed the irritability of the stomach, promoted the secretion of bile if it had been suspended, or moderated it if it had been profuse." "One full evacuation of dark-yellow bile was generally produced by it within six or eight hours; and at this time senna and manna or castor oil was required to cause four or five operations of almost pure bile, or mixed with dark mucus" (p. 188).

No one doubts that whenever in yellow fever the biliary secretion, after having been suspended—as is most generally the case—becomes re-established, the sign must be hailed as of good omen; for the fever, pain, oppression, thirst, and restlessness then abate, secretion in the other organs shortly succeeds that of the liver, and the disease soon yields. Certainly if calomel, given in large doses, can be instrumental in producing this effect, it must be regarded as an invaluable remedy, and very generally prescribed. But it is a question which admits of some doubt, whether, when the secretion of the liver becomes re-established, the effect is due always, or generally, to the large doses of calomel employed, or whether it is not simply the result of a return of health, obtained through the curative efforts of nature, aided by remedial means, which might just as well have been selected among articles of a less heroic or revolutionizing character. Though friendly to the use of calomel as a purgative in yellow fever—though aware of its soothing effects and of its power of controlling irritation, and little prepared to join with those who fear its effects when used with due discretion—I am no advocate of this profuse employment of the remedy.

The physician must have other objects in view, in the treatment of yellow fever, besides the excitement of the biliary secretion. It is not certain that the restoration of that secretion is much more essential than that of the cutaneous secretion; and those who would maintain that as the latter returns to its normal

¹ Merrill, ix. 251, ii. 227; Kelly, xiv. 390; Imray, liii. 86; Ib., lxiv. 329; Cathrall, p. 49; Ffirth, p. 30; Currie, pp. 41, 42; Perlee, iii. 15; Osgood, p. 45; Ralph, ii. 85; Townsend, p. 209; Gilkrest, ii. 281; Wood, i. 310; Stone, p. 187; Wurdeman, Am. Journ. Med. Sci., N. S., ix. 53; Blair, p. 107; J. Clark, p. 25; Amiel, xxxv. 280; Dickson, Elements of Medicine, p. 276.

state when recovery takes place—and mischief is often produced by an attempt to force it—so the biliary secretion will observe the same course, and that it may be hazardous to urge its restoration, would perhaps not be far from the truth. If this be admitted, it is impossible to feel disposed to advocate the very profuse use of a remedy which, however harmless it may be when employed with moderation and care, is one of serious import when thrown into the system in the large quantities mentioned. To this opinion I am the more inclined to adhere, as it will be found that the calomel practice, pushed to the extent alluded to, does not appear to recommend itself by any remarkable amount of success obtained from it; that it has lost much ground in this country, and that everywhere, among the best professional authorities, the number of its opponents has always been, and continues to be, large.

By those who advocate the use of calomel in the way mentioned, reference is made to the benefit obtained from it in dysentery, in which considerable inflammation of the intestinal tube exists, and in which the effect produced is of a soothing character. We have seen that Dr. Stone resorted to it in large doses, in great measure from a belief that it acts as a sedative. Let it be remembered, too, that Dr. Annersly¹ thought himself justified in concluding, from experiments he instituted on the subject, that calomel was really a direct sedative to the inflamed mucous membrane of the digestive organs. The reader may recollect, in addition, the statement of Dr. Beaumont, relative to the effect of calomel on the stomach of his extraordinary patient Martin. He perceived that in cases of indisposition the organ ceased to secrete gastric juice, became unusually florid and dry, and was often marked with numerous red pimples, filled at times with a secreted fluid. This state of the membrane was removed, and health restored, by a full dose of calomel and of aloes, either swallowed or introduced through the artificial opening into the stomach.²

While, therefore, it will be proper to use calomel, in ordinary purging doses, for the purpose of exciting gently the secretions and evacuating the bowels, and to aid it by mild, but efficient, purgatives, it will be safer to avoid a profuse administration of the remedy, and even to abstain from it altogether when the object can be obtained with other means.

In latter times, much has been said of the efficiency of certain purgatives, either as possessing a specific influence over the disease; or for reason of the particular nature of their cathartic effect; or, again, in consequence of their supposed power of controlling the existing irritation, local or general. By some, especially by the Spanish physicians, olive oil has been highly extolled. Father Jos. Constans acquired a great reputation in Spain, some thirty years ago, in the treatment of the yellow fever, and it is well known that his chief reliance was in the substance in question, which he gave in large doses.³

More recently, the croton oil has been introduced, and recommended as

¹ Sketches of the most prevalent Diseases of India, pp. 389, 390, &c.

² Caldwell's review of Beaumont, *Transylv. Journ.*, vii. 370.

³ Audouard, p. 306.

an invaluable remedy. It has been used with advantage by Dr. Cartwright, of Natehez (p. 31), and by Dr. Imray.¹ Mr. Tagart, of the British medical staff,² gave it in the dose of a drop or two on the tongue. In his hands, it not only excited the immediate action of the bowels without increasing the irritability of the stomach, but also favoured the secretion of the kidneys.

Another British physician, Dr. Hacket, who saw much of the disease in the West Indies, speaks also very favourably of the effects of this article. When, after bleeding, sulphate of zinc as an emetic, enemata, and castor oil, there was much excitement, croton oil was invariably given to the extent of three or four drops. Dr. Hacket remarks, "I have known this repeated thrice through the night; and it is most worthy of remark, that the more irritable and distressed the stomach, though *primâ facie* to those unacquainted with the great febrifuge virtues and extraordinary powers of croton oil in restoring the peristaltic motion of the intestines, which seems in other diseases to be inverted altogether, this irritability, hitherto our bane—I may say the very leading feature and peculiarity of tropical fevers—would be to them a cogent reason for not administering the oil, yet in almost all such cases it was found invariably to be triumphant; so that in the morning we generally found our patient, thus treated, with a perfect or nearly perfect remission." He says a little further on: "The power of croton oil in allaying gastric irritability and general nervous excitement, as well as restoring the circulation to the surface, and thus relieving the congestive state of the internal and deep-seated central vessels, is really extraordinary; and though it may seem for the moment, when first given, to increase that irritability, yet, after a little time, I have hardly ever seen it fail in producing the desired end."

Much as we are taught by experience not to be too confident in our expectations of the efficacy of medicines from the advantage which may seem to result from their employment in particular instances, and the praise bestowed upon them by respectable physicians, we cannot but admit that the above statements relative to the effects of croton oil, made as they are by a gentleman who has had an ample field of observation, are entitled to serious consideration.

Mercury.—During the epidemic of 1793 in this city, mercury, as a salivator, was introduced in the treatment of yellow fever. It was first used by Dr. Rush (iii. 156) in consequence of his having derived good effects from it four years before in the treatment of the bilious fever, then generally prevalent in the city; and also of its being advocated in fever by Haller, in his short account of the works of Dr. Cramer,³ and by Dr. Clark⁴ in dysentery. Encouraged by the good effects observed on that occasion from a salivation, Dr. Rush was induced, the next year, to excite it as early as possible in all those cases which did not yield immediately to bleeding and purging. He was delighted to find that it immediately attracted and con-

¹ Edinb. Med. and Surg. J., liii. 86; lxiv. 329.

² Med. and Phys. Journ. Aug. 1825.

³ Biblioth. Med. Pract., iii. 491.

⁴ Observations on Diseases which prevail in Long Voyages, ii. 334.

centrated in the mouth all the scattered pains of every part of the body, checked nausea and vomiting, and gradually, when it was copious, reduced the pulse—thereby preventing the necessity of further bleeding and purging (iii. 225). He used mercury still more extensively in 1797 (iv. 23, 24); and during the memorable epidemic of 1798, a salivation was found by him “to be the most certain remedy of any that was used in this fever,” for he “did not lose a single patient in whom the mercury acted upon the salivary glands” (iv. 47).

It is scarcely necessary to remark, that the employment of mercury as a salivant in the yellow fever was neither original with, nor peculiar to, Dr. Rush. Dr. Lind (*Diseases of Seamen*, p. 267) speaks of it as familiar in the West Indies before his time. Warren, many years anterior to Lind, speaks in terms of censure of a “very odd and unwarrantable practice for many years among several of the plantation practitioners, in Barbadoes, of giving calomel in inflammatory fevers” (p. 33). Dr. Wright began the use of mercury in the cure of the acute diseases of Jamaica in 1764, and attributes the introduction of the practice to a Dr. Smith, of Savannah la Mar, who used it long before that time.¹ Chisholm speaks of calomel being the only efficacious remedy in a most destructive yellow fever which prevailed in Grenada in 1788 among the soldiers of the 45th British regiment.² Dr. C. Maclean used mercury in a case of yellow fever (in Jamaica) in the year 1790, having already used it at sea in 1789 in a case of intermittent fever;³ and in 1793 it constituted the principal remedial agent in the hands of Dr. Chisholm,⁴ in Grenada, and of Dr. J. Clark, in Dominica.⁵

As to the controversy which soon arose respecting the claims of originality, so far as the introduction of the remedy is concerned, little need be said. In the first edition of his work on the epidemic of Grenada, Dr. Chisholm claimed the practice, regretting that Dr. Lind had neglected to give his authority for the statement above alluded to, and roundly asserted that no such practice was ever known before his own period in those islands, nor in Jamaica, “if we judge by the silence of medical practitioners of those islands who have written” (1 ed. p. 160). He subsequently discovered his error, and in his second and enlarged edition, recalled his pretensions (i. 423). On the other hand, Dr. Charles Maclean, in the volume already referred to, as well as in the preliminary discourse to his large work on epidemic diseases (i. 130), sets forth his claims to the introduction of the remedy. He rates Dr. Bancroft roundly for having attributed the priority in that matter to Dr. Chisholm, or rather for having referred to the latter as the first who made use of mercury as a salivant in the disease; grounding his claims on his having resorted to it three years before Dr. Chisholm, and laid before the public the results of

¹ Quoted from Chisholm, i. 422.

² *Ib.*, i. 423; Duncan's *Med. Commentaries*, xviii. 288.

³ *Practical Illustrations of the Progress of Medical Improvement for the last thirty years.* Lond., 1818.

⁴ *Op. cit.*, i. 350, &c.

⁵ *A Treatise on the Yellow Fever as it appeared in the Island of Dominica*, pp. 36-7.

his experience in *A Treatise on the Action of Mercury on Living Bodies*, which appeared at Calcutta (India) in 1796.¹

But whatever may be thought of the omission of Dr. Bancroft in not referring to the production of Dr. Maclean, and to the latter having used mercury several years before the epidemic of Grenada, it is not less certain that this physician is not entitled to the credit he seeks on this occasion, for he was, as we have seen, anticipated by several others, and by Dr. Chisholm himself. As regards Dr. Rush, he may have been acquainted with the statements of Lind and Warren on the subject, and he admits his having seen, soon after, Dr. Chisholm's account of the fever of 1788; but he could not have had any knowledge of either the same physician's history of the fever of Grenada, of Dr. Clark's details concerning the epidemic of Dominica, or of Dr. Maclean's work above alluded to, as these were published the first in 1795, and the two others in 1796.

But whether the mercurial practice in this city originated or not with Dr. Rush, certain it is that not a long time after, that practice became very general—in great measure through his influence—not only in this country, but in tropical regions and in Europe, and elicited the warmest eulogiums from high professional authorities. “Our greatest dependence,” says Dr. J. Clark—“or, in the nautical style—our sheet-anchor, was mercury.” All other means were collateral. Salivation was obtained by calomel and mercurial frictions (*op. cit.*, pp. 36–7). The practice met the approval of Dr. Ferguson, who held that mercury, when “not empirically used, but duly applied at the proper stage and period,” is “a great, and probably the only remedy upon which much reliance is to be placed” (p. 189). “Mercury, during the violence of the first symptoms, would, in all probability, have done mischief by increasing the effect of general stimulation; but in the second stage, it must, from its known qualities, have powerful effects in rendering pervious the obstructed viscera: without it, I believe that tonics and stimulants would only do harm by rousing the system into undue efforts, from which it must sink exhausted; but duly combined, the vital powers may be made to go hand in hand with the artificial action of the mercury.”²

In mild cases, the late Dr. Waring, of Savannah, who long and deservedly enjoyed considerable reputation in our Southern States, relied on the mild antiphlogistic plan. “But when,” as he says, “the force of fever becomes higher and more destructive, it fails so often as to produce the necessity of employing more efficient means. It leaves so much to be done by the powers of nature—for really it professes no more than the removal of obstacles to the curative process, which is performed or attempted by them—that they cannot be confided in exclusively. The powers of nature, in such cases, are too feeble to resist and subdue the disease with their assistance alone. A remedy is demanded which can cut short or mitigate it, by translating excitement from

¹ Reprinted in Philadelphia in 1797, together with “A View of the Science of Life,” and “A Dissert. on the Source of Epid. and Pest. Diseases,” by the same author.

² Med.-Chir. Trans, ii. 189, 190.

a part which is so vital to one which is less so. *Sthenic* medicines aggravate the evil; *asthenic* medicines cannot reduce it, notwithstanding any reduction of the fluids or excitement; and blisters are incapable of diverting it. Under these circumstances, mercury has been resorted to with the most happy results; and although it does not succeed universally, it has filled up a considerable space in the deficiency of remedies" (p. 66). Dr. Waring further states that mercury, when it fails, does so in cases which would have resisted all other modes of treatment; but that it certainly does occasionally succeed in desperate cases, and that on trial it will be found "there are cases which tend to the destruction of the patient, which are seasonably mitigated or broken up by the influence of the medicine before they arrive at their utmost violence, and become so firmly seated as to preclude its operation on the system altogether" (p. 68).

Another of our best southern medical authorities, Dr. Dickson, of Charleston, relies almost exclusively on the effects of mercury, and speaks of its curative powers in the most eulogistical terms. "In the whole practice in yellow fever which I have seen, I have not noted, nor do I recollect a single case, in which ptyalism was induced during the continuance of the first stadium or febrile paroxysm, which terminated fatally. Nor are examples wanting of an abrupt check being given by this means to the progress of the disease, after it had advanced into the second stage, new life being thus aroused in the prostrate constitution."¹ In one of his late publications, the same distinguished physician remarks: "It has always required some moral courage to avow one's self a mercurialist; but of late, since the loud Thomsonians have joined the cry against the mineral, it has been deafening. For my part, *odi profanum vulgus et arces*, I shall never abandon the most powerful article of the materia medica because it has occasionally proved too powerful for the skill of the physician, however sagacious, or for the impaired constitution of a patient prostrated by the most malignant form of fever."²

Much to the same effect is the opinion as to the high value of mercury recorded in other professional writings of this and other countries.³

Dr. Gilkrest thus sums up what he considers the state of professional opinion on the subject: "On a review of the different modes of practice adopted in this proteiform disease, within the last forty-two years, by practi-

¹ Dickson, *Elements of Medicine*, pp. 276-7. See also *Am. Journ. of Med. Sci.*, ii. 74.

² *Bell's Eclectic Journal*, iv. 115.

³ Stuart, a *Dissert. on the Salutary Effects of Mercury in Malignant Fevers* (1798), in *Caldwell's Select Theses*, ii. 53; Warren, of Boston, in *Tytler*, p. 504; Cartwright, *Recorder*, ix. 31; Merrill, *Med. and Phys. Journ.*, ix. 253; Washington, *ib.*, vi. 129; Perlee, *ib.*, iii. 15; Davidge, p. 119; Wood, i. 310, 311; Willey, *Med. Repos.*, xii. 228; Rand, *ib.*, ii. 451; Monro, *ib.*, iii. 137; Selden and Whitehead, *ib.*, iv. 4; *Ib.*, vi. 251; Dwight, *ib.*, viii. 237; Hall, *ib.*, vii. 22; Kollock, *ib.*, iv. 4; Holt, *Fev. of New London in 1798*, p. 14; Potter, see *Barrington, Am. Journ.*, xii. 317; Wurdemann, *Am. Journ.*, N. S., ix. 53; Wallace, *Edinb. Journ.*, xlv. 279; O'Halloran, p. 94; Boyle, *Fev. of Coast of Africa*, pp. 175, 235, 288; Linton, in *Gilkrest, Cycl.*, ii. 232; Amiel, *Edinb. Journ.*, xxxv. 280; T. Smith, *ib.*, p. 43; Wilson, p. 12; Comrie, *Edinb. Journ.*, xiii. 167; Heustis, *A Treatise on Yellow Fever*, p. 93.

tioners in the British West India Islands, the United States, and Gibraltar, this remedy seems to have best maintained its ground ; for though it be quite true that it has from time to time fallen into discredit from persons having, in the course of an epidemic, frequently found that, like all other human means, it made no impression on the most aggravated forms of the disease, it nevertheless has stronger testimony in its favour than any other practice which can be named."¹

Dr. Chisholm resorted to mercury from its known specific effects in derangements of the liver, an organ he found diseased in those who had died of the yellow fever; from its effects in the fever which had prevailed in Grenada several years before; and because other means having failed, he thought it better to try a doubtful remedy than to continue to use others of no efficacy.² Dr. Rush used it in the first stage as producing a counter-irritation on the salivary glands, and in the second stage he prescribed it as a stimulant. Both these physicians, and those who succeeded them, were encouraged in the practice by the apparent success obtained; averring that none die in whom salivation is excited; and that it restores to health many who otherwise would have fallen a victim to the disease. It is said to be especially called for when, by other means, no action is produced on the liver, as evinced by cream-like or clay-coloured stools, and when the general symptoms continue with unabated violence.³ In the inflammatory form of the disease, it is not prescribed before the excitement of the vascular system has been subdued by suitable means, or has run itself down. The ptyalism obtained in the former case, is said to be attended with a soft skin and pulse, with a free secretion of bile and urine, and with a return of the organic sensibility of the system. But if the reaction is not subdued, mercury often produces blackness, succeeded by sloughing of the gums, accompanied with a perpetual oozing of a considerable quantity of blood; or a great swelling of the gums and salivary glands, partial ptyalism and hemorrhage, which subsiding are succeeded by a burning heat, an alkaline breath, and dryness of the mouth, &c.⁴

In the congestive form, in which it is highly recommended by Dr. Wallace⁵ and others, it is given from the outset, when an effort should be made towards mercurializing the system. Dr. W. regards it as possessing sovereign power in such cases; and, while admitting that it is sometimes impossible to introduce it, and, when introduced, it sometimes fails in its anticipated effects, he states that such is so seldom the case that we may "look to it with more confidence than to any; nay, than to all put together, of the remedies we are in the habit of employing along with it."

Another writer, already mentioned, looks upon it as the sovereign remedy in that form of the disease. "As affirmed by Armstrong, so I also have seen all the peculiar symptoms of congestion subside upon the earliest token

¹ Cycl. of Pract. Med., ii. 282.

² Op. cit., p. 158, 1st edit.; Ib., i. 423, 2d edit.

³ Kelly, Am. Journ., xiv. N. S. 391.

⁴ Cartwright, Med. Rec., ix. 31.

⁵ Edinb. Journ., xlv. 280.

of the mercurial affection—the weak pulse become fuller and regular; the cold harsh skin covered with genial moisture; the dry tongue softened, and the incoherent wandering cease; and the whole desperate condition of the sufferer improved into absolute convalescence.”¹

The quantity of mercury administered for the purpose varies much in different cases, and has occasionally, and particularly by some physicians, been pushed to a considerable extent. In some instances, when the system has been prepared, or the disease mild, salivation is easily and quickly obtained; but under opposite circumstances, the result is different. Dr. Chisholm, in 1793, gave it in pills composed of five grains of calomel, two of antimonial powder, and one of opium, repeated eight times in twenty-four hours. He frequently found it necessary to increase the dose and the number of the pills. In one case, he gave as much as four hundred grains (i. 352). In 1794, he commenced earlier and gave larger quantities than he had done the year before—ten grains every three hours till salivation was obtained; with opium to restrain its purgative effects. The first dose alone was combined with jalap (p. 353). Dr. R. Jackson says: “The quantity of a 1000 grains of calomel has often been given internally, ounces of ointment being rubbed at the same time upon the body externally.”² Dr. Arnold saw as much as 400 grains, and, in one case, 700, administered to one individual (p. 49). At Tortola, as much as 1100 grs. of mercury have been administered. At Port Royal, the quantity was pushed to the extent of 2500 grains.³ Dr. Warren tells us that in a case treated in Jamaica in 1799, the quantity did not fall short of 5,704 grains in the course of a few days—64 by the mouth, 2,040 by injection, and 3,600 by friction.⁴

In this country, such doses have not unfrequently been resorted to; and it was and is not unusual to aid the operation of the calomel by mercurial frictions—sometimes by causing the patient to wear socks filled with mercurial ointment, or throwing the latter up into the rectum in a suitable vehicle.⁵ Dr. Dickson gives 10 grs. \mathfrak{Zi} , or even \mathfrak{Zss} every two or three hours (i. 364). His object is to employ the due amount, and he thinks there is nothing to be feared from an overdose, except its rejection, or its purgative effects (i. 365).

But mercury as a salivant—praised, relied upon, and highly eulogized as it has been in the treatment of the yellow fever—has met from the very time of its introduction, and continues to meet, to the present day, with a decided opposition on the part of careful observers and experienced physicians, both in this country and elsewhere.⁶ Dr. Currie, who was a contemporary of Rush, disapproved of its use. Dr. Monges states that he never tried to salivate his patients. “In doing this,” he says, “I was not guided, however, by any fears of the effect of a salivation, since I was well aware that a ptyalism occurring in malignant diseases is often a favourable crisis; but by a knowledge of the great difficulty experienced in producing it, and from the

¹ Dickson, *Elements of Medicine*, p. 284.

² Sketch, &c., i. 287.

³ Moreau de Jonnes, p. 135.

⁴ Warren, on *Mercury in Febrile Diseases*. Boston, 1813.

⁵ Rush, xiv. 23.

⁶ *Fever of 1799*, p. 79.

observation, that in cases in which it was obtained much valuable time had been lost, and the patient might have recovered without."¹ Mercury was opposed by Dr. Ffirth (p. 32), and taken no notice of by others. As regards the epidemic of 1820, Dr. Jackson remarks: "The mercurial treatment certainly lost ground, and did not sustain the character that had been attributed to it. Many of the instances of its successful action were cases of a light disease, that would have probably recovered under any kind of treatment properly directed. There were, however, three or four, in which its beneficial operation was conspicuous. A struggle for mastery appeared to exist for a time between the disease and the remedy, while the fate of the afflicted patient hung in suspense. The establishment of a decided salivation became the harbinger of victory, and every untoward symptom disappeared. But in the far greater proportion of the more violent cases, and in those of the first class (congestive), its impression was not felt" (pp. 73, 74). It may be doubted whether any physician, in 1853 and 1854, thought it advisable to resort to mercury as a salivant.

In New York, mercury has found no advocate in Dr. Hosack,² or Dr. Francis.³ In Providence, it was condemned by Dr. Wheaton.⁴ The former remarks that this remedy has been so highly extolled in the treatment of the yellow fever, that it has been called the Samson of the *materia medica*. "In the yellow fever," he continues, "it has truly proved a Samson; for I verily believe it has slain its thousands. When salivation can be effected, doubtless it is oftentimes a useful medicine; but that effect is not to be obtained in one case out of ten, as far as I have had an opportunity of seeing this practice; and if it fails of producing a salivation, it so deranges the stomach and destroys the vital functions of the system, that no hope remains of recovery from the use of other remedies; even when salivation has been produced, in some instances the disease has proved fatal." Indeed, even in our southern States, where the mercurial practice has met with its warmest advocates, it has been found by many to be inefficient or injurious, and hence openly discarded or ignored.⁵

Dr. Barrington remarks, from the trials made on board the *Peacock*, &c., "that the mercurial treatment of yellow fever is not what it has been so long considered by a majority of writers on this disease, the only rational and successful mode of management. In many cases, the prevailing symptoms have been certainly aggravated, and new ones brought into existence. But I have also seen the disease 'broken,' as it were, on the appearance of a sore mouth or a flow of saliva; and 'post hoc, propter hoc' is always triumphantly applied to such fortunate coincidences."⁶

Dr. Wragg, one of our latest and most reliable authorities, in summing up the result of his experience in the Roper's Hospital, Charleston, during the

¹ North Amer. Med. and Surg. Journ., ii. 66. ² Med. Essays, iii. 431.

³ Townsend on Fever of 1822, p. 216.

⁴ Med. Repository, x. 338, xiii. 139.

⁵ Heustis, p. 117; Harrison, p. 324; Thomas, pp. 117, 118; Irvine, pp. 34, 518; Wurdemann, Am. Journ. Med. Sci., N. S. ix. 35; Stone, N. O. Journ., ii. 189; Daniel, p. 95.

⁶ Am. Journ. Med. Sci., xii. 325.

sickly season of 1854, remarks: "I never used calomel with the intention of producing salivation; and when I discovered symptoms of the setting in of this condition, I at once discontinued the medicine. My experience this season tended to confirm the views I have previously entertained in opposition to the ultra calomel plan of treatment; for though salivation occurred in a certain number of our cases, it was particularly remarked that no indication of benefit resulted, and several of the fatal cases were from among those who gave unmistakable evidence of mercurial impression."¹

A still later writer, in an essay on the epidemic to which Dr. Wragg refers, Dr. Chisholm, states that in Charleston the profession, with but few exceptions, were unanimous against the use of mercury, pushed to ptyalism, as fraught with danger. "Salivation is no longer looked upon as the great desideratum in yellow fever—the crisis from which health must be obtained. Too many cases were observed in which salivation had no control over the disease. Many cases died with the strong mercurial fetor on their gums. In some, death was attributed directly to the effect of mercury—diminishing the plasticity of the blood, and destroying life by profuse hemorrhages. Mercury, as formerly given, was altogether rejected."²

Elsewhere it has found no favour with Bancroft (p. 77), Musgrave (p. 140), Dariste (p. 203), Caillot (p. 290), Imray (liii. 86, lxiv. 329), Arnold (p. 49), R. Jackson (i. 217), Copland (iii. 184), Doughty (p. 62), Lefoulon (p. 340), Sheppard,³ J. H. Dickson,⁴ Macarthey,⁵ McCabe,⁶ Grant,⁷ and Bone (p. 48).

From all I have been able to observe myself, from the results of my researches and reflections on the subject, and from a due consideration of the nature of the disease, I am disposed to coincide with the opponents of the mercurial treatment. It is acknowledged on all hands, and the fact is too evident to be denied or doubted, not only that patients labouring under severe attacks of the yellow fever recover under the use of mercury, but that it is occasionally, perhaps in many instances, the instrument of relief. But it is not less certain, also, that mercury, under all circumstances, is an inconvenient and disagreeable remedy, and, besides, that it often produces effects of a highly painful, injurious, and dangerous character. Nothing, therefore, but a decided superiority as to the success of the remedy can justify its employment; for if we do not save more of such cases with it than we can without it, there is no reason for inflicting on the sufferer the inconvenience, and making him incur the risks, attending the employment of such a remedy. Nor is it less true that when high inflammatory symptoms prevail, or when the circulation is languid and the skin cool and dry, it is next to impossible to produce a salivation; that when we do succeed in obtaining this, effects are at the same time occasioned which enhance the danger of the case; and that the salivation affords no relief, and the patient, as Dr. Heustis remarks, dies, "notwithstanding the

¹ Charleston Journ., x. 84.

² Charleston Journ., x. 450, 451.

³ Edinburgh Journal, xiii. 427.

⁴ Ibid., ix. 58.

⁵ Ibid., xiii. 436, 437.

⁶ Reports illustrating the Diseases of Warm Climates, p. 88.

⁷ An Essay on Yellow Fever, p. 50.

appearance of this promising and wished-for symptom, or a slow and difficult recovery confines him for a long while to a sick bed" (p. 117).

Add to this that, excluding mercury from the list of our therapeutic means, we act more strictly in accordance with the state of our knowledge respecting the disease and the known effects of the remedy. The reader will recollect that the blood, from an early period of an attack of yellow fever, is seriously vitiated, and passes rapidly to a state of disintegration and destruction, which renders it inadequate to the performance of the functions assigned to it, and indeed incompatible with life. If, with this before him, he bears in mind that mercury has a tendency to produce effects of the kind; that it diminishes the plasticity of the fluid; that it breaks it down, as it were, and tends to hemorrhagic effusion—he will perceive that the remedy, instead of proving an antidote, must have the effect of enhancing the existing mischief, by increasing the virulence of the poison.

When we take these facts into consideration—the unpleasant and often injurious effects of the remedy; the uncertainty of being able to place the system under its specific influence; the probable failure, when obtained, of this influence effecting the object for which it was prescribed, and the tendency of mercury to diminish the plasticity of the blood; and when, at the same time, we bear in mind that, while trusting to this remedy, we are allowing moments to pass which might be more profitably employed—we cannot refrain from the conclusion that it is imperative in us, before aiming at mercurializing the system, to ascertain whether the advantages derived from the process are likely to outweigh the objections to its use, founded on the circumstances mentioned.

That the mercurial practice has been more successful than other plans of treatment, is a fact which nothing as yet adduced justifies us to admit. I am aware that it is generally asserted, by the advocates of the mercurial plan, that no one dies from the fever after salivation is fully established. The assertion, as already seen, is not altogether correct, for patients sink, though the system has been mercurialized. But, supposing it correct, we cannot derive from the fact an argument in favour of mercury; for experiments made on a large scale have shown that the success among those by whom mercury has been extolled is not greater than among those who have treated the disease differently. Dr. R. Jackson gives us to understand that it appears, by a reference to hospital case-books, that in one in three of severe yellow fever cases, in which calomel was given alone or with opium to a very large amount, it produced no increase of the salivary secretion, and consequently did not occasion the effect which controls the fatal tendency of the disease. The mortality, then, among such cases, was one in three; the other two-thirds, who had taken mercury and been salivated, having recovered. But he adds: "If it appear, through the same channel of information, that the same disease, when left to its own course, or opposed by ordinary means of treatment, does not destroy life to a greater extent than one in three, those most prepossessed in favour of the remedy will not, it is presumed, venture to maintain that we gain by the experiment; and it is evident that if we gain nothing

important, we lose time and chances of gain from other sources." So much for the more severe and concentrated form of the fever.

If now we inquire into the effects obtained in the milder form, or when mercury has been used in the severer form after preparation, we find the success more frequent than in the others, though not as uniformly so as is maintained; for even in these the effect sought for is not obtained. In such cases, when the fever is slight, the skin soft and moist, and the pulse yielding and free from tension, or when the symptoms are made so by previous treatment, the mercury easily takes effect; the mouth soon becomes gently affected, and, as this takes place, the remaining symptoms subside. But this change can furnish no indication of the utility of that medicine. Much more reasonable is it to say that it only evinces a mitigation of the disease, which permitted the mercury to show its specific effects, and that the disease would have been as readily cured had no salivant effect been induced.

To this conclusion we are led by the fact that so long as the disease is of a different character, when the reaction runs high or is deficient, it is impossible to excite a salivation, which, even when obtained, affords often no relief, or is not more efficient than other remedial means; and, I repeat, by the circumstance that the success of those who employ it, under the most promising circumstances, is not greater than, if as great as, that resulting from a different mode of treatment. On this subject we have the testimony of the highest professional authorities. Dr. R. Jackson, to whom I have just referred in regard to this matter, after noticing the difference in the effects produced by mercury, when administered with and without preparation by antecedent treatment, says: "The knowledge of this fact, obtained through observation of the progress of diseases, and confirmed by reference to case-books still preserved in the office of the Inspector of Hospitals at Barbadoes, warrants the conclusion that mercurial action, manifested by increased discharge of saliva, instead of being the cause which arrests or suspends the course of the disease by its own proper power, is no more than an indication that such suspension has taken place either by a process inherent in the constitution, or by forcible means of art. Mercury does not act whilst the disease exists in force; that is, it does not counteract the progress of the morbid cause while action produced by that cause is in progressive activity" (i. 288).

But whether this be so or not, certain it is that the result of the mercurial practice, as applied to the disease in all its various forms and varieties, is not such as to justify the praises it has received. Nowhere is the success of those who use it greater than that of physicians who follow a different course. Had its success here and elsewhere been conspicuous, it would not have been abandoned or avoided by competent practitioners, who not only were fully sensible of the many inconveniences it produces, but saw its failure in arresting the disease, and the injuries it often entails on those who recovered under its use. Its failure, during our epidemic of 1820, was notorious. Dr. R. Jackson remarks that, if we allow ourselves to estimate the value of the remedy by the success of the effort exemplified in the hospital returns of the person who first adopted the practice at Grenada in the year 1793, and of those, who

have pursued a similar practice in the different military hospitals in the West Indies since that time, the arguments for the continuance of the mercurial plan of treatment do not appear to be strong (i. 283, 290).

Dr. Wurdemann, of Charleston, states that the result in his hands of the mercurial plan was a loss of 1 in 7. All those who had the black vomit, died. He subsequently modified the treatment, and gave calomel merely to obtain the peculiar green tinge in the alvine evacuations indicating its action on the liver. Under this plan his loss was reduced to 1 in 9, and 7 who had black vomit, recovered.¹

CHAPTER XXXV.

TREATMENT—CONTINUED.

Diaphoretics.—Much benefit may be derived from a judicious and timely use of means calculated to excite perspiration. So advantageous, indeed, is, in general, a free discharge of that fluid, that by some practitioners an almost exclusive reliance has been placed on the means of obtaining it. Such was particularly the case among leading physicians in olden times, and even at a period not very remote from our own, when not a few resorted, from the outset of the attack, and under every variety of circumstances and conditions of the system, to the more stimulating diaphoretics and alexipharmics. By Dr. Warren, more particularly, this mode of treating the yellow fever was regarded as the safest and most beneficial, and was extensively used during the prevalence of the disease at Barbadoes from 1733 to 1738. The means employed were both internal and external. Discarding at the outset evacuates of all sorts, especially emetics, and limiting himself to a “moderate draught” of blood from “the arm or jugular, proportionable to the constitution and other circumstances of the sick person,” he ordered the patient to be warmly covered with a coverlet or blanket, and to take plentifully of alexipharmic juleps and warm possets (pp. 53, 55). “When,” he says, “the eruption of sweat is, by these means, continued in a copious and profuse manner for twenty-four hours, or more, it seldom fails to assuage the fury of the fever, gives a sensible relief to anxiety, and usually prevents or overcomes the great propensity to retching and vomiting, jactitations, yellowness, sickness, &c. But I generally endeavour to keep up the sweat or moisture of the skin (though in a more moderate manner) for two or three days longer, and a good diaphoresis during the whole remaining course; which is easily done by frequent supplies of the aforesaid liquors” (p. 56). Dr. Warren, while recommending and eulogizing this method of treatment, affirms that, though

¹ Amer. Journ. of Med. Sci., N. S. ix. pp, 54, 55.

it may be said to be alexipharmic, yet it is by no means hot, parching, or inflaming; and that the moisture or sweat he endeavoured to "extrude," is rather to be attributed to the quantity of tepid diluents, as sage tea, saek-whey, &c., than to the real heat of the medicines themselves (p. 61). Nevertheless, he recommended to those who could not command prompt professional assistance, to take, "upon the first invasion of the symptoms, a good dose of warm snake-root wine, then covering the body well in bed, and provoking plentiful sweats for the space of a day and night, by large and frequent draughts of sage-tea, or any other sudorific liquids" (p. 65).

Other authorities, of less remote date, might be cited in support of a mode of managing the disease not very different from that so warmly advocated by Dr. Warren;¹ but it is needless. Experience soon taught that from such a method of treatment little benefit could be expected; that the disease, from its sthenic character, in a number of cases, calls at the outset for means very different from those suggested by Warren; that in these and in others in which the reaction does not run so high, sudorifics should not be administered in the way recommended, especially before the system has been prepared for their reception by the removal of arterial excitement; that, under opposite conditions of the system, free perspiration is generally with difficulty obtained; and that when obtained, so far from conducing to beneficial effects, it produces very frequently, from the irritating nature of the means employed, an aggravation of the symptoms. I have already called attention to the deleterious effects of tartarized antimony, administered in emetic doses. Given with a view to a diaphoretic action, it is not less objectionable, in consequence of its increasing and keeping up the irritability of the stomach, a symptom it is so desirable to counteract. Combinations in which opium enters as an ingredient, with ipecacuanha and other remedies of sudorific power, are equally to be shunned, as they at first have a tendency to increase the heat of the surface, and occasion the ordinary effect of that medicine, determination of the blood to the brain, and its frequent attendants, coma or delirium.

Most other sudorifics possess a stimulating character, and have a tendency to aggravate the irritation of the stomach. For these and other kindred reasons, sudorifics have been objected to from an early period, in this city and elsewhere. In his Essay on the fever of 1799, Dr. Currie says that "the sweating treatment, by the means directed by Dr. Warren, of Barbadoes, cordial and stimulating remedies, and the external applications of heat and moisture, whether with or without a load of oppressive bedclothes," as far as he had been able to learn, "proved not only unsuccessful, but, in the majority of cases that submitted to it, was a speedy, though painful, passport to the grave" (p. 82). Nearly half a century before Currie, Moultrie (p. 20) expressed his disapprobation of the hot sudorific plan. Similar were the views of Dr. Williams, of Jamaica, whose volume on the fever of that island appeared in 1750, and who therein affirmed that alexipharmics "hurry on the inflamma-

¹ Savarésy, p. 318; Pugnet, p. 367.

tion to a gangrene." "Surely," he continues, "were these advocates for sudorifics acquainted with the animal economy, would they, in ardent, rarefactive, and inflammatory fevers, administer the vinal, spirituose, and volatile medicines, &c., which do so often, by the injudicious administration of them, bring on gangrenes, delirious frenzies, apoplexies, or hemorrhages?" (P. 34.)

Views of similar import have been advanced by Devèze (p. 286), Dariste (p. 203), Desperrier (p. 86), Jackson (i. 280), Bancroft (pp. 73, 74), Bcugnot,¹ Burnett (p. 33), Pariset (p. 583), Audouard (p. 316), Chisholm (i. 348-350), and others. Everything indeed tends to show that those who recover under the plan, do so not so much owing to, as notwithstanding, its agency. If useful, it is only, and can only be, in atonic cases, when the pulse bears little or no mark of excitement, and is withal feeble; when the skin is cool, flabby, and torpid; and when, from these and other kindred symptoms, we may presume that stimuli are called for. Under such circumstances, it becomes a desirable object to arouse the torpidity of the skin, increase its temperature, and promote its secretory functions; and as warm and stimulating sudorifics and external means, while contributing to the production of these effects, have, at the same time, a tendency to excite the circulation, they will be found of useful application, and are often indispensable.

Nor is it less true that, even were the sudorific plan of treatment more frequently harmless, in ordinary cases of yellow fever, than it has proved itself to be, experience has shown that it is not necessary to the attainment of the object in view, and that it may, therefore, be safely dispensed with. Many patients get well without a critical or free discharge of perspiration; and, on the other hand, it not unfrequently happens that a natural and copious perspiration ensues from the use of means calculated to reduce the general excitement of the system, and remove the excess of cutaneous heat. "I have frequently observed," says Dr. Williams, "that after the use of cooling, diluting, relaxing, and solutive medicines, a sweat would break out spontaneously, to the great ease and relief of the patient." Again, the same writer states that "large draughts of cold water, or other cool liquors, have occasioned profuse sweats in fevers when all the sudorifics in the shops would not have had the same effect" (p. 32). Few physicians conversant with the disease will feel disposed to gainsay the Jamaica practitioner. The relaxing and diaphoretic virtues of depletory means are too well attested to need a detailed notice here; while the agency of cold ablutions or immersions, in producing the like effects, has been recorded by the best writers on the subject.²

The objections urged against heating and stimulating sudorifics, or against any attempt to force out free perspiration at an early period of the disease and during an excited state of the system, do not apply to the milder diaphoretic agents, internal and external, especially when these are used with caution, in cases unattended with much arterial reaction, dry and parched skin, and local determinations, and particularly when they are preceded by proper preparatory means. Especially are they unobjectionable when nature seems to

¹ New Orleans Med. Journ., i. 9.

² Bancroft, p. 73.

indicate a resort to them. Cases present themselves, more frequently in some epidemics and earlier in some instances than in others, which are characterized by a more or less decided disposition to a cutaneous discharge; while in some a copious perspiration breaks out spontaneously. In many of these cases the discharge proves critical; or, if not, must be viewed as of favourable omen, particularly when it occurs early, is diffused over the whole surface, and is accompanied with an abatement of the main symptoms. It is to be viewed "as the most agreeable evacuation to nature, and the surest termination."

This appearance of perspiration, or even of copious sweat, takes place occasionally a short time after the commencement of the attack, sometimes after some days, and is preceded by more or less febrile action, as characterized by heat, dryness of skin, and strength, fulness, and quickness of pulse. In other instances, the skin is relaxed and moist from the outset of the attack, and associated with more or less arterial excitement. To counteract the former symptom, and accelerate the relaxation of the surface, or promote it when already present, sedative, refrigerating, depletory, and evacuant means, graduated to the state of excitement, and other circumstances, should be premised. When the cutaneous discharge, at whatever time occurring, takes place, or even before, when, by the above remedies, the pulse has been sufficiently reduced, it must be encouraged or excited by means, internal and external, calculated to produce those effects, without, however, occasioning the detrimental results occasioned by heating sudorifics. Indeed, in many cases, when the excitement does not run very high, and the local determinations are not formidable, little preparation need be employed, and the mild sweating plan may be resorted to at once. Of the means calculated to attain the object, the most useful are the warm and tepid bath, partial or general; pediluvia of simple water, or mixed with mustard or ashes; fomentations with simple water or aromatic herbs; warm and slightly aromatic drinks; the artificial vapour bath; and saline diaphoretics. Of the safety and propriety of these means, alone or in combination with others, and of the curative method under consideration, there can be no possible doubt. This method is based on ample and correct experience of its results, and is commended to our notice by the highest professional authorities in this and other countries.¹

The practice was successful in this city in 1797, 1802, and 1803, according to Dr. Rush (iv. 80, 86); as also in 1805, as we learn from the same writer (ii. 96), and Dr. Caldwell (p. 103), and was almost exclusively relied upon by the late Dr. D. Hosack, of New York, whose experience in the yellow fever was ample. In a letter addressed so early as 1800 to Dr. Currie, and published in the latter's account of our epidemic of 1799, that eminent physician carried his preference of the method so far as to affirm that "if there

¹ Makittrick, p. 142; Williams, pp. 31, 47; Moseley, pp. 449, 450, 463; Caldwell, p. 108; Barnwell, pp. 382-384; Thomas, p. 132; R. Jackson, i. 280; Bayley, pp. 103, 106; Valentin, p. 196; Francis, in Townsend, p. 217; Currie, p. 48; Palloni, p. 31; Imray, liii. 87; A. Hosack, p. 31; Monges, pp. 64, 65; Pariset, pp. 574, 582; Bally, p. 504; Copland, iii. 187; Humboldt, p. 781; Finlay, p. 19; Chatard, *Journ. Méd. de Marseilles*, v. 337; Gonzales, in Bally, p. 477; Joseph Brown, in Bally, p. 506.

is a specific in the cure of any disease—if the Peruvian bark is to be relied upon in the cure of intermittent fever—sweating, when induced within the first twelve hours from the commencement of the disease, is a no less certain remedy in the cure of yellow fever” (p. 102). In subsequent letters to Noah Webster, and the Board of Health,¹ and in his lectures, he again dwells on the advantage of this method, which a more enlarged experience had but served to confirm. Acting on the principle that the yellow fever, when “taken out of the hands of nature, is rendered a much more mortal disease than it would prove if treated by less violent remedies,” Dr. Hosack avoided those internal remedies to obtain the object in view, which, as we have seen, are open to serious objections. After emptying the bowels by the use of the mildest remedies—injections, castor oil, rhubarb, and magnesia, or salts in small doses—he resorted to warm drinks, when the salutary discharge took place spontaneously before or immediately after the operation of the purgative, which is sometimes the case, and only requires to be encouraged; or, in other instances, to herb teas, such as catmint, sage, snakeroot, or boneset, aided by the spirits of mindererus, warm covering, warm bath, the artificial vapour, and hot pediluvia—care being taken “to continue the perspiration without the least intermission until the fever is entirely removed.”

Dr. Hosack was fully aware that his method would be objected to as calculated to aggravate instead of diminishing fever; but he was convinced, from what he had seen, that the benefits derived from perspiration in the treatment of yellow fever more than counterbalance all the disadvantages of the heat and temporary stimulus employed to obtain it.²

The practice of commencing the treatment of the disease by hot foot-baths, and other kindred means of exciting perspiration, is now very common in this country and the West Indies.³ In this city, in 1853 and 1854, it was successful in several cases. In some it was preceded by venesection or purgatives; and in several was resorted to at the outset. In all, it was followed by other means presently to be mentioned. An intelligent physician, who saw much of the disease in 1853 in one of our southern cities, states in a private communication, that he resorted to the sweating plan almost exclusively. “Sage,” he remarks, “is generally used for sweating, and I use nothing else. The stomach is very often irritable; warm or hot tisanes, you may suppose, would increase this dangerous and very annoying symptom; but sage tea stops it entirely, and very soon. Six ounces of dry sage leaves, to which add one quart of boiling water, is the way we make it. The feet must be bathed, and the body covered heavily with blankets, and no air admitted to any part of the surface till the fever has left. The tea must be drunk as hot as possible. The patient will urinate freely; but must not get up for that purpose. An action on the bowels is not required till the subsidence of the fever. Should they act, a bed pan must be used, for the patient must not be elevated, and his skin must not be exposed to the air.

¹ Essays, iii. 427, 437.

² Ibid., iv. 432–6.

³ Wedderburne, N. O. J., v. 209; McCormack, do.

The sweating will sometimes continue twenty-four and thirty-six hours. When it stops, the fever has gone, and quinia must be given in \mathfrak{D} j doses every two hours for a term of six hours."

Dr. Finlay's plan was the following: "The patient's feet were placed in hot water—as hot as it could be borne—adding sometimes, according to the violence of the symptoms, a cupful of mustard, and surrounding him with blankets. A general warm bath would have been better, but I was unfortunately unprovided with the means of giving one. I kept him in the bath till a perspiration appeared, gradually adding more hot water as the other became cold (the average time I kept my patient in the bath was about half an hour); then I had him well dried and put to bed, covered him with seven or eight blankets, and gave him calomel and quinine, of each twenty grains (at times, thirty grains), with pulv. Jacobi five grains. I then carefully watched for a continuance of the perspiration. This, however, took a long time to make its appearance for the second time. In three hours I repeated the calomel and quinine, and also the bath (unless perspiration had made its appearance again—if not, it was unnecessary; following up in three hours with ol. ricini \mathfrak{Z} ijj, and sp. æth. nit. \mathfrak{Z} ij."

Even when less attention is paid to the sweating plan, and other means are used, as well as to meet particular indications—relieving pain and local inflammation—pediluvia, warm or tepid;² fomentations and cataplasms are generally used and found useful.³ Warm frictions, either dry or with soap, hot oil, whiskey, or stimulating lotions;⁴ or, again, with lemon juice, also find a useful application. The latter means—frictions with lemon juice—constitute a main instrument of what is called the creole treatment, or that of the coloured women of the West Indies, and is favourably spoken of by medical writers of experience in tropical climates and this country.⁵

The warm bath is particularly appropriate when, in the early stage, the reaction is imperfect, partial, or deficient—when, in a word, the disease assumes one of the grades of the congestive form. In such cases, a general bath of high temperature—not less than 100°—will tend to promote a return of heat to the surface, and diffuse it all over if unequally distributed, and revive the activity of the circulation. In general, the water may be used

¹ Observations on the Remittent (so called) and the Yellow Fever of West Indies, p. 19.

² Barnwell, p. 384; Bally, p. 514; Grant, p. 41; J. Clark, p. 26; S. Brown, p. 66; Catel (1844), p. 232; Ib. (1838), p. 14; Gilbert, p. 82; Furlong, p. 291; Hosack, iii. 433.

³ Gilbert, p. 85; Hosack, Essays, iii. 437; Desportes, i. 213; Wallace, xlv. 279; Catel, p. 14; Ib. (1844), p. 233; Monges, p. 64; Barnwell, p. 384; Moseley, p. 463; Pariset, p. 584; Currie (1793), p. 50; Chatard, Repos., iv. 355; Dickson, p. 381; Burnett, p. 384; R. Jackson, i. 243, 267.

⁴ Caillot, p. 312; Dickson, p. 373; Wood, i. 311; Blair, p. 111; Wilson, p. 14; R. Jackson, i. 243, 261; H. McLean, p. 179; Audouard, p. 316; Imray, liii. 88; Pariset, p. 574; Bally, pp. 479, 541; Dariste, p. 193; Merrill, N. O. J., viii. 4.

⁵ Dariste, p. 193; Bally, p. 541; Gillespie, p. 74; Rochoux, p. 581; Caillot, p. 312, 317; Gilbert, p. 89; Savarésy, p. 517; Kelly, p. 389; Thomas, p. 153; Mabit, p. 18.

alone; but in case of extreme collapse, its beneficial effects will be greatly enhanced by the addition of salt, spirits, or mustard.¹

Sedatives, Anti-Emetics, and Astringents.—To quiet general and local irritation—that of the gastro-enteric organs more particularly—and to restrain hemorrhages, various internal and external means have been resorted to with more or less success. Of the latter, I shall speak in a separate section. Among the remedies used for that purpose, the acetate of lead, owing to the praises bestowed upon it in recent times, deserves our especial notice. The introduction of this agent in the treatment of this fever is far from being of as recent date as some appear to suppose, for Moultrie, as early as the middle of the last century—full a hundred years ago—remarked that he knew a physician who made use of it, with great success, in small doses, to suppress diarrhœa particularly (p. 22). “*Audivi medicum, cujus modo mentionem feci, satis feliciter sacehar. saturn. paucis dosibus adhibuisse, quod verisimile est, quoniam valde astringit, et ad diarrhœam supprimendam magum usum habere credo, ergo bolos, si aliter alvus astringi non posset, adhiberem.*”

But, be the origin of its application what it may, many years after the publication of Dr. Moultrie's essay, and apparently unconscious of the statement it contained respecting the article in question, Dr. Matthew Irvine, of Charleston, resorted to it and warmly recommended it in a pamphlet which appeared in 1820, and attracted some notice. Dr. Irvine thought the remedy applicable to all stages of the fever. In the first as a sedative; in the second as an astringent to restrain hemorrhage, and in the last stage of debility as a subtonic or safe stimulant, suitable for endowing with new energy the exhausted and enfeebled organs. In reference to its sedative and astringent powers, those more particularly under present consideration, Dr. I. regarded this medicine as endowed with singularly beneficial influences in diminishing the inflammatory excitement of the gastric mucous surface; checking its morbid secretions; subduing its irritability, and thereby restraining the frequent retchings and violent vomiting so characteristic of the disease. It is on the decline of the fever that its exhibition is recommended. It may be given in doses of from two and a half grains every two hours, to three grains every four hours, either in pills or solution, but always uncombined; the intervals between the doses to be shortened or extended as the greater or less urgency of the symptoms may require.²

It does not appear, however, that Dr. Irvine made many converts to his mode of treating the disease. In this city, it certainly did not produce the beneficial effects that might well have been anticipated from it. It is true, that Dr. Wood regards it as the most appropriate remedy to counteract the inflammation of the stomach after the subsidence of the febrile stage—affirm-

¹ Wilson, pp. 14, 39; Dickson, op. cit., pp. 282, 283; Turner, Med. and Phil. Reg., iii. 420; Wallace, xlv. 279; Hosack, iii. 435, 437; Ralph, ii. 83; Wood, i. 311; Williams, p. 47; Harrison, ii. 333; H. McLean, pp. 146, 150; Audouard, p. 315; Bryson, p. 60; O'Halloran, pp. 87, 133; Kelly, xiv. 389; Barton, p. 18; Davidge, p. 119; Cartwright, ix. 36; R. Jackson, i. 255; Dickinson, p. 151.

² Essay, p. 41; see also Ecl. Rep., x. 519, 520.

ing that he has employed it in some bad cases with the happiest results, and that in one, who recovered, the matter ejected from the stomach had begun to assume the flaky character of the black vomit (i. 312). But Dr. Wood's trials of this remedy, which were made more than thirty years ago, and never repeated by him, were, and could but be too limited to furnish materials for supporting the testimony of Dr. Irvine, in opposition to the conclusion of other physicians here and elsewhere. Even had those trials been numerous, we have reason to infer that the results obtained, so far from justifying the praise bestowed on the article in question, go far to prove its inefficiency; for we learn from Dr. Jackson, the historian of the epidemic of 1820, that only "one person recovered under its use in the hands of Dr. Wood" (p. 73). If such be the case, it must have failed in all the other instances; and from the effects obtained by other of our physicians from the same remedy, and the frequent recoveries under the most diversified modes of treatment, even in some cases in which the matter ejected from the stomach is of the nature described, we may naturally doubt the propriety of ascribing the recovery in this solitary case to the acetate of lead. That the amount of good derived from the remedy was not great, may be inferred also from the statement of Dr. Jackson, who informs us that "Dr. Worthington gave it to Miss Drinkwater in the last stage of the disease, and *thought* some advantage was gained by it. The vomiting was allayed, and the disease protracted; but it still proved fatal." "Dr. Parrish *thought* it proved serviceable to young Scattergood, who recovered from a severe attack of the disease. Dr. Harvey Klapp, also, exhibited it to Davenport, one of the guards employed in the infected district. This case was the most protracted that occurred, but ended fatally with black vomit on the eighteenth day." (*Ib.*, p. 73.) In none of the instances in which I have seen it used were any beneficial effects produced which could justly be ascribed to it—none that could entitle it to confidence, especially as a quieter of gastric irritability. In a few cases marked by a hemorrhagic disposition—oozing of the gums, or tongue, or sanguineous discoloration of the discharges—it *appeared* to be useful.

By others, elsewhere, who have had more ample opportunities of testing the effects of the remedy, the results obtained have not been favourable. Dr. Proudfoot used it in Spain without success.¹ Evans (p. 301), Kelly (p. 391), say not much in its favour. Dr. Dickson did not find it useful to quiet irritation, or to meet the indications laid down by Dr. Irvine; and advocates it only as an astringent to suppress hemorrhage.²

Dr. Waring employed it both before and after the occurrence of the black vomit; but without the least apparent benefit. He resorted to it also to allay irritability or vomiting; but so far from its producing such effects, it either aggravated those symptoms, or at least did not control them. "I conceived," says that intelligent physician, "it might palsy the powers of the stomach and afford an opportunity, at least, for the administration of cura-

¹ Edinb. Journ., xxviii. 291.

² Essays, &c., p. 370; see also Eclectic Journ., iv. 114.

tive remedies (p. 63), in cases where that organ was too excitable to bear them: but, so far from quieting, it increased its disturbance very often. It irritated the stomach instead of palsyng it, and did as much harm, and in the same way, as it does in extremely acute inflammation of the eyes" (pp. 63, 64).

Oxide of Bismuth.—Mr. Magrath has recommended this article in the latter part of the first, or commencement of the second stage, in which subacute inflammation of the mucous membrane exists—which, he thinks, is always the case previous to the appearance of black vomit. He has used it sometimes after the black vomit set in; but he places little reliance on it under those circumstances. He generally combines carbonate of soda with the bismuth, in doses of three grains each every second hour as long as he sees an indication for their continuance.¹

Rhatany.—Viewing, like some few other pathologists, the yellow fever in the light of an essentially hemorrhagic disease, and aware, from personal observations and the reports of Spanish and other physicians, of the salutary effects of rhatany (*Krameria triandra*) in sanguineous effusions, menorrhagia and hæmatemesis particularly, Dr. Foureau de Beauregard² suggests its use in the former complaint. The plan proposed is to administer, in form of drink, during the first stage of the disease, a lemonade or oxycrate made with the rhatanic vinegar. At a subsequent period, when jaundice makes its appearance, and the symptoms foreshadow the occurrence of black vomit, the patient is to take, two or three times a day, an effervescent mixture composed of thirty-two grains of bicarbonate of potash, four or five ounces of aromatized water, and a tablespoonful of rhatanic vinegar. Finally, in the last stage, Dr. F. proposes the use of drop doses of the krameric acid or ether, as prepared by Mr. Peschier, of Geneva.

Unsupported as it was by the result of clinical experience, and founded on a theory which, though enumerating among its supporters Kéraudren, Bally, and a few other respectable authorities, is, to say the least, of very questionable value, this plan of treatment met with little favour, if indeed any notice from the profession. But, a few years after the appearance of the publications to which attention has just been called, it was taken up by Dr. Senac,³ of New Orleans, who, in a memoir read before the Medical Society of that city, revived the idea, suggested long ago by Dalmas and others, respecting the scorbutic nature of the yellow fever, and warmly recommended the rhatany as a remedy of great efficacy in its treatment. I am not aware, however, that the views entertained by our countryman have made many proselytes in this or any country, or that the rhatany has proved successful in the hands of other practitioners. Discarding, as we must do, the pathological views of Drs. Beauregard and Senac, believing that remedies possessing only astringent qualities can only find their application on the occurrence of passive hemorrhages,

¹ Jamaica Physical Journ., March and April, 1836; Eclectic Journ., i. 237.

² Vues Prophylactiques et Curatives, sur la Fièvre Jaune, &c., Paris, 1826, p. 9; and see also Ann. Mar., 2d pt., 1826, ii. 294; Report on same, to the Acad. of Med., by Dr. Espiand, ib., p. 459.

³ See Thomas, pp. 120, 121.

and aware of the little benefit derived from these, as also of the impropriety of relying on them exclusively in the management of so formidable a disease, I cannot but conclude that the article in question, if at all useful, can only be regarded as an accessory means, appropriate to combat particular symptoms, and not one of general and constant application, and possessing a specific agency in the disease, likely to recommend it to our special notice.

It has been administered for its astringent virtues, in combination with alum, at Cayenne, during the epidemic of 1850–51.¹ The effects obtained in restraining vomiting appear to have been of a satisfactory kind, in some instances, at least.

Tannin.—On the same occasion, and for the same purpose, pure tannin was employed in combination with cinnamon and orange-flower water in doses of fifteen grains of the first, and half a drachm of each of the others (*Ib.*). The same remedy has been used in Mobile² and other places. It acts as tonic, astringent, and anti-emetic.

Creasote.—Dr. Blair states that emesis was frequently moderated or checked by the exhibition of drop doses of creasote made into a mild emulsion with mucilage and sugar. It acted most beneficially when the tongue had lost its fiery edge and tip (p. 111). Creasote has also been used by Dr. Anderson, of Mobile,³ and Dr. Paton, of the West Indies.⁴ By the former, however, this remedy, as well, indeed, as tannin, is considered as of doubtful efficacy when administered internally. He appears to prefer them as washes to restrain hemorrhage.

Adrue.—For the purpose of restraining the sanguine effusion constituting the black vomit and quieting the stomach, recourse has been had, in some of the West India Islands, to a strong decoction or infusion of the nutty root of the adrue. Dr. Cowan, in a MS. quoted by Dr. Arnold, states that the discovery of the surprising properties of this plant in effecting the object in question, was made by a Dr. Howell, of Jamaica. Dr. Cowan must have felt very confident, from personal observation, of the great utility of the discovery; for he remarks, that “a strong infusion of this plant is as much a specific in restraining vomiting in yellow fever, as the Peruvian bark in the cure of intermittent. The first teacupful of the decoction or infusion represses the vomiting; the second or third, cures.”⁵ (*Arnold*, p. 47.)

Nitrate of Silver.—To attain the same or like objects—as a sedative to quiet the stomach and hiccup, and as an astringent to arrest hemorrhages—the nitrate of silver alone, or in combination with opium, has been administered internally, and used externally. It is mentioned by Dickson (p. 281), Kelly (p. 391), O'Halloran (p. 134), Arnold (p. 47), and others. In cases marked

¹ Report, Ann. Marit., March, 1852, p. 181.

² Anderson, Tr. of State Med. Society of Alabama, 1854, p. 46.

³ Op. cit., p. 46.

⁴ Lancet, 1852, ii. 288.

⁵ By experiments made on the use of the different parts of the plant, it is found that the strongest preparation is made by boiling the whole plant, cut or sliced, roots, seeds, leaves, and stem, all together; the quantity, two handfuls in three pints of water, boiled to the evaporation of one-third.

by a hemorrhagic disposition—oozing of the gums or tongue, or sanguineous discoloration of the discharges—the first named of these writers states he is pleased with the effect of the remedy “which not only exerts evident control over the bleeding surface exposed, rendering the red tongue pale, and checking promptly, especially when aided by pressure, the diapedesis from it, but, when given internally in doses of one-eighth to one-third of a grain, improving the condition of all the discharges. Pledgets, dipped in a strong solution of it, being taken into the mouth, stop the hemorrhage there; and enemas of the same solution aid us in arresting the intestinal flux.” Dr. Lyons reports having obtained benefit from it, combined with opium, in two cases of the disease.¹ In reference to the first case, he states that the black vomiting had already occurred; from the first dose the vomiting ceased. They (pills of nitr. of silver and opium) were continued for six or eight hours, and a dose of croton oil was then administered. The patient convalesced the third or fourth day, and had no return of fever; although he continued languid for some weeks. In another case, the remedy was given with the effect of stopping the ejection of black vomit for eight or ten hours before death. Dr. Lyons was induced to use it from its beneficial effect in conjunctivitis; and we know that it has been useful in obstinate diarrhoea, dysentery, and other kindred complaints.² In the second case mentioned by Dr. Lyons, thirty-two ounces of blood were drawn from the arm, and pills of nitrate of silver and opium given. The patient recovered.

Whether or not full confidence can be placed on the remedial virtues of this medicine, as a sedative to the stomach, or as an astringent, to arrest the sanguine exudation from the stomach, we must leave to further experience to decide. Its effects in the latter capacity rest on a better foundation. The results obtained by Dr. Lyons are not such as to place the curative power of the medicine beyond question. The cessation of the vomiting in one case some hours before death, cannot be regarded as a satisfactory evidence of the efficacy of the remedy, inasmuch as instances of a similar cessation not unfrequently occur, in which it cannot be referred to that or any other agent, and there is no proof that it would not have occurred had the case been treated without the nitrate of silver and opium. Nor can we, for kindred reasons, ascribe to those remedies the recovery of the first case; for it is no unusual thing for patients to recover, though subjected to no regular, or even to an improper, treatment. In this city, the remedy has not been used extensively; recommended, however, as it is by Drs. Dickson and Kelly, it is entitled to our regard, and should not be laid aside without further trial. Like Dr. Dickson, Dr. Kelly gave it by the mouth in the dose of one-eighth to one-third of a grain, or in solution as an enema. I have every reason, from my own personal observations, to be pleased with the effects of the remedy under the circumstances mentioned, especially as a topical application to restrain hemorrhage.

It may be proper to remark, that Dr. O'Halloran, while surgeon to a Bri-

¹ Obs. on Black Vomit, Lond. Med. and Phys. Journ., N. S., iv. 101, 102.

² Thomas Aikin, Dublin Medical Press; Boudin, Gaz. Méd., No. 51, 1836.

tish regiment (the 77th), at Jamaica in 1827, tried the nitrate of silver in doses of four to six grains, so as to act as an emetic, and that he at one time conceived he had derived much advantage from it. Nevertheless, in a conversation which he subsequently had with Dr. Gillkrest, from whom we derive the fact, he did not express himself very confidently as to its efficacy.¹ Dr. O'Halloran (p. 134), found it useful in checking severe hiccup, and thinks it a remedy which may be tried without detriment in such cases.

Lime-water.—Among the remedies employed to soothe the irritation of the stomach, allay its irritability, and correct its acidity, lime-water, combined with an equal quantity or two-thirds of new milk, in doses of a wineglassful, or given by itself in proportionate quantity, deserves notice. This medicine was first introduced into the treatment of the disease, for the purposes mentioned, and to prevent or arrest the black vomiting, by the late Dr. Hosack, of New York, before the close of the last century,² and has since proved very useful in the hands of other physicians, in this city and elsewhere. It was well spoken of by Dr. Rush (iv. 48), in 1798, who, while remarking that he does not know that it saved any lives, states that he is “sure it gave ease by removing a painful symptom; and thus, where it did not cure, lessened the sufferings of the sick.” Dr. Hosack thought it more appropriate to afford relief in that species of the black vomit wherein the matter discharged exhibits the appearance of coffee-grounds. In such cases, he affirms that he was more successful with lime-water than with any other remedy, and that he could, if it were necessary, enumerate more than twenty cases wherein this generally fatal symptom had been removed, and the patients had recovered by its use. Drs. Bard and Charlton, two of the most distinguished physicians of New York at that period, and many others, were warm in its praise as a useful remedy to check the black vomit and relieve the heat, burning, pain, and irritability of the stomach. Its inefficacy in the hands of other physicians, in New York and Philadelphia, Dr. Hosack ascribes to the fact of its having been delayed until every other remedy had failed; of its having been exhibited at the same time with other remedies of a counteracting kind; and of its not being sufficiently persevered in.³ Dr. Hosack sometimes gave it, with benefit, in porter, when the mixture of it with milk was rejected.

The lime-water has since been tried and spoken of in terms of commendation by various American physicians in this and other cities, either combined in the way mentioned or alone.⁴ Without enlarging on the subject, it may be mentioned that Dr. Waring remarks that sometimes he “permitted the patient to take twelve ounces of lime-water, with an equal quantity of sweet milk, in the twenty-four hours, prohibiting all other food or drink. In this way the vomiting was often suppressed, even after the secretion of the black matter.”

¹ Gillkrest, *Cycl. of Pract. Med.*, ii. 281.

² Letter of Dr. Hosack, in *Currie on Fever of 1799*, p. 108; Letter of Dr. Bard, in *ib.*, p. 109; *Hosack's Essays, &c.*, iii. 439.

³ *Currie, op. cit.*, p. 111.

⁴ Waring, p. 74; Wood, i. 313; Dickson, p. 281; Francis, in *Townsend*, p. 218; Hall, *Reposit.*, viii. 21; Chisholm, *Charleston Journ.*, x. 453.

Many years after the introduction in this country of the lime-water as a useful remedial agent, it was administered at Guadaloupe by a Dr. Raiffer, and spoken of as a novelty by Rochoux (p. 596), who states that that physician resorted to it, mixed with milk, in five cases of black vomiting. They all recovered. Dr. Rochoux, who on this as on most subjects is ignorant of what has been done in this country, is disposed to attribute the success of the mixture in the hands of Dr. Raiffer to the *milk* used on the occasion, but waits for further experience before forming a definite opinion as to the merits of the medicine, of which he speaks doubtingly, because in the only case in which he tried it the patient died.

The experiments made half a century ago, by the late Dr. Cathrall, of this city, render it very probable, if not certain, that the salutary effects obtained from the lime-water in the yellow fever result from its correcting the corrosive acid which is generally contained in the stomach at the time or before black vomiting sets in; an explanation which receives support from the known agency of the article in neutralizing the gastric acidity noticed in other fevers and various forms of indigestion.

For the same purposes—soothing the irritation, allaying the irritability, or correcting the acidity of the stomach—other means have been used, with more or less success. Among these may be enumerated the carbonates of soda and potash,¹ the saline mixture,² the effervescent draught,³ yeast,⁴ calcined magnesia,⁵ porter,⁶ chalk mixture,⁷ liquor potassæ in barley water,⁸ carbonate of ammonia and hydriodate of potash,⁹ and spruce beer and essences.¹⁰ Of all remedies of the alkaline class, the chlorate of potash is entitled to most notice in this place, from the praise it has recently received from the high authority of Professor Frost, of Charleston.¹¹ The remedy was long ago introduced as a therapeutical agent, for oxygenating purposes, by Dr. Garnett,¹² of Glasgow, and subsequently mentioned by Dr. Chisholm and others.¹³ At a more recent period, it constituted one of the main ingredients in the saline treatment recommended in fevers and cholera by Dr. Stevens, of Santa Cruz.¹⁴ According to Dr. Frost, the chlorate of potash acts beneficially by entering the circulation and thereby preventing those chemical changes from taking place which break down the texture of the blood, and by assisting in eliminating the poison from the system. "It acts upon the skin and kidneys, and, through either organ, may effect much in removing from the system a fatal poison—a

¹ Dickson, p. 370; Kelly, p. 391; Blair, p. 112.

² Hosack, in Currie on Fever of 1799, p. 108; A. Hosack, pp. 31, 36; Wilson, p. 31; Ralph, p. 88.

³ Wood, p. 311; Dickson, p. 370; Gillespie, p. 76; Bally, p. 516; Currie, pp. 45–48; Madrid, iii. 14; Bayley, p. 102; Valentin, p. 209.

⁴ Hosack, in Currie on Fever of 1799, p. 108; A. Hosack, p. 36.

⁵ Currie, p. 45; Monges, p. 67; Caldwell, p. 112; Wilson, p. 38.

⁶ New Orleans in 1839; Thomas, p. 134; Hosack, p. 108; Ralph, p. 89.

⁷ Blair, p. 111.

⁸ Ibid.

⁹ Ibid., p. 112; Amiel, xxxv. 353.

¹⁰ Ralph, p. 89; Chisholm, i. 399; see Capt. Ball's letter, H. McLean, Thomas, &c.

¹¹ On Alkaline Treatment of Fevers, Charleston Journ., viii. 176.

¹² Duncan's Annals of Medicine, ii. 409.

¹³ Chisholm, i. 491; Copland, iii. 187.

¹⁴ Observations on the Blood, pp. 155, 296.

poison which at times strikes with death as hopelessly and irremediably as if the patient had been bitten by a rattlesnake." "Nor is this its only operation. It is known to be the most fruitful source for the production of oxygen gas. Its liberation in the system of the sick, by giving additional properties to the blood, must strongly aid in preserving the vital energies and reinstating a healthy condition. To this cause I would attribute the rapid convalescence which attended the cases where it was employed." "To those who resort to the remedy," Dr. Frost adds, "it is remarkable how much the powers of the constitution are sustained, without the deleterious use of brandy, opium, and other stimulants; but often, with no other nourishment than plain water, the strength is supported until, by renewed secretions and the improved condition brought about by the use of this article, the strength creeps, the functions resume their activity, and the body returns to its healthy condition." Again, we are told by Dr. Frost that, in the opinion of his colleague, Prof. Shepard, the medicine in question must possess an antiseptic effect, by giving rise, in the decomposition it must suffer in the circulation, to hypochloride of potassa, which is a well-known bleaching and disinfecting compound; and, lastly, that its effects must be tonic, from the ultimate production of chloride of potassium, into which state the potassium of the chlorate of potassa must finally pass.

The chlorate of potash was tried in many cases in this city in 1853 and 1854; but the results were not such as might have been expected from the nature of the praises bestowed upon it by Dr. Frost. They certainly did not encourage us to place any reliance upon it as a main therapeutic means in the treatment of the disease. As an adjuvant, to meet certain indications—to soothe gastric irritation by correcting acidity or otherwise, it may be and has been useful. Dr. Finlay, who also used it in the West Indies, came to the same conclusion, for he remarks that, "without being supposed to detract from the remedy, he feels convinced that a great deal of good was done by the non-administration of it" (p. 20). Dr. Stevens, whose object was to restore to the blood its due proportion of saline ingredients, of which, he wrongly supposed, it was always deprived in fevers through the action of the poison, gave the chlorate in combination with other salts—the muriate and bicarbonate of soda.¹

But although it may be true, that certain salts, particularly the muriate of soda, the nitrate of potash, the tartrate of potash, as also the alkaline carbonates, have the effect of rendering venous blood florid; although they may influence its fluidity and coagulability, it is now known that the dark colour of the blood in the yellow fever—as indeed in cholera and other zymotic diseases—is not due to the loss of the saline ingredients of that fluid, and that the too chemical and too exclusive treatment predicated upon that loss, has been found ineffectual, and very generally abandoned. The results obtained by Dr. Imray are not different from those obtained elsewhere. A trial was made by

¹ Verheyen, ii. 29; Schwenke, *Hermatologie*, p. 190; Hales *Hermastat*, p. 154; Eller, *Mem. Acad. Sci. of Berlin*, vii. 13; Boerhaave, *Elem. Chym.*, ii. 378; Petit, *Lettre Seconde*, p. 34; Haller, *Phy.*, ii. 74; Sauvage, *Effets des Médicaments*, p. 37.

him of the saline treatment recommended by Dr. Stevens in the malignant fevers of the West Indies, founded on his hypothesis of the morbid state of the fluids. "The fact of the blood being in a highly diseased condition was sufficiently obvious; but unhappily the salutary change said to be effected by the action of the neutral non-purgative salts, did not take place, as may be inferred from the malignant symptoms continuing unabated. Notwithstanding the exhibition of these salts in large doses, and the administration of the carbonate of soda, muriate of soda, chloride of soda, nitrate of potash, &c., variously combined; yet in no instance in which they were prescribed could it be said that they produced any marked effects, either in preventing malignant symptoms, or in removing them after they had made their appearance."¹

It may not be improper to remark that, as Solomon said there is nothing new under the sun, so the saline treatment was suggested long before the days of Dr. Stevens; for the carbonate, acetate, and sulphate of potash, the tartrate, sulphate, and phosphate of soda formed the principal basis of the treatment proposed rather than pursued by the late Dr. Mitchell, of New York, whose object was to neutralize with them, or destroy the septon or septic acid formed in the stomach, and to impart vitality to the blood.² Indeed, it would not be difficult to show, that long before the time of Dr. Mitchell, the alkalies were regarded not only as useful, but as essential in the management of fevers. Vanhelmont taught that the stomach contained an acid required for the process of fermentation, but which, by its excess, produces disease. Hence, in imitation of his master, Paracelsus, from whom most of his knowledge was derived, he employed extensively alkaline substances. Sylvius, too, viewing the excess of acid as the most common cause of febrile diseases, had recourse to the same remedies. But whatever be the theory we may adopt as regards the cause of the formation and the agency of the acid eliminated and existing in the stomach, even in the early stage of the yellow fever, and continuing there to the close of the attack; and whatever be the views entertained respecting the influence of alkaline salts on the blood, there can be no doubt of the formation of an excess, not of septic acid, or oxide of septon, as maintained by Dr. Mitchell, but of hydrochloric acid, and that the alkalies may be, and have been, advantageously used to neutralize it.

Chloroform.—Dr. Chisolm informs us that, during the prevalence of the fever at Charleston in 1854, the internal administration of chloroform in half drachm doses, every hour, stopped the ejection of black vomit in some desperate cases—without, however, having the slightest effect on, or controlling the march of the disease. (*Op. cit.*, p. 453.)

Opium.—This remedy, in substance, or in its salts, is occasionally found useful, and even necessary, with the view to meet particular indications. 1. It answers well to check diarrhœa, when the latter is carried beyond proper bounds, either from the effect of the disease or of purgatives, or to prevent the evacuating effects of mercurials or bark. For these purposes it has been

¹ Edinburgh Journal, liii. 89.

² Mitchell, Letter to Dr. Percival, i. 265; *Ib.*, Med. Repos., Letter to Dr. Woodhouse, ii. 297.

used advantageously by American and West Indian physicians.¹ 2. The same remedy, either alone or in various combinations, acts favourably in relieving gastric irritability—a symptom of frequent occurrence and distressing nature, and which calls for speedy relief.² 3. Opiates have also occasionally found a useful application in cases attended with some of the many nervous symptoms enumerated in a former chapter—to remove restlessness, hiccup, or promote sleep.³ 4. As a sedative, to reduce vascular and nervous excitement, relax the skin, and induce perspiration.⁴ 5. Lastly, opium has been found useful as a powerful stimulant in the early stage of congestive cases, to induce reaction, or in the latter stages of ordinary cases to support the sinking powers of the system;⁵ and, with physicians of the Brunonian school, has constituted a favourite agent in the general treatment of the disease.

But, although opiates are sometimes found useful for some of the purposes mentioned, the cases in which they succeed are limited in number, and the circumstances which admit of their employment—whatever be the form or character of the disease in which they are employed—are of a peculiar kind. As a general rule, it may be stated that, except in the congestive form, when stimulating means are required to arouse the prostrated powers of life, opium should never be used in the early stage of the disease. It seldom, if ever, is applicable in cases marked by arterial reaction, and in no form or stage when the brain is implicated, and the patient affected with coma or delirium—symptoms which the remedy is sure to aggravate and not unfrequently to bring on. For this reason—from the uncertainty of its beneficial effect, from the probability of its giving rise to unpleasant and dangerous results—and also from its checking the action of the bowels, opium is justly regarded as a remedy of doubtful safety, more frequently hurtful than otherwise, and consequently seldom admissible in the management of the disease under consideration. Hence it is that few remedies have been more generally and decidedly reprobated than the one in question, whatever be the phenomena which would seem to call for its employment. Even by those less unfavourably impressed in regard to its utility, it is generally acknowledged that opium can only be useful under very special circumstances, and that these are but seldom encountered.

More than a century ago, Dr. Warren, though admitting the occasional good effect of anodynes in checking hypercatharsis and diarrhœa, remarked :

¹ Warren, p. 43; S. Brown, p. 68; Warren (of Boston), in Tytler, p. 504; Dickson, p. 281; *Ib.*, Amer. Journ., ii. 76; Wood, i. 311; Chisholm, i. 369; Desportes, i. 212; Moreau de Jonnes, p. 130; Copland, iii. 184.

² Hillary, p. 177; Currie, p. 48; Bally, p. 550; Hunter, pp. 91, 95; Bruce, p. 293; Caldwell (1826), p. 182; Copland, iii. 184; Seaman, pp. 46, 50; Caillot, p. 300; Stone, p. 190; Imray, lxiv. 329; Ralph, p. 87; Bancroft, p. 68; Davy (notes to Blair), p. 106; Blair, p. 106; Dickson, p. 280; Wood, i. 311; J. Clark, p. 29; Gros, p. 21; Valentin, p. 109; Davidge, p. 124; Dickenson, p. 167; Perkins and Harrison, in Townsend, p. 110.

³ Osgood, p. 58; Davy, notes to Blair, p. 106; Blair, p. 106; J. Clark, pp. 30, 37; Chisholm, i. 369; Cathrall, p. 60; Hillary, p. 160.

⁴ Pugnet, p. 367; Chabert, 37; Fenner, p. 138.

⁵ Kelly, p. 390; Dickson, pp. 283–4; Wood, i. 313; Ralph, p. 83; Tully, p. 329; Flores (Sir J. Fellowes), pp. 314, 315.

“What I have principally to offer from my own certain and constant observation is this, that upon taking an anodyne (which the patient himself often sues hard for, to obtain a little rest, or to stop his wearisome retchings), the yellowness shows itself generally the next morning all over the surface of the body and the whites of the eyes; indeed, the fever will thereupon seem somewhat to abate; but I have demonstrated before, how unhappy and almost deplorable his condition then is” (p. 43). Moultrie was not less decided on the subject: “Æger fere semper vigiliis fractus est, non sine multis querelis, attamen anodyna vitanda sunt, hæc enim sanguinem rarefaciunt, stimulo suo febrem irritant, morbum accedunt, et impetum humorum in vasis cerebri augent; præterea Warren, ait, sæpissime anodyna gangrænam inducere, vel effusionem flavam postero die extrudere, et pulsum contrahere, quæ omnia sunt symptomata lethalia.”

Dr. Blair, in one of the most recent works on the same fever, says, that among the medicines which were attended with danger, the salts of opium may be mentioned; and while stating that, in some cases, irritability of stomach, when accompanied by restlessness and uneasiness, was tranquillized by small doses of morphia, he mentions having seen stupor, prostration, and complete narcotism follow the use of one-tenth of a grain of the salt, and concludes with the remark that, “considering the tact and discrimination necessary to obtain beneficial results, it would, perhaps, be more judicious to place it in the *index expurgatorius* of yellow fever materia medica” (p. 106). Between these two periods—1741 and 1850—it is scarcely possible to find a physician of experience in this country or elsewhere, who is disposed to entertain a very different opinion. In this city, opium has very generally been found hurtful.¹ In other cities it has been equally condemned.² In the West Indies, Moseley pronounced it to be a fatal medicine (pp. 474–75), and his opposition to it has been repeated by succeeding physicians, in more or less decided terms.³ In Europe, too, it has been discarded by Palloni (p. 21), Rochoux (p. 638), Vilaseca (quoted by Rochoux) and others.

There is reason to believe that opiates are not equally injurious at all seasons of epidemical manifestation, and that it may consequently be used with better success at some periods or places than at others. Thus, Dr. Rush observed it to be less hurtful in 1794 than it had been in the fever of 1793 (p. 229), and while Dr. Blair found it almost uniformly dangerous at Demerara, in 1837, 1841 (p. 106), Dr. Davy remarks, that opium and morphia were used about the same time, at Barbadoes, with advantage and with comparatively little risk of the bad effects referred to by the former.⁴

¹ Rush, iii. 162; Currie, p. 48; Deveze, p. 270; Barnwell, p. 385; Monges, p. 67;

² Munson, p. 187; Bayley, p. 115; E. H. Smith, p. 147; Thomas, pp. 60, 61, note, 1st edit.; Merrill, ii. 229; Ib., ix. 253; Harrison, p. 34; Pascalis, Letter to Thomas, and quoted by him, p. 61, note.

³ Stevens, pp. 282, 288; Madrid, p. 25; Hume, pp. 231, 232; H. McLean, pp. 125, 126, 163, 164; Lefort (De la Saignée), p. 562; Dariste, p. 205; Caillot, p. 307; Bancroft, p. 71; Poissonnier, p. 57; Chisholm, i. 369; Lefoulon, p. 336; Stevens, pp. 282, 288.

⁴ Davy, notes to Blair, p. 106.

Opium may be administered in substance, or in the form of morphia, either by the stomach or as an endermic application. It is used alone, or combined with calomel to restrain the purgative effects of this medicine, or to meet particular indications; or combined with the ingredient of Dover's powder to obtain the usual effects of that medicine, or with sulphuric ether, &c., for antispasmodic purposes. (*Osgood*, p. 58.)

External Applications.—While the disease is combated by some of the above-mentioned means, the analogy existing in respect to several phenomena between the yellow fever and other febrile affections has suggested, and experience has sanctioned, the use of various others of more or less efficiency in controlling particular symptoms or producing an impression on the system at large. Among these, cold applications claim a large share of our attention.

In the early stage of the disease, when the skin is hot and dry, cold water, applied over a considerable extent of the surface, has been found, at times, to exercise a beneficial influence, and in the hands of many physicians, has constituted an important agent in the treatment of the disease. Such has been the case in tropical climates; and the results there obtained will be found fully corroborated by those recorded in temperate latitudes. I am aware that a writer of some note, and much pretension, Rochoux, while expressing a favourable opinion of the external use of cold water in the fever of hot climates, condemns an extended application of it in the fever of cold latitudes, from a fear of hastening thereby the depression of the vital forces which succeeds so rapidly to the short period of excitement; and, roundly asserting that it has never been thought of, deduces from this supposed neglect of the remedy a proof of a difference between the two fevers. Nor is it less true, that some respectable authorities in this country¹ have, by expressing themselves hostile to, or doubtingly of, this mode of practice, apparently lend support to the opinion of Rochoux; so far, at least, as regards the fever they had in view.

But whatever may have been the case during the epidemic which was noticed by that physician at Barcelona in 1821, and particularly within the sphere of his personal observation, and whatever may be the support he derives from the statements of the writers mentioned, a reference to other and more numerous authorities will easily convince us, that on this, as on many other points, the assertions of Rochoux are at variance with well established facts—that cold has been applied externally with considerable success, not only in hot climates, but in various parts of this country—that if it proves injurious in our yellow fever, it does so only in some particular forms of the disease—that whenever the same forms show themselves in tropical climates, which is not unfrequently the case, the agent in question is equally unsuccessful. We shall find, too, that the opposition made by several writers to the remedy has special reference to some particular mode of application of it, and not to all other modes, some of which are, by not a few, acknowledged to be innocuous, or even, under certain circumstances, advan-

¹ Dalmas, p. 122; Currie (1799), p. 85; Thomas, pp. 119, 138; Gros, p. 21; Davidge, p. 120; Harrison, p. 333; Rush, iii. 162-3.

tageous; and that when this opposition is expressed in more comprehensive terms, it has reference to the application to the disease generally of a rule of practice based on experience obtained in the treatment of only one of its several forms.

In this, as in other places visited by the yellow fever, cold water has been applied externally in various ways. By some,¹ the form of immersion, or bath, is either very greatly preferred or usually selected. Others reject the use of the bath, or resort to it but seldom, and apply cold water in the form of affusion; others, again, very generally—some invariably—have recourse to simple ablution or sponging over the whole or only a portion of the surface; while another set use them indiscriminately, according to the circumstances of the case.² In this country, the cold bath has been long ago used with success. Dr. Bayley, of New York, states that it was employed in the epidemic of 1795 in that city. Dr. Charlton, of the same place—a physician of much experience—who had in many instances witnessed its good effect, favoured Dr. Bayley with the following communication: “I experienced the happiest effects from cold bathing in a variety of cases. I used river water, and had patients placed in a large tub, and a couple of pailfuls thrown over them. It happened in one instance only that the patient did not feel so much refreshment as to be impatient for a repetition of it. In this case, the patient grew chilly after using the bath, which I thought a sufficient reason for discontinuing it. In every other instance, I found no method so effectual for relieving the extreme distress of the head and stomach so generally attendant on that disease.”³

In more recent times, cold similarly applied has been strongly advocated by Dr. Dickson, of Charleston, who thinks it equally effectual in subduing morbid excitement, and controlling irritation, without any positive expenditure or subtraction from the vital forces. Relief from the pungent heat of skin, the tormenting thirst, the distressing headache, pain and irrita-

¹ Dickson, *Elements of Medicine*, p. 275; *Ib.*, *Eclectic Journ.*, iv. 114; *Ib.*, *Am. Journ.*, ii. 76; Pinkard, ii. 378; Dariste, p. 209; Bayley, pp. 109, 110; S. Brown, p. 67; A. Hosack, p. 33; H. McLean, pp. 146, 151–2; Caillot, p. 305; Bally, p. 507; Chisholm, i. 384; Gillespie, p. 73; Wright, vii. 2; *Ib.*, *Lond. Med. Journ.*, vii. 109; Bourgeois, *Voy. Interessants, &c.*, p. 488; Barbe (N. O.), 1843; Nagle, in Currie, *Report on Cold Water*, ii. 134; Wilson, *Ib.*, ii. 16; Gilbert, p. 82; Moreau de Jonnès, p. 130; Grant, p. 56; Imray, lxiv. 329; Hulse, in Joubert, p. 975; Guyon, *Réponse à M. Lefort*, pp. 46–7; Repey, *Diss. sur la Fièvre Jaune*, p. 7; Davidson, in Chisholm, i. 386; Armstrong, *Ib.*, p. 389; Crawford, in Trotter, ii. 93; Fontanges, in Mabit, p. 17; Jolivet, p. 16.

² Powell, p. 11; McArthur, p. 352; Arnold, pp. 50–1; Amiel, p. 352; Perlee, iii. 13; Vatable, p. 349; Caillot, p. 305; Merrill, viii. 3, ix. 254; Dickinson, pp. 151–6; Kelly, xiv. 389; Ferguson, *Med.-Chir. Tr.*, ii. 192; Heustis, p. 118; Valentin, p. 197; Caldwell (1826), p. 159; Cartwright, ix. 36; Osgood, pp. 52–3; Wood, i. 311; R. Jackson, *Sketch*, i. 246; *Ib.*, *Tr. on Fev. of Jamaica*, p. 226; *Ib.*, *Outline*, p. 287; *Ib.*, *Exposition of the Practice of Affusing, &c.*; Boyd, in Johnson, on Hot Climates, p. 304; Waring, p. 76; Davidge, p. 120; Bancroft, pp. 60–1; Dalmas, p. 122; Ralph, ii. 83; Copland, iii. 151; Rush, iii. 155, 224; Shecut, p. 124; A. Hosack, p. 31; Furlong, p. 291; Vincent, p. 31.

³ Bayley, *Fev. of 1795, in New York*, p. 116.

bility of the stomach, we will, he affirms, never fail to procure. "The termination of the chill, if there be one, when the face becomes flushed, and the surface dry and hot—a condition almost characteristic in the degree attending this form of fever—is the moment for affusion. Seat the patient in a convenient vessel, and pour rapidly from some slight elevation upon his head and shoulders and over his naked body a full large stream of cold water, continuing it until his face becomes pale, or his pulse sinks. In general, the sick man himself will exult in the delightful ease which follows it, and will solicit its frequent repetition. I have never seen any unpleasant consequences from it. The surface should be rubbed dry, and the patient, on lying down, covered so as to be comfortably warm. A mild glow precedes a free cutaneous heat, and restless tossing demands the repetition of the bath."¹

Another highly esteemed physician, Dr. Hulse, of the U. S. Navy, uses douches on the head and shoulders, and the salt bath. After this, the patient is well dried, and made to take hot drinks, the effect being a profuse perspiration. (*Joubert*, p. 975.)

Respectable as may be the authority of these physicians, whose experience in the yellow fever has been ample, and backed as they may be by some West India and American writers, they have found but few imitators among us as regards the employment of the cold bath. It was repeatedly tried in the early stage of the disease during the calamitous epidemic of 1793, in this city, but without the beneficial effects that were expected (*Currie*, p. 85). In consequence, it has not been resorted to since to any great extent. Nor will it be found that the cold affusion, though more generally recommended, and so strongly eulogized by Dr. R. Jackson, and other physicians of high standing, enumerates warm supporters among us. More commonly, recourse has been had to simple ablution, or sponging, as being more convenient in its application, safer in its results, and on the whole sufficiently efficacious. Such is the mode in which cold water was applied by Dr. Rush (iii. 155, 224), Dr. Wistar, and others during our former epidemics; such has been the practice at subsequent periods; and such, I am persuaded, will be found the preferable mode whenever circumstances demand or permit a resort to that agent.

While saying this, I may mention that, so far as I am informed, the injurious effects apprehended by Rochoux and others from cold bathing and affusion—their depressing too suddenly the vital forces and the power of reaction, already enfeebled by the action of the specific cause—has not often occurred in judicious hands. Indeed, from the experience and character of those by whom such modes of application have been recommended, and from what I have myself witnessed, I have no hesitation in concluding, that so far from being always detrimental, they may be and are frequently, when resorted to in proper cases, and under suitable conditions of the system, highly advantageous. Nor can it be otherwise; theory leads us to believe, and experience proves, that one of the most effectual ways of preventing the depression in question, is to remove, by appropriate antiphlogistic and sedative means, the

¹ Elements of Medicine, p. 275; see also Med. and Phys. Journ., iii. 263; Ib., Am. Journ., ii. 76.

reaction when it is carried too far. The same results are obtained in other diseases in which prostration succeeds to excitement, and in which, as is well known, the former is generally the greater and the more sudden, in proportion to the violence of the latter; and it would be extraordinary, indeed, were matters to take a different turn in the yellow fever. In this, as in other cases, the prostration is, in some measure, the result of the depressing effects produced on the nervous and vascular systems by the preceding excitement; or, if not so produced, it is at least the more readily noticed when such an excitement has been permitted to run high.

The use of the cold bath, as well as of affusions and ablutions in fevers of various kinds and grades, is not of recent date. They were recommended by Galen, Paulus Ægineta, and other ancient writers. They have long been resorted to in the East and in Europe, as the writings of Chardin, Bruce, Sir John Floyer, Wright, De Haen, Currie, R. Jackson, and Giannini, will satisfactorily show; and the results in these might well, in the absence of positive facts, lead us to infer that the cold bath and cold affusions, which are so efficient in reducing the excitement of the arterial system in those other febrile diseases, will often produce like effects in the one under consideration. Nevertheless, as the inflammatory or vascular excitement of this fever is evidently of a peculiar nature, and governed, in some measure, by laws of its own—and as the system, when placed under the impress of the peculiar poison which gives rise to the disease, does not bear so well a sudden or considerable abstraction of stimulus either by the lancet or otherwise, there can be no doubt of the propriety, as a general rule, of resorting in preference to that mode of applying cold externally which will enable us, while obtaining the desired effect, to avoid the injury that might result from the more violent impression produced by immersion and affusions.

As a substitute for ablution, Bancroft recommends, when the weather is hot, to cover the patient, while in bed, with a sheet wrung out of cold water. This, by evaporating, will gradually reduce the temperature to a proper standard (p. 61). The practice, though claimed by modern empirics, is not new. It was in constant use at Surinam many years ago.¹ Mackittrick, in his Inaugural Dissertation, mentions that Dr. Dalrymple, who was physician to the army sent out on the Carthagera expedition in 1740, cured many whose cases seemed desperate by wrapping them in blankets wetted with water; these softened and relaxed the skin and brought out a profuse sweat, which carried off the fever (p. 59). In latter times, the practice has been particularly recommended by Dr. Blair, who made ample trials of it at Georgetown (Demerara), during the severe epidemic of 1837–1842. “The application of the wet sheet till it dried was generally sufficient to reduce the temperature to a natural standard, and then the sheet was removed; but if the heat persisted, aspersion was required, or the sheet might be redipped and reapplied a second or third time.” Dr. Blair, while on this subject, remarks that before the heat or reduction admitted of being *kept* at a natural standard, the skin generally had

¹ Grimaud, *Traité des Fièvres*, iii. 97.

to be rendered cold, with some appearance of maceration of the fingers on the removal of the sheet. He did not recollect a case which required its continuous application longer than twenty-four hours. Its application is not unpleasant to the feelings of the patient, except sometimes from the first shock (p. 110). At the English hospital of Bellavista, near Callao, in Peru, appointed for the reception of British and American seamen and subjects, strong, able-bodied men, but with constitutions unacclimatized, were daily admitted from the seaport. The practice at first adopted in this establishment consisted of large and repeated doses of calomel, under an impression that the disease arose from a congested state of the liver; but every case treated in this way died. The practice was abandoned, and recourse had to the water system, with better success. Dr. Gallagher assured Dr. A. Smith, from whom I derive the fact, that when the wet sheet was resorted to early, and continued during the hot febrile stage, the fatal tendency of the transition to the second stage was disarmed of danger in a very great proportion of cases.¹ Dr. Wragg, in the late epidemic at Charleston (1854), also tried packing in wet sheets in some of the hot, dry cases; but he is not prepared to say that any very marked advantage was obtained, beyond the decided comfort the patient experienced during their employment (p. 84). In reference to this subject, Dr. Chisolm, in his account of the last mentioned epidemic, states that a physician, who tried the practice in some cases with intensely hot, dry skin, informed him that a decided mitigation of the symptoms was at once obtained, and a greater amount of relief afforded than by any other mode of treatment. "This amelioration was not always permanent. Of seven bad cases, which he treated in this way, five recovered rapidly. The treatment he considers hazardous, and should be used with caution. I saw the remedy tried in two cases—one of these was decidedly improved by the treatment, with a relief of the distressing pains and sensations of fatigue; the other was not so fortunate. The cold cloth was followed by so much prostration as to require stimuli, freely administered, to bring on reaction; and he died very early in the disease, which led us to infer that the remedy had hastened the fatal termination."²

The same principles must govern the practitioner, in the application of this remedial means in the yellow fever, as in other febrile diseases—namely, it should, as a general rule, be resorted to only when the skin is hot and dry, and the reaction well established. Dr. Arnold, of Jamaica, who has, more perhaps than any one else, paid attention to the thermometrical heat of the surface, lays it down as a principle that cold applications to the body are not admissible unless the temperature of the latter is above 102°. In such cases, the application of cold water is usually followed by a reduction of heat and of vascular action, as well as, also, in many cases, by a tendency to perspiration, a sensation of comfort, and an abatement of many unpleasant symptoms. The same effect, but in a less degree, will result from partial

¹ Edinb. Med. and Surg. Journ., lxxxii. 200.

² Charleston Med. Journ., x. 452.

applications of cold water or cold pediluvia. When applied, on the contrary, under other circumstances, very opposite results will very generally follow.

If the skin be cool and the pulse depressed, the sedative effect of cold water will have a tendency to aggravate the symptoms. Reaction, in such cases, seldom occurs; the disease becomes more concentrated within the internal and important organs, and the prostration of the vital powers increases rapidly. When the skin, instead of being dry, is warm, moist, and relaxed—which, as seen, occurs sometimes in all epidemics, and very generally in others—the application of cold to the surface has a tendency to check this salutary condition, and will almost surely aggravate instead of benefiting the disease. Nor are cold ablutions to be less avoided when the patient complains of chilliness or dyspnœa; or when he labours under diarrhœa, or deep congestion, or well-marked inflammation of internal organs; or, again, when he is of weak constitution or of advanced age.

Differing from most writers on the subject, Dr. R. Jackson controverts the propriety of restricting the use of cold water to cases attended with increased temperature of the surface, and in his sketches of febrile diseases cites cases in corroboration of his views. “In the first period of my own experience,” he remarks, “the temperature of the body did not present itself as an essential condition for authorizing the application of the remedy. It was sometimes high, sometimes low, even lower than the standard of health by some degrees; the effect was nevertheless salutary, independent of these varying circumstances” (p. 249). And further on he states that the instances in his own practice, and even in the experience of others, are so numerous and so pointed as to the inconsequence of temperature, that he should not have thought it necessary to dwell upon the subject, had not contrary precepts taken such possession of the public mind as to deter almost every one from employing the remedy where the prescribed condition does not exist in an open and marked degree (p. 250). In cases in which the temperature is low, cold water is salutary—raising it to a just standard, animating the circulation, and giving, in many instances, force and energy to the renewed action of health (p. 251). The authority of Dr. R. Jackson is certainly entitled to high regard, but I do not find that the practice thus recommended has met with the sanction of many other physicians in the West Indies or in this country.

From all that precedes, it will be perceived that cold water, whether used in the form of immersion, affusion, or ablution, when resorted to in the treatment of yellow fever, does not find its application in any other than the first or inflammatory stage, and that it must be viewed as an aid to, or substitute for, depletory means, which are inadmissible in subsequent periods of the disease. In cases attended with strong and vehement action of the heart and bloodvessels, and particularly when there exist local determinations, as well as when vascular action is oppressed, the pulsations hard, contracted, deep, and inexpandible, while, at the same time, the skin is hot and ardent, or thick, compacted, and torpid, the abstraction of blood is a useful, if not indispensable, precursor to cold affusion. When signs of internal congestion—with a cold and torpid skin,

and a deep-seated, sluggish, and oppressed pulse—manifest themselves, cold ablution must be preceded by the use of means calculated to arouse reaction—frictions, the hot bath, perhaps venesection. And again, when the fever is accompanied with symptoms of a nervous character, the application of cold water had better be postponed until after the employment of remedial means useful for their removal.

But even in those cases in which cold ablutions or affusions, and, still more particularly, the cold bath, may be contraindicated, or after the proper period for using them has passed, but in which symptoms of irritation or congestion in some important organ manifest themselves, the local application of cold to the affected part will usually be found useful, and at times highly serviceable. Thus, when the brain and its membranes are implicated, the application of bladders filled with powdered ice or iced water, or of folds of muslin wrung out of water or some cooling lotion, or the exhibition of a douche to the bare scalp, is very justly recommended.¹ Dr. Blair, in such cases, directed the head to be shaved, and “covered with a piece of thin cambric, on which water dripped from a basin suspended over the head, by the capillary action of a bundle of cotton threads, having one end immersed within the water, and the other end hanging over the edge of the basin” (p. 110). When the stomach is the suffering point, similar applications to the epigastric region are resorted to with decided advantage.² Like means are used on the abdomen, when the bowels are affected.³

Warm Bath preceding the Cold Bath, or Affusions.—By not a few practitioners, the warm bath is made to precede the use of cold affusions, and this, too, not only in cases attended at the outset with a cold surface and defective reaction; but also when the disease presents a stage of well-marked arterial and vital excitement. Dr. Robert Jackson, though not the originator of the practice, for it can be traced back to the time of Galen,⁴ has, more than any one else, contributed to give it currency; and enforces the propriety of it in strong terms. “When,” he remarks, “a case presents itself, where the febrile heat is deficient in degree, or where it is unequally distributed on the surface; but where no primary mark of local inflammation or congestion in any one of the internal organs is discernible”—“I conduct the patient into an apartment where the air is of a high temperature; I apply warm fomentations to the extremities; I purify the skin by friction with brushes, soap, and warm water; I affuse warm water on the surface generally, or I immerse the whole body into a warm bath. It is evident that equal distribution of heat and superficial circulation cannot fail to be promoted by the operation of the

¹ Le Riverend, xii. 543; Chisholm, i. 390, 391; Maher, p. 906; Valentin, p. 197; Seaman, p. 45; E. H. Smith, p. 133; Rush, i. 224; Gillespie, p. 73; Imray, lxiv. 329; Kelly, p. 392; Wood, i. 312; O'Halloran, p. 91; Vatable, p. 349; R. Jackson, i. 259; Bancroft, p. 71; Leblond, p. 117; Catel, p. 14; Wragg, p. 84.

² Levacher, p. 98; Vatable, p. 349; Deveze, p. 269; Rush, iii. 155, 224; Chisholm, i. 390.

³ Deveze, p. 269; R. Jackson, p. 260; Maher, p. 907.

⁴ Method. Medend., lib. iv.

means stated. Susceptibility of impression is restored, even artificially increased by friction with soaped brushes and warm water; the condition favourable for the action of remedies is prepared; and the condition being prepared, the affusion of the cold water acts with power; it ordinarily produces great effect, frequently a decisive arrest of the erroneous course, and finally a complete cure of the disease."¹

By Dr. Ralph, the practice was found successful at Barbadoes, in 1816, and strongly recommended (ii. 83).

Tepid Bath.—In cases attended with a moderate degree of temperature of the surface; or when, after depletion, or without, the skin exhibits a tendency to perspiration; when, again, the patient suffers from restlessness or other nervous symptoms, or when the shock from cold water is unpleasant, the tepid bath, or ablution with water of slightly elevated temperature, or a sheet dipped in tepid water, is found highly advantageous. It proved so in ancient times in inflammatory and other fevers, and is highly spoken of by Hippocrates, Galen, Aetius, Prosper Alpinus, and by many modern writers. The bath may be general or partial, in the form of semicupium. By some, the tepid bath is prolonged during several hours—as many as twelve.² But, though when so used, it has occasionally produced the happiest effects, we shall generally find it more advantageous and safer to employ the tepid bath in greater moderation. By its means, the surface, when cool, often acquires a more natural warmth; perspiration is promoted, and the restlessness and other nervous phenomena, as well as symptoms of abdominal irritation, when they exist, are greatly relieved or removed.³

Moultrie speaks highly of the practice of covering the body with folds of linen dipped in tepid water, and making immersions with emollient decoctions. By this means, as he states, perspiration is obtained without irritation, to the great relief of the patient. “*Medicus quidam, qui cum D. Vernon profectus est, ægrorum hoc morbo in primo stadio laborantium corpora pannis lancis in aqua calidam vel decocta emolientia immersis, ut sæpe audiui, obtexit; hoc enim modo, sudore copioso perpetuo inducto, ægri postea sanitatem recuperabant*” (p. 20).

In cases in which it is inconvenient or improper, for particular reasons, to have recourse to the hot or tepid bath—general or partial—the effect will often be obtained by means of the artificial vapour-bath,⁴ obtained by pouring water, either alone or mixed with vinegar, on hot bricks and introduced under the bedclothes.

¹ Jackson, *An Exposition, &c., of the Practice of Affusing Cold Water to the Surface of the Body*, p. 275; Sketch, i. 257.

² Garnier, cited by Bally, p. 504.

³ Valentin, p. 197; Musgrave, p. 138; Blair, p. 109; Bally, p. 504; Gillespie, pp. 73, 77; Moseley, p. 453; Gilbert, pp. 82, 83; Pouppé Desportes, i. 126, 157, 213; Cartwright, p. 36; Kelly, p. 389; Burnett, p. 384; Wright, vii. 1; Shecut, p. 124; Mac-kittrick, p. 142; Thomas Martorelli, in Bally, p. 477; Jackson, i. 255; Deveze, p. 269; Furlong, p. 291; Catel, p. 14; Audouard, p. 315. The tepid bath is in great use among coloured women in the West Indies; Caillot, p. 312.

⁴ S. Brown, p. 66; Monges, ii.; A. Hosack, p. 31.

Aulagnier, in a note appended to his French translation of Moultrie, states, on the authority of Professor Rutherford, of Edinburgh, from whom he obtained the fact, that the physicians of the English Army at Carthage and Jamaica, employed the same means with the greatest success in the treatment of the disease after others had failed (p. 51).

Drinks.—Much attention must be paid to the drinks of the patient—both as regards the substances used, and the temperature at which they are allowed. From an early period in the history of the disease, cold drinks have been found most serviceable in the first or inflammatory stage, when the excitement runs high, when the skin is hot and dry, and the stomach is irritable.

The practice of resorting to cold drinks in fevers of hot and temperate regions is of old standing. Such drinks were used in the days of Hippocrates.¹ Galen,² Celsus (lib. iii. cap. ix.), and Aretæus,³ by all of whom they were strongly recommended. They formed part of the mode of treating fevers among the ancient Egyptians.⁴ Paulus Ægineta,⁵ Oribasius,⁶ Avenhoes,⁷ Avicenna,⁸ and Chardin, inform us they were freely employed in Persia.⁹ Kæmpt says the same of the Japanese;¹⁰ Lomnius, Bontius, Bernier, of the natives of the East Indies.¹¹ The celebrated Cirillo of Naples, in a paper read before the Philosophical Society of London in 1729 (July), and inserted in the *Transactions* of that learned body,¹² recommended their use in fevers in the most eulogistic terms. Since then, the practice has been more or less highly recommended by Heberden, Fordyce, Lanzoni, and others.

So far as regards the yellow fever, Dr. Williams, a century ago, remarked that “large draughts of cold water, or other cool liquors, have occasioned profuse sweats when all the sudorifics in the shops would not have had the same effect” (p. 32), and, in a subsequent page, he records the fact that “those persons who had this fever on board of the vessels in the harbour, who seldom drink anything but cold water, (have) no beds to lie on, or clothes to cover them, with free admission of air, frequently recover” (p. 47). The experience of a more recent period, in the West Indies, Africa, Europe, and this country, has tended to confirm the results recorded by Dr. Williams, so far, at least, as refers to the low temperature of the liquid used. Such was found to be the case during the epidemics of this city, and those that have occurred elsewhere. In lieu of *cold* water, ice-water will be found

¹ Affect. 14, vol. vi. 222; De Morbis, lib. ii. sect. 40; Littré's translation, vii. 57.

² Method. Med., lib. ix. cap. 5.

³ De Morb. Acut., lib. ii. cap. 4.

⁴ Prosp. Alpinus, De Med. Egypt., lib. iv. cap. 15.

⁵ Lib. 2, sect. 18, 27, 300; Adams's Tr., i. 237, 257, 262.

⁶ Bk. 5, Trans. of D'Aremberg, i. 318, 320, &c.

⁷ Adams, in Paulus Ægineta, i. 263.

⁸ Ibid., i. 338.

⁹ Voy. en Perse, ix. 300.

¹⁰ Amœnat. Exot., p. 580.

¹¹ De Curand. Feb. cap. xi. sect. 3.

¹² “On the Use of Cold Water in Fevers,” Philosophical Trans., abridged, vii. pt. 3, 245.

both grateful and advantageous; and when the stomach is irritable and bears with difficulty the ingestion of liquids, small lumps of ice at short intervals will sometimes be retained, and afford decided relief.

Besides water, other substances are often selected as appropriate. Dr. Rush used lemonade, tamarind, currant-jelly, and raw apple-water, toast and water, and weak balm and chamomile tea. Weak lemonade and orangeade have been approved of by other respectable authorities.¹ According to Dr. Rush, "the sub-acid drinks were preferred in most cases, as being not only most agreeable to the taste, but because they tended to compose the stomach (iii. 155)." Others prefer thin flaxseed or gum-water, either alone or slightly sweetened, and avoid acid as likely to irritate the stomach.² The patient, however, is the best judge as to the proper substances, and may safely be allowed to govern the physician in that respect. In many cases iced carbonic acid water will be found agreeable and useful—both by refreshing the patient and allaying the irritability of his stomach. Under the use of cold and iced drinks, the irritation of the gastric mucous membrane often subsides; the sense of heat and ardour at the epigastrium; nausea and vomiting; gastric hemorrhage, as well as the general excitement of the heart and arterial system, diminish; while the temperature of the skin is apt to lessen, and a disposition to perspiration not unfrequently manifests itself.

But, in all cases—even in those characterized by considerable thirst—care must be taken not to overload the stomach, as nausea and vomiting will almost inevitably be the result. The preferable plan is to direct the patient to drink often and to take but a small quantity at a time. Even in cases unattended with much thirst or irritability of the stomach, it is proper to enjoin the same rule of frequent and moderate drinking, for the double purpose of soothing the irritation of that organ, and guarding against awakening a disposition to nausea and vomiting, which, as we have seen, are almost constant attendants on the disease, and require but a trifling cause to bring them on.

In cases characterized by symptoms different from those enumerated, the drinks should be of a higher temperature; and where there exists a tendency to perspiration, they should, the condition of the stomach allowing, be taken warm and slightly aromatized.³ We have already seen that sage tea has been highly recommended. Long before our time, it was used by Moultrie

¹ Riverend, xii. 543; S. Brown, iii. 67; Thomas, p. 133; Catel, p. 14; Copland, iii. 185; Fev. of N. O., 1839, p. 360; Currie, p. 51; Rush, i. 155; Monges, p. 67; Powell, p. 11; E. H. Smith, p. 133; Wood, i. 312; Heustis, pp. 116, 118; Seaman, p. 134; Moultrie, p. 20; Humboldt, p. 781; Bancroft, p. 62; Stone, ii. 189; R. Jackson, Sketch, i. 259; *Ib.*, Exposit., &c., of Cold Affusion, p. 366; Levacher, p. 100; Rouppe, p. 310; Blair, p. 107; Desperrière, p. 50; Deveze, p. 268; Dariste, p. 192; Bruce, p. 282; Gillespie, p. 74; Savarésy, p. 517; Caillot, pp. 312–14–17; Bally, p. 540; Rochoux, p. 581; Bonneau, p. 59; Valentin, p. 117; Piso, Nat. and Med. Hist. of West Indies, pp. 314, 315.

² Moseley, p. 458; Monges, p. 67; Warren, p. 44; Hunter, p. 101; Catel, p. 14.

³ Maher, p. 907; Monges, p. 67.

(p. 19), Makittrick, &c. By some physicians,¹ weak veal, mutton, or chicken-water are used sometimes in a more advanced period of the disease; at others, even in the early stages, either as drinks or vehicles for remedies; and by Dr. Rush they were permitted, towards the crisis. But the propriety of the practice may justly be doubted at an early stage of the attack; not only because the substances in question are of too nourishing a character, but because their presence in the stomach will call into play a greater exercise of the digestive function than is compatible with the irritated condition of the organ.

In an advanced stage of the disease—when the powers of life are fading, the drinks should be of a stimulating and nourishing kind. Porter and water, weak punch, weak wine-whey, claret, champagne, or hock wine, brandy and water, &c. But these will be more particularly and appropriately noticed under another head.

Injections.—Considerable benefit is derived, at various periods of the disease, from the administration of injections, as well for antiphlogistic, sedative, soothing, purgative, as for other purposes. In cases attended with abdominal irritation, the injection of cold water into the cavity of the large intestine rarely fails, as Dr. R. Jackson well remarks, to give relief. “The practice,” says that eminent physician, “is not usual, but it is safe and grateful; it gives solace from pain, and even contributes to effect a decisive and final cure.”² It has not been neglected in this city and other parts of our country,³ and in the West Indies has received the sanction of others besides Dr. Jackson.⁴ In the place of pure water, an infusion of flaxseed or some other mucilaginous and emollient substance will be used with advantage.⁵

In cases attended with obstinate gastric irritability, in which the stomach rejects everything swallowed or the matters formed and accumulated within its cavity, much benefit is often derived from the exhibition of small and strongly purgative injections. They act on the principle of revulsion—excite the peristaltic action of the tube downwards, and tend to quiet the symptoms mentioned. In the advanced stage of the disease, stimulating and tonic agents are often introduced by means of injections, especially when they are rejected by the stomach, and sometimes prove successful. On the subject of their purgative effects, we have already spoken in a preceding chapter.

Cool Air.—There are few physicians, conversant with the yellow fever, who have not had ample opportunities of appreciating the advantages of placing the patient under the influence of *cool* or *fresh* air. The good effects of it,

¹ Dalmas, p. 128; Deveze, p. 268; Valentin, p. 225; Gilbert, p. 82; Valentin, p. 268; Catel, p. 14; Dariste, p. 192.

² Sketch, i. 260.

³ Rush, iii. 155, 224.

⁴ Williams, p. 43; Wood, i. 212; Desportes, i. 207; E. H. Smith, p. 133; Heustis, p. 116; Riverend, xii. 543; Stone, ii. 189; Chevallier, p. 33; Seaman, p. 134; Dariste, p. 193; Bancroft, p. 62; Currie, p. 48; Monges, ii. 67; Caldwell (1826), p. 159; Ib. (1805), p. 112; Arnold, p. 53; Barnwell, p. 379; Osgood, p. 53; Gilbert, p. 83; Moseley, pp. 453–463; Bruce, p. 282; Monson, p. 187; Gonzales, p. 320.

⁵ Rush, p. 159; Monges, p. 66; Thomas, p. 133; Moultrie, p. 20; New Orleans in 1839; Catel, p. 14; Desperrière, p. 50; Chatard, *Reposit.*, iv. 355.

during the epidemic of 1793, were, according to Dr. Rush, obvious in almost every case in which it was applied. "It was equally proper, whether the arterial system was depressed, or whether it discovered in the pulse a high degree of morbid excitement. Dr. Griffiths furnished a remarkable instance of the influence of cool air upon the fever. Upon my visiting him, on the morning of the 8th of October, I found his pulse so full and tense as to indicate bleeding; but, after sitting a few minutes by his bedside, I perceived that the windows of his room had been shut in the night by his nurse, on account of the coldness of the night. I desired that they might be opened. In ten minutes afterwards the doctor's pulse became so much slower and weaker, that I advised the postponement of the bleeding" (iii. 154). "The cool air was improper only in those cases where chilliness attended the disease" (iii. 155, 224, iv. 28). Dr. Cathrall says that the chambers of the sick should be spacious and airy, kept cool, and frequently sprinkled with vinegar. "Great attention should," he adds, "be paid to these circumstances, as they are of infinite importance to those who frequent the apartments of the sick, and are highly conducive to the cure of the disease" (p. 51). Dr. Smith (E. H.) found the same benefit to accrue from this means in New York, during the epidemic of 1795. "The advantages which resulted were," he says, "universal and wonderful. To this end, the sick were ordered to be placed on a hard bed, with very little covering, in the middle of the room; and the doors and windows were, as much as possible, kept open day and night" (p. 133). "In all the stages and circumstances of this malady—of whatever form"—Dr. Dickson says, "we must give the sick man fresh air. If his apartment be close and unventilated, he must needs die." (*Op. cit.*, p. 284.)

Similar observations have been made elsewhere, whether within or beyond the tropics;¹ and, to enhance the effect, it is often found advantageous to sprinkle the room floor with water, alone or mixed with vinegar.² The results are the more marked when the air is not only cool, but also when it is pure, its renewal unimpeded, and its circulation free. For this reason it is that the mortality from the fever is greater where these advantages cannot be obtained. Hence the practice, in the West Indies and elsewhere, of removing the patients from hot and confined localities to spots where the air is cool and pure, and the ventilation as complete as possible; or, where such changes are not attainable, to place them, if possible, in an upper and well-shaded room.³ The necessity of these means has long been recognized. Rochoux dwells on it (p. 654); Lafuente is positive on the subject;⁴ as are also Bally, François and Pariset (p. 608), Pugnet (p. 375), Dariste (p. 187), Lind (p. 139), Valentin (p. 196), Chisholm (i. 406), &c. In these matters, as in most others, there is nothing new under the sun. Galen, some two thousand years ago, in treating of the synochus, directed the apartments of the sick to be kept cool and well ventilated, and to have the floors sprinkled and rendered fragrant by means of suitable flowers.⁵

¹ Munson, p. 187; Currie, p. 46; Valentin, p. 199; Moultrie, p. 18; Rochoux, p. 654.

² Currie, p. 46.

³ Rush, iv. 28.

⁴ Observaciones sobre la Fiebre Amarilla.

⁵ Therap. ad Glauc.

CHAPTER XXXVI.

TREATMENT—CONTINUED.

Counter-Stimulants.—In the treatment of the yellow fever, counter-irritants of various kinds naturally find their application. Of these, external revulsives—blisters and sinapisms, and the like—deserve special attention, not only on account of the extent to which they are used in febrile diseases generally, the potency of their effects, as revulsives or otherwise, in these and other complaints, and the inference naturally drawn as to their probable utility in this particular form of fever, but also for the diversity of opinion to which they have given rise among experienced practitioners in both hemispheres. And first, of blisters. By not a few respectable writers the use of this important mode of counter-irritation has been discountenanced, or even decidedly opposed, in the treatment of the yellow fever. If some of them admit that blisters are sometimes useful for the mitigation of particular symptoms, they are of opinion that such applications produce no permanent benefit, and fail as means of cure; or, at any rate, that the advantages obtained are not sufficiently frequent and great to counterbalance the evil to which the remedy usually gives rise.

Dr. Williams, of old, viewed them as a means which should never be resorted to “without due care and consideration” (p. 46); and such were and are the sentiments of other good judges in matters of the kind, especially as regards the use of blisters in the early stage of the disease.¹ And sure it is that the effects resulting from the application of the remedy in question are often of a nature to justify the opposition it has encountered. It requires, for example, but little experience to perceive that in not a few cases blisters, when used even at the most propitious period of the attack, and when apparently most decidedly called for, produce no effect whatever on the skin; that, when they do, they merely sear or excoriate the surface; or occasion extreme soreness, which affords no relief; that the affected part gives passage to a flow of sanious fluid, or becomes the seat of passive hemorrhages or of black or gangrenous eschars—which are of dangerous tendency, and call for the special care of the physician. Nor is it less true that, even under the same circumstances, blisters are found, though drawing well and suppurating kindly, to produce no material amelioration in the symptoms, or marked effects in averting the fatal proclivity of the disease.

¹ Warren, p. 38; Hillary, p. 170; Palloni, p. 29; Chisholm, i. 362; Bruce, p. 282; Bally, p. 526; E. H. Smith, p. 136; Caillot, p. 275; Gros, p. 21; Cartwright, ix. 34; Rochoux, pp. 604, 657; Moultrie, p. 22; Levacher, p. 101; Monges, ii. 66; Blair, p. 111; Chatard, Med. Reposit., iv. 356; Lefoulon, p. 125.

Nevertheless, although these objections have been, and may be justly urged against the use of blisters in yellow fever; although these sometimes produce no sensible effect on the skin, and at others occasion an injurious one, cases often occur in which they are used not only without detriment, but with advantage. In many instances, indeed, they produce a very material change in the condition of the patient, averting the fatal tendencies, or mitigating the violence of the disease by removing such local irritations of vital organs as may occur in the course of the case, or by occasioning a general stimulating, revulsive, and revolutionizing impression on the system at large.

Without multiplying proofs of their frequent harmlessness and utility, I shall merely revert to the fact that, in 1805, Dr. Caldwell, of this city, prescribed blisters for two hundred patients, and that in only one case did a disposition to gangrene occur in the irritated points, and in this instance the effect was checked by an appropriate treatment (p. 111). And there are but few physicians who have not seen patients whose recoveries appeared to have been promoted by the timely application of blisters. Hence, notwithstanding their inefficacy in some cases, and their unpleasant effects in others, blisters have been extensively used for the purposes mentioned; and whatever be the disappointment or annoyance to which they give rise, we cannot but admit that the objections urged against them have been carried too far, and that in many instances the remedy may be employed with safety and advantage.

But however true this may be, it is a fact placed beyond doubt, and the truth of which is generally conceded, that blisters, though useful in the after stages of the disease, should be carefully abstained from at the outset of the attack. As may readily be presumed, from a knowledge of the condition of the system at that period, and the known effect of blisters during the stage of reaction of kindred fevers, they have a tendency to increase instead of mitigating the irritation. The impropriety of their application, under these circumstances, has been insisted upon by Rush (p. 158), Deveze (p. 287), R. Jackson (i. 315), Stone (p. 186), Williams (p. 46), Moultrie (p. 22), Dariste (p. 158), and other writers already referred to, many of whom are not unfriendly to their use at subsequent periods; while, by many others, that impropriety is tacitly admitted by the omission of any reference to them at that period. When, however, the disease is about passing, or has passed into the second stage—when the febrile period is declining or has subsided, and symptoms of an unpromising character indicate the continuance or aggravation of the danger, blisters to the ankles, or other parts of the body, will often prove serviceable on revulsive or stimulant principles, thereby translating the irritation or congestion from the internal organs, or preventing the depression of the vital powers. They are mentioned in connection with the treatment of the disease at this stage by writers of this city and other parts.¹

¹ Rush, iii. 158, 226; Caldwell (1805), p. 110; Deveze, pp. 271–287; Waring, p. 76; Munson, p. 186; Irvine, p. 40; O'Halloran, p. 91; Gilbert, p. 83; Bally, p. 563; Audouard, p. 315; Towne, p. 24; Moseley, p. 472; Barnwell, p. 384; Catel, p. 24; Imray liii. 88; Gillespie, p. 84; Moreau de Jonnes, p. 130; Joubert, p. 973; Fev. of Cayenne in 1850, p. 181; Dutroulau, in Rufz, p. 61.

At a later period of the attack, when symptoms of prostration present themselves, blisters are often resorted to in conjunction with other stimulating and exciting means; and though often failing, and sometimes occasioning some of the unpleasant effects mentioned, they not unfrequently contribute in arousing the sinking energies of the system.¹

Such being the case in the advanced or collapse stage of the disease, there can be no difficulty in perceiving the applicability of blisters in the early period of the several varieties of the congestive form, when the great object is, while, or before, unloading the overloaded vessels, to restore action to the external surface, and promote excitement in the general circulation and the nervous system. Dr. Currie (p. 50) and others, found them useful in such cases during our epidemics; and though failing often to produce vesication or proper inflammation, from the extreme torpor and insensibility of the skin, and though sometimes giving rise to unpleasant effects, they have met the favour, under like circumstances, of the highest authorities in other parts of our country, in Europe, and in tropical regions—Dickson, R. Jackson, Wilson, Kelly, Harrison, and Ralph.

But perhaps, after all, the main advantage derived from blisters in the treatment of the yellow fever results from their efficacy in moderating or removing the irritation and inflammation of particular organs. Of these no one, as we have seen, suffers more generally and severely than the stomach—often in the early, but more usually at a later period—as marked by pain and heat, and especially by incessant nausea and vomiting. To relieve these symptoms when they occur in the second stage, or in the first, if the febrile excitement does not run high, blisters to the epigastrium or spine are found highly useful. They prove so, even when useless in other respects, and hence have been recommended in such cases by almost every writer on the disease—even by those who object to them in every other respect.²

Similar results are obtained from blisters in the affection of the brain and its membranes, which sometimes attends on the various stages of the fever—headache, delirium, stupor, or coma. Applied to the back of the neck, the occiput or the upper extremities, they often produce a decidedly beneficial effect—removing the irritation of the cerebral organs, or dispersing the con-

¹ Currie, p. 47; Barnwell, p. 384; Caldwell, p. 102; Ffirth, p. 32; Wood, p. 313; Nassy, p. 36; Bayley, p. 98; Moultrie, p. 23; Waring, p. 74; Hosack, iii. 437; Dickson, i. 371; Stone, p. 189; Pariset, p. 586; Audouard, p. 315; Towne, p. 26; Harrison, ii. 334; Osgood, p. 51; Dariste, p. 201; Hume, p. 240; Moseley, p. 472; Imray, liii. 87; Dickenson, p. 159; Pugnet, p. 571; Townsend, p. 209; Rush, iv. 29; Savarésy, pp. 305, 319.

² Bayley, p. 102; Arnold, p. 46; Davidge, p. 124; Ffirth, p. 32; Rush, iii. 159; 1799, p. 86; Wood, i. 313; Currie, p. 47; Seaman, p. 46; Monges, ii. 66; S. Brown, p. 65; Kelly, xiv. 391; Dickson, i. 361; Hosack, iii. 439; Munson, p. 186; Irvine, p. 519; Townsend, p. 209; Perlee, iii. 14; Merrill, ix. 254; Caldwell, p. 110; Williams, p. 46; Moseley, p. 463; H. McLean, p. 166; McArthur, p. 352; Catel, p. 14; Dariste, p. 200; Makittrick, p. 142; Gillespie, p. 79; Pariset, p. 583; O'Halloran, p. 94; Blair, p. 108; Chisholm, *Charleston Journ.*, x. 452.

gestive state of its vessels.¹ Pain in the region of the thorax, whether originating from a morbid condition of the lungs or thoracic muscles, is successfully met by blisters to the affected parts;² while the rachialgia, the source of so much suffering to the patient, is sometimes relieved by the same means.³

It is not always necessary to produce decided vesication in order to obtain from blisters the effect desired. Much benefit will often ensue from their action as rubefacients. In all cases, indeed, in which such minor and temporary effects will suffice, as well as in those in which a more extensive and permanent inflammation of the skin would be likely to occasion some of the unpleasant results alluded to above, it will be safer to abstain from doing more than reddening and irritating the surface. By this means, a revulsive action is obtained without risk of gangrene, soreness, and hemorrhage, while room is left for renewing the effect when it subsides too soon (*Caillot*, p. 295). There are not wanting those who, as a general rule, prefer this to any mode of using blisters, and seldom, if ever, aim at producing complete vesication.

Sinapisms.—For similar reasons, as also on account of the rapidity of their effects, sinapisms may be usefully substituted to blisters, and should always be preferred when the urgency of the symptoms calls for a prompt and energetic treatment. They are particularly useful to tranquillize the stomach, and remove local pain; and, in congestive, as well as in the advanced stages of ordinary cases, to invite excitement to the surface, and arouse the failing power of the nervous and arterial systems. By some they are used early to counteract congestion.⁴ “Sinapisms,” says Dr. Daniel, of Savannah, “have heretofore been used for their rubefacient virtue. I have resorted to them to inflame the skin. Used as rubefacients, they are certainly in some measure revulsive; here their influence is usually as evanescent as the discoloration which they produce. Where the skin is inflamed, and that inflammation continued, the revulsive or derivative power of the sinapism is permanent, and consequently perpetuates the good which it at first achieved. Heretofore, the sinapism has been chiefly resorted to in moments of emergency for temporary benefit. Hereafter, it will be considered as one of the most efficient means of arresting and controlling autumnal fevers; at least, such it has been in my hands.” “Whether the pulse be full, rapid, irregular, and frequent; or thin, feeble, and quick, the inflammation of the skin by sinapisms improves its character, and approximates it most nearly to a healthy condition. Whether the heat of the system, generally, be above or below the grade of health, the successful application of sinapisms restores it much more

¹ Ffirth, p. 32; Currie, pp. 56–7; Townsend, p. 209; Rush, p. 227; S. Brown, p. 65; Caldwell, p. 110; Hosack, iii. 439; Moultrie, p. 22; Ralph, ii. 87; Blair, p. 108; Gilbert, p. 85; Dariste, p. 200; Barnwell, p. 384; Pariset, p. 583; Gillespie, p. 73; Poissonnier, p. 82; Pignet, p. 371.

² Dariste, p. 200; Gillespie, p. 84.

³ Pariset, p. 583.

⁴ Dickson, i. 361–9; Ralph, ii. 87–9; A. Hosack, p. 37; Blair, p. 108; Rochoux, 604, 656; Deveze, p. 272; Currie, p. 50; Monges, p. 66; Pariset, p. 584; Cartwright, ix. 34; Ffirth, p. 30; Chisolm, *Charleston Journ.*, x. 450; Fellowes, p. 407; Finlay, p. 21; *Lancet*, 1853, i. 470, Am. ed.; Imray, liii. 88; Hunter, p. 184; Copland, iii. 187.

nearly to its ordinary standard. Whether the skin be hot, harsh, and dry ; or cold, shrivelled, and bedewed with clammy perspirations, the inflammations produced by sinapisms very much improve its condition.”¹ I doubt whether the experience of others, in and out of Savannah, will justify the praises thus bestowed on the remedy in question ; but of its usefulness for the purposes mentioned above, there can be no difference of opinion. In the congestive form it is of very useful application. Dr. Dickson, in such cases, has the patient placed in a hot bath, rendered, if needs be, more stimulant by the addition of common salt, spirits, or mustard. “On taking him out,” he adds, “sinapisms, or rather mustard poultices, should be applied extensively to the surface of the patient, the epigastrium especially and along the spine, the ankles, legs, thighs, and arms. It is difficult to speak too strongly of the beneficial effects of this mode of irritation in rousing the energies of the vascular and sensorial systems, now enfeebled and paralyzed to the greatest degree consistent with life.”²

Moxa.—By some writers, in this country and Europe, moxas along the course of the spine have been proposed and used, under the impression that the spinal marrow was the seat of the disease. Of this practice little need be said. It has never been tried here, and probably never will be. Dr. Thomas recommends it, and directs the application to be made not only to the spine, but to the epigastric region also ; only, however, in cases in which there is reason to hope that the irritation thereby produced will be superior to that existing in the spinal marrow and the stomach. He cites cases, one of which got well, whether in consequence or in spite of the remedy does not appear.³ Dr. Thomas, it should be stated, is of opinion that the spinal marrow and brain are always the seat of a strong inflammation or irritation in the yellow fever, but that this inflammation or irritation, like every other that may occur, is secondary, and does not constitute an essential part of the disease.⁴ Others have suggested the use of the same means.⁵ Moreau de Jonnés informs us that it was tried at Martinique in 1802, but that the only result obtained from it was to increase the sufferings of the patient (pp. 130, 131).

Actual Caution.—In cases attended with great nervous irritation, delirium, and convulsions, Valentin, other means failing, had recourse to the actual cautery applied to the summit of the head or occiput.⁶ In a work specially devoted to the subject of the remedial effects of that powerful agent, that writer relates several of the cases of fever in which he used it advantageously. He admits, however—and every one who examines the details into which he enters will coincide with him—that the disease in those cases was not *exactly* yellow fever ; and that while the cautery applied to the occiput appeared to act beneficially in them, it failed to produce the effect in other

¹ Observations upon Autumnal Fevers, pp. 103, 112.

² Elements of Med., p. 283.

³ Tr. de la Fièvre Jaune, p. 97, 1st edit.

⁴ Ib., p. 137.

⁵ Bally, in Rep. on Fever of Barcelona, 1821, p. 586 ; Audouard, p. 317.

⁶ Tr. de la Fièvre Jaune d'Amérique, p. 221.

instances of a less problematical character.¹ The same remedy was proposed, but not used, by Dr. François, whose early experience in the yellow fever was derived in St. Domingo, and who, after a profound examination of the subject, came to the wise conclusion that the disease is located in the cerebellum (!), and may be controlled by the action of fire applied in the immediate vicinity of that organ.²

Stimuli and Tonics.—At the period of the ascendancy of the Brunonian doctrine—when the yellow, like every other fever, was regarded as invariably of a typhoid character, the result of direct prostration of the vital powers—stimuli constituted the favourite or main instruments of treatment, and were resorted to in all stages of the disease—even from the outset of the attack. In this city, during the epidemic of 1793, Dr. Rush at first had recourse to wine, brandy, aromatics, and other articles of kindred nature (p. 125). Stimulants have been freely used by other American physicians, at different seasons and in different places.³ They have also been resorted to in Spain and the West Indies,⁵ after slight or no preparation. “During a certain period of the war of 1793,” says Dr. Jackson, “stimulation by wine and brandy was often carried to excess in British military hospitals, even from the beginning of the disease.” But the experience of the profession generally—not in one country only, but in all—has taught, what indeed our knowledge of the nature of the disease, in its early stage, ought to have indicated, that the stimulating treatment is not calculated to meet the indications. In this city it failed completely; and the results elsewhere have not proved of a different kind, whenever the case was marked by symptoms of febrile reaction. In this variety of the disease, the stimulating plan cannot, and does not, fail generally to increase the inflammatory condition of organs implicated, and of the system at large; and if not *always* of fatal tendency, it is almost invariably injurious, and never necessary. Hence, though still upheld by a few physicians as safe and proper, stimulants, in the early period of the variety in question, are condemned by most writers, and silently ignored by a still larger number.⁶ In cases, however, in which, at the outset, the reaction is feeble or deficient—in some of the modifications of the congestive variety—stimulants are sometimes required.⁷ They tend to arouse the energies of the vascular and nervous systems, and induce a healthful reaction; and are admissible even when bleeding and other modes of depletion are employed to empty the congested organs, and thereby enable them to perform their natural functions. I am fully

¹ Mémoire et Observ. concernant les bons effets du Cautère actuel appliqué sur la Tête, pp. 17, 56, 57.

² Obs. et Réfl. sur la Fièvre Jaune, Journal de Méd. of Corvisart, &c., vol. vii.

³ Daniel, p. 116, &c.; Tully, p. 328.

⁴ Rochoux, p. 648; Arejula, p. 191, &c.

⁵ Powell, p. 13; Savarésy, p. 301; Moreau de Jonnés, p. 131; Davidson, i. 166; Guyon, Arch. Gén., xii. 634, 635; Ib., Réponse à M. Lefort, pp. 39, 40.

⁶ Jackson, Sketch, i. 299; Rush, iii. 125, &c.; Williams, p. 31; E. H. Smith, p. 141; Bayley, p. 111; Desperrière, p. 86; Waring, p. 63.

⁷ Jackson, i. 301; Currie, pp. 52, 54; Wood, i. 313; Imray, lxiv. 329; Caldwell, Mem., 1826, p. 166; Wilson, pp. 11–14; Merrill, iii. 226; Chisholm, Charleston Journ., x. 452.

aware that, in not a few cases of even the congestive form, the early use of stimuli are not only unnecessary, but improper; and willingly unite in the censure they have encountered from physicians of our country and elsewhere.¹

Dr. Rush, in exemplification of the impropriety of the practice, remarks: "The system, in certain states of malignant fever, resembles a man struggling beneath a load of two hundred weight, who is able to lift but one hundred and seventy-five. In order to assist him, it will be to no purpose to attempt to infuse additional vigour into his muscles by the use of the whip and of strong drink. Every exertion will serve only to waste his strength. In this situation, let the pockets of the man be emptied of their contents, and let him be stripped of so much of his clothing as to reduce his weight five-and-twenty or thirty pounds," and "he will rise from the ground" (p. 153). Nevertheless, though stimulants are not generally adequate to the removal of the congestive state of the organs, and other means are required for the purpose, they may be and are sometimes necessary for the purpose of reanimating the failing powers of the system.

With these exceptions, stimulants are only admissible in the advanced or sinking stage of the disease, when, although not generally successful, they deserve to be tried, and are often required in very large doses. In few cases do they prove beneficial, or even safe, in the second stage, or that of remission or metaptosis, tending, as they then do, to irritate the stomach and excite unduly and unnecessarily the system at large. Nothing but a marked state of general debility can justify their use, or, at least, a resort to any but such as are of a mild character. But when this stage has passed off, and symptoms of threatening dissolution present themselves, stimulants are called for, and are sometimes instrumental in sustaining the powers of life or mitigating particular symptoms, and thereby enabling the patient to outlive the period of duration of the disease.²

For this purpose various means have been used. By many, capsicum has been used and much approved of.³ Dr. Wright, whose communication, under date of December 10, 1794, is to be found in the seventh volume of *Medical Facts and Observations*, says: "Hitherto, the black vomiting has usually been considered as a fatal symptom, and a remedy to obviate it has long been a desideratum amongst physicians. To whom the happy idea of such a remedy in the capsicum is owing, I have not yet learned; but he merits the thanks of his country and of mankind" (p. 12). Capsicum, according to Dr. Dickson, is one of the safest and best stimulants. He objects to its

¹ Rush, p. 153; Merrill, ix. 251.

² Harrison, ii. 335; Currie, *Fever of 1793*, p. 52, &c.; Copland, iii. 185; Paton, *Lancet*, 1853, ii. 288; Imray, liii. 88; *Ib.*, lxiv. 529; Ralph, ii. 86; Anderson, p. 46; Deveze, pp. 273-275; Arnold, p. 54; Joubert, p. 973; Hillary, p. 156; Moseley, p. 460; Bruce, p. 282; Desportes, i. 201; Lefoulon, p. 90; Bally, p. 561.

³ Daniel, p. 116; Wright, see Chisholm, i. 462; *Ib.*, *Facts and Observ.*, vii. 11; *Ib.*, *Ann. of Med.*, ii. 244; McLean, pp. 144, 169; Wood, i. 313; Dickson, *Elem. of Med.*, p. 281; Tully, p. 330; Dickinson, p. 166; Drummond, in Valentin, p. 24; Bayley, p. 112; Copland, iii. 183; J. H. Dickson, in Doughty, p. 43; Arch. Smith, lxxxii. 201.

exhibition while the febrile excitement continues; but as soon as that has subsided, it may be administered. The pill, he says, is its best form, but even that may occasion an unpleasant burning, and require to be desisted from.

Few have bestowed higher praise on this article than Dr. Daniell, of Savannah. After informing us that he is much at a loss for a phrase by which to mark its character, as he is unable to find a proper class for it—that it is so essentially different from every other article of the *materia medica* as to be entitled to a place by itself, and that it certainly partakes of some of the qualities of both a stimulant and a tonic, and yet its prominent virtue differs from both, Dr. Daniell remarks, that to a system suffering under the debilitating influence of fever, the Cayenne pepper imparts an energy, a warmth and a tone entirely peculiar to itself, without ever producing an undue or injurious excitement, if that may be so called, which approximates the debilitated and overpowered energies of the system nearer to a state of health, though still below it. “Its pungency on the palate has certainly given to Cayenne pepper a character for harshness wholly unmerited, and daily disproved in its extensive application to culinary purposes, and table use. In no part of my practice have I found so much difficulty in overcoming the prejudices of patients.” He uses this article in the form of infusion (say a tablespoonful of the powdered Cayenne to a pint of boiling water; of which, from a tablespoonful to a wineglassful may be given every hour, or, *in extremis*, oftener, according to circumstances); and states that patients frequently express the comfortable feelings which this article produces upon the stomach; and especially where great gastric distress had existed. Dr. Daniell further says: “Cayenne pepper comes admirably in aid of sinapisms in equalizing the circulation and heat, and preparing the system for the administration of the serpentaria or bark, or both. It is equally important in removing the cold stage of adynamic fevers, and in sustaining the system against the recurrence of the paroxysm. It gives elasticity and moisture to the hot, harsh, dry skin; and has a powerful influence in checking profuse cold, and clammy perspirations. It comes admirably in aid of sinapisms, in relieving determinations to the brain, as indicated by headache and dilated pupils. It has a decided tendency to remove the dry, rough condition of the tongue.”

Dr. Arch. Smith in like manner speaks highly of this article. He combined it with camphor and opium, as prescribed by Dr. Copland. In a vast number of cases, according to him, this practice was attended with the best effects; for it kept up the vigour of the circulation, at least to the degree of not allowing the surface or extremities to become cold, and of preventing the realization of the often threatened exudation of black vomit.¹

Some have thought well of serpentaria.² Brandy, Madeira, and other wines,

¹ Edinb. Med. and Surg. Journ., lxxxii. 201.

² Powell, p. 14; Devèze, p. 271; Bruce, p. 281; Tully, p. 329; Monges, ii. 67; Daniell, p. 119; Hillary, p. 161; Barnwell, p. 382; A. Hosack, p. 39; Bally, p. 562; Flores, in Fellowes, p. 314; Valentin, p. 210; Dariste, p. 197; Chalmers, i. 165.

are often necessary, and required in notable quantity. Of the latter, champagne has been highly recommended by Dr. R. Jackson, as having advantages over all others. Ralph (p. 86) and Paton (p. 288) also speak well of it; and, in regard to its effects, Dr. Imray remarks that few articles were found to answer so well—its exciting action being more diffusive and rapid than that of other wines, while it was more likely to sit on the stomach (liii. 88). It has been used with advantage in this country.¹ The experience in this city, during the later epidemics, is rather favourable to its employment, and in confirmation of the views of Dr. Imray. Dr. Blair gives the preference to hock. “Of all cordials,” he says, “the best is Rhenish wine.” “When of good quality, it is retained when everything else is rejected, and it is universally liked by the patients. I have seen the most excellent effects from its use, and have often given it to the extent of two bottles in twenty-four hours. I believe it has saved many lives, and I know of no adequate substitute for it” (p. 112). Dr. B. adds that the flavour of the wine should be marked, yet delicate. “If it sensibly effervesces with the carbonates, it is useless, for it will not only be rejected by the stomach, but disliked by the patient” (p. 112).

Impressed with the idea of the supposed analogy existing between the poison of yellow fever and that of the viper, Dr. Bally, many years ago, suggested the use, internal and external, of ammonia (p. 552). In 1822, he and his colleagues, Drs. Pariset and François, in their accounts of the epidemic of Barcelona, reiterated the suggestion on similar grounds; but it does not appear that they derived much benefit from the remedy. Dr. R. Jackson enumerates it as “among the most powerful of efficient stimulants,” available in this disease—adding, however, that as its effects are of short duration, it can only be beneficial in the hands of those who narrowly watch, or in a manner nurse the sick (i. 302). In this country, it has been used by Tully (p. 332), and others; Wood (i. 313) speaks of it; Arnold (p. 54), Valentin (p. 213), Savarésy (p. 321), Copland (iii. 185), and Wilson (p. 29), do so likewise, and I have seen it answer a good purpose, both as a stimulant and a tranquillizer of gastric irritability.

Dr. Blair found that when the stomach would bear carbonate of ammonia and musk in combination, the effect was highly beneficial. Ten grains of carbonate of ammonia, and thirty grains of musk were an ordinary dose, which was frequently repeated. In some rare cases, wherein the heat of the extremities persisted during the third stage, the aq. ammon. acetat., with an excess of ammonia and hydriodate of potash in solution was often used and retained with salutary effect (p. 112). Camphor² alone, or combined with musk or other substances of kindred nature, has been used and recommended.

¹ Anderson, Trans. of Alabama State Med. Society, 1854, p. 46.

² Moreau de Jonnes, p. 130; Rochoux, p. 648; Powell, p. 14; Devèze, p. 271; Dariste, p. 206; Gilbert, 80; Bruce, p. 281; Barnwell, p. 385; Pariset, p. 584; Valentin, p. 210; Copland, iii. 183; Arnold, p. 54; Leblond, p. 119; Lefoulon, pp. 92, 379; Desperriere, p. 81; Savarésy, p. 321; H. McLean, p. 169; Arch. Smith, lxxxii. 201; Pignet, p. 367.

More lately the chloride of sodium has been found useful, as a stimulant and antispasmodic when nervous symptoms set in. It is mentioned by Dr. Levacher (pp. 99, 100). Dr. Imray remarks that it was used with the happiest effect by Mr. Millengen in the Military Hospital of Dominica, during the epidemic of that island, when bleeding from the gums had taken place.

It was administered by injection as well as by the mouth.¹ Dr. Thomas, of New Orleans, speaks in the highest terms of this remedy in the last stage of the disease. The salutary effect was particularly marked during the epidemics of 1837 and 1839, and such as to induce Dr. T. to regard the chloride as one of the most efficient means in the management of that period of the disease. It acts both by its stimulant and antiseptic qualities (pp. 106, 127); and several of the cases reported by Thomas in the last edition of his work, appear to bear him out fully in the praise bestowed upon the remedy.

As a stimulant and nervine, *sulphuric ether* has long held its place among the remedies appropriate in the third or last stage of the yellow fever. It is found beneficial, in some cases, for the mitigation or removal of hiccough or symptoms of kindred nature.²

The atonic condition of the nervous system in the latter stage of the yellow fever, as indicated by prostration of strength, difficulty of breathing, passive hemorrhage, insensibility of the skin, slowness and weakness of the pulse, and the like, has suggested the use of the sulphate of strychnia. This remedy appears to have been first employed by Dr. Rouanet, of New Orleans, during the epidemic which visited that city in 1847. He prescribed it in doses of a sixteenth of a grain every three hours, gradually increasing the amount to a half grain, or even a grain, in the twenty-four hours. In a letter to Dr. Thomas, he speaks highly of its success.³

Having obtained a decided benefit from this remedy, during the period of prostration, Dr. Rouanet ventured to prescribe it in other stages, and in the several forms of the disease. But, although some patients recovered who had taken it from the outset of the attack, he is of opinion that the strychnia will be found serviceable only when administered during the stage of prostration. To this remedy he attributes the recovery of several of his patients. Dr. Thomas, already cited, from whom we derive these facts, has likewise used the strychnia in several unpromising cases of the disease with results such as to impress him with an exalted idea of its efficacy (p. 237, &c.).

Of opium, as a sedative agent, I have already spoken. Although highly extolled by a few physicians, as very useful in the treatment of the yellow fever, it is now so generally discarded, except occasionally to relieve restless-

¹ Edinb. Med. and Surg. Journ., lxiv. 329.

² Poissonnier, *Mal. des Gens de Mer.*, p. 351; Bally, p. 548; Chisholm, i. 369; Flores, in *Fellowes*, p. 314; McArthur, p. 353; Devèze, p. 274; Dazille, p. 49; Tully, p. 332; Osgood, p. 59; Arejula, p. 191; Powell, p. 14; Pariset, p. 584; Ralph, ii. 89; Valentin, p. 210; R. Jackson, p. 302; Gillespie, p. 76; Arnold, p. 54; Townsend, p. 212; Wilson, p. 29; Pagnet, p. 367; Finlay, p. 21; Lallemand, p. 148.

³ See Thomas, pp. 237, 240.

ness and mental dejection, that nothing more need be said on the subject. By some, not a little reliance is placed upon it in reference to its stimulating effects in the sinking stage, though more particularly in the congestive form of the disease. Among these we may cite Dr. Dickson, of Charleston. On this subject that distinguished physician remarks: "I have found opium one of my most available excitants; but it must be administered boldly, and its effects kept up by repetition. I was led to its use by the obvious analogy which in one case I found to exist between the actual condition of the patient and that with which I was so familiar in delirium tremens. This patient, a gentleman of the most correct habits—temperate and studious—lay feeble and almost unconscious, trembling, and muttering, and moaning, while the blood oozed constantly from his mouth and nostrils. He was restless, sleepless, and highly incoherent; he took opium, in three-grain doses, at an interval of two hours, for some time, while his strength was supported by the infus. cinchonæ with mucilages and brandy. He recovered."¹

As a stimulant in the latter stages of ordinary cases, and also in virtue of its diuretic effects, the spirits of turpentine has sometimes been used with advantage. In congestive cases, it is also viewed as highly serviceable.² Dr. Dickson states, that in such cases it is an invaluable remedy, and that when used in full doses it is doubly serviceable, as inducing a safe cathartic operation on the bowels, while it excites the system most agreeably and quickly.

The turpentine was used in this city in 1805. During the epidemic of that year, Dr. Physick gave it in doses of ten drops every two hours, combined with a little molasses or syrup, or sweet oil, for the purpose of checking vomiting. Its success, in several cases, was encouraging. Dr. Rush, from whom we learn this fact, administered it with success in the last stage of the disease, and by Drs. Church and Parrish it was freely employed among the patients of the City Hospital, in considerable doses, both by the mouth and in injections.³ The effects obtained with it in that institution, do not appear to have been of such marked benefit as to have acquired for it a high degree of confidence. In the year 1820, the remedy was again introduced into the treatment of the disease, and for a time, under the high patronage of Prof. Chapman, acquired considerable reputation. On this occasion, it was not prescribed simply to quiet the stomach, or as a common stimulant to invigorate the failing powers of life. It was raised to the rank of a specific. Dr. Jackson, the historian of the epidemic of that season, states the grounds upon which the practice was founded. The stomach, he remarks, being considered the part of the system in which the morbid action first commences, and from which it is diffused by sympathetic or associated action to all the other portions, it was believed possible to strike at the disease in its strong hold. The excitation of counter action in affections from

¹ Elements of Med., p. 283.

² Wilson, pp. 29, 35-6; Dickson, op. cit., p. 283; Ralph, ii. 90; Wood, i. 313.

³ Rush, iv. 96, 97.

local irritations, is founded on the best established and most approved principles in medicine. Blisters are the best remedies for erysipelatous, and some species of phlegmonous inflammation. Turpentine for the healing of burns, and Cayenne pepper in some cases of angina. It was conceived, therefore, that an irritation excited in the stomach, might establish a new action, different from that produced by the poison that occasions malignant fever. On this principle it was, that the free and liberal exhibition of the oil of turpentine was commenced at the City or Fever Hospital. It was there combined with some other essential oil—the usual prescription being

Ol. terebinth.	.	.	.	℥viii.
Ol. menth.	.	.	.	℥i.

Of this mixture, thirty drops were administered every hour (pp. 69, 70).

It was used also in private practice by other physicians; but neither in the city nor at the hospital was the success such as to entitle the remedy to more confidence than it had acquired at former periods. Of twenty-one cases in which it was administered, according to the statement of Dr. Jackson, twelve recovered and nine died. On this subject, Dr. J. remarks that the practice at the hospital, where turpentine was then the favourite remedy, was more frequently successful than in the city. But, he properly adds, that the circumstance is not attributable to any superiority in that or any other remedy employed; for it has uniformly occurred in every preceding epidemic. More natural is it to attribute the result to the influence of a pure and salubrious atmosphere, constant medical attention, and good and faithful nursing, as also to the circumstance that in several of the hospital cases the disease was of the milder grades, and would have recovered under any treatment (p. 74).

That the success attributed to the turpentine, even at the hospital referred to, may fairly be called in question, would appear from the testimony of the physician charged with its administration. The late Professor B. R. Rheese, then house-surgeon of the establishment, in a letter now before me, dated March, 1828, says: "With two exceptions, I have never seen the ol. terebinth. employed in yellow fever to advantage, where stimulants were not clearly indicated, and, in a majority of these, the vol. alkali was found to be retained where the tereb. was rejected, and to answer the same purpose. I infer, therefore, as I did in my inaugural dissertation (not published), that, in these cases, its utility depended entirely on its stimulant properties." After having mentioned one case in which it seemed to be useful, Dr. R. continues: "In this case I have never been able to see any illustration of the utility of ol. tereb. in yellow fever as a distinct disease. It will, and did here answer particular indications, such as stimulation of the circulatory, urinary, and gastric systems; but it is a moot point with me whether other articles would not meet the same indications much more readily."

During the memorable epidemic of Savannah, in 1820, the ol. tereb. was extensively tried; but the success was more than doubtful. After remarking that there may be a stage of the disease in which this remedy might be applicable, Dr. Waring states that he had a record of 41 cases, 29 of which were marked as fair, for the administration of remedies. Of the whole number, 8

only recovered under the use of turpentine. In two or three most violent and malignant instances, it really appeared to him to have saved the life of the patient; at all events, the cure was accomplished. Dr. Waring states that his colleague, Dr. Kollock, was positively against it, and that in similar circumstances he would not administer it again. "In many instances, it increased the distressing sensations of the stomach; in some it brought on the burning pain, and appeared to accelerate the symptoms of black vomiting. The patient frequently remarked that it burned the stomach like coals of fire instantly after swallowing it. Very commonly, it tended to produce a dysenteric state of the bowels with a tenesmus, and, in all such instances, it unquestionably hastened the destruction of the patient by augmenting the degree of inflammation" (p. 64).

The turpentine is mentioned by Dr. Amiel, who used it at Gibraltar, in 1814, as a stimulant and diuretic in doses of 30 drops every two hours. Dr. A. says he has reason to believe that this medicine was very useful.¹ The turpentine is, in like manner, spoken of by Copland (iii. 181-3-4), as of useful application as a stimulant and astringent.

Very recently, this remedy has again been brought forward as an invaluable remedy in the treatment of the yellow fever, in a communication on the subject by Mr. James Laird, Surgeon of the Royal Navy, whose experience was obtained in the Bermuda Naval Hospital. Mr. Laird attributes the credit for originating the practice to Dr. Gilbert King, then in charge of that establishment. Dr. King, and like him Mr. Laird, used the turpentine in small doses. The latter does not consider that the yellow fever, in the general sense of the term, can be considered as a disease of the inflammatory diathesis; but rather, primarily, one of the blood itself. The turpentine is resorted to conformably to these pathological views. If the patient had received no medicine prior to admission, the primæ viæ were cleared out by a purgative dose of calomel or blue mass, along with a solution of Epsom salts, and the turpentine then given in doses of twenty minims in a little camphorated water, three times a day. If strangury occurred, which was rarely, sweet spirits of nitre was added. Small doses of tincture of opium, and also castor oil, were occasionally combined with it, in cases attended with frequent bloody, and otherwise vitiated dejection, or in an opposite state of the bowels. Besides these, sinapisms and blisters to the epigastrium, emollient enemata, and, during the stage of debility, negus, beef-tea, and cinchona injections, were resorted to.

The remedy is thought by Mr. Laird to be particularly indicated by the hemorrhagic character of the disease, depending, doubtless, on some change either in the chemical or vital properties of the blood; and by the speedy embarrassment of the different excretory functions, particularly that of the kidneys. "Turpentine is, therefore, a mere simple, innocuous remedy, which, besides its sedative, styptic, and antiseptic properties, possesses also the peculiar power, in virtue of its actions on the secretions and excretions gene-

¹ Amiel, in Johnson, p. 269.

rally, of re-establishing and keeping open the two great natural drains of the system—those of the skin and kidneys.” In evidence of the salutary effects of the remedy, Mr. Laird states, in a note, that 164 cases treated without it, at the commencement of the epidemic of Bermuda, in 1843, gave a mortality of 25, or 1 in 6.6; while in an equal number in whom the turpentine treatment was pursued, only 19 died, or one in 8.6.¹ The total number of cases treated with turpentine at the Naval Hospital of Bermuda, from 27th of August to the cessation of the epidemic in December, 1843, was 882. Of these, 80 died, or 1 in 11. The recoveries from black vomit were 24. In 1818, there were 105 cases and 28 deaths, or 1 in 3.8. In 1819, the number of cases was 106, and of deaths 25, or 1 in 4.2; and in 1837, the cases amounted to 140, and the deaths to 22, or 1 in 6.4.

We learn, also, from Dr. A. Smith, that the turpentine was advantageously used in the fever which prevailed in Peru in 1854. It was particularly useful in the hemorrhagic form of the disease, and, “when practicable, was used towards the close of the first stage, before the vital powers showed signs of sinking, and, therefore, before hemorrhage or black vomit had actually commenced.” “The result of the turpentine administered in doses of $\mathfrak{z}\text{i}$ by the mouth, or $\mathfrak{z}\text{ss}$ by the anus (in the advanced stage), several times daily, saved not fewer than 50 per cent. of the Indian race admitted in the Lazaretto in the last stage with black vomit, while the same remedy had but comparatively little power of curing this disease in the white or European race.”²

It remains to be stated, on the subject of the turpentine, that it was used in several cases during the late epidemic of this city, but without sufficiently perceptible advantage to entitle it to entire confidence. A few got well under its use—one after throwing up the black vomit; but, as it failed in many more instances, and patients got well without using it, we cannot justifiably attribute the cure, in the instances mentioned, to its agency.

Peruvian Bark.—Impressed with the idea of the existence of, or tendency to, putrefaction in the disease, or from a desire to strengthen and invigorate the system, recourse was early had to the Peruvian bark and other remedies of kindred character. By many in former times, and by not a few even in our own days, it has been used from the outset of the disease, after more or less preparation as regards the state of the alimentary canal, and with or without stimuli, according to the particular pathological views of the medical attendant. It forms the principal ingredient in the Spanish plan of treatment, pursued not only in the Peninsula, but in many parts of America. By most of the French commissioners sent at various periods to investigate the epidemics of Cadiz, Barcelona, Seville, and other cities of Spain, the early use of bark was commended; and in the West Indies it has enumerated warm advocates.³ Nor is it less true that in this country writers are now and then encountered who place much reliance on the same means.

¹ Lond. Lancet, Oct. 1853, p. 298–300, Am. ed.

² Ed. Med. and Surg. Journ., lxxxii. 201.

³ Bahi, in Pierquin, p. 108; Comoto, Mém. adressé au juges del Real Tribunal del Consulado de Vera Cruz—see Humboldt; Don Jose Ignacio Pombo, Noticias varias sobre

Notwithstanding the noted respectability of many of those by whom the use of bark, in the early stage of the disease, has thus been advocated, the well-ascertained tonic effect of the remedy, combined with our knowledge of the inflammatory or irritative nature of the fever, and the peculiar condition in the gastric organs at that period, would alone suffice to render the propriety of the course doubtful; and experience, here and elsewhere, has tended to confirm what theory points out. In 1793, the cinchona treatment was tried by Dr. Rush, at the recommendation of Dr. Stevens, of Santa Cruz, but was soon laid aside, as offensive to the stomach and otherwise injurious (iii. 126, 161, 227). It was found equally so by Dr. Currie (1799, p. 86) and others of our physicians. Dr. Bayley, in like manner, was disappointed, at New York, in 1795, with the effect of the remedy, and was compelled to give it up (p. 105). The practice was abandoned almost everywhere; and, from one extremity of this country to the other, it would be difficult to find, at the present moment, one physician of note disposed to rely on the exhibition of bark—in substance, infusion, or tincture—during the stage of excitement, or while the pulse remains active and the skin hot, and there exist signs of gastric irritation.

Chisholm, who notices the practice of the early use of bark, is loud in its condemnation (i. 365), except when other means have failed, and fears are entertained in relation to the issue of the case. He, however, used it, under peculiar circumstances, in the form of injection. Dr. Steward, of Grenada, as quoted by Chisholm (p. 367), also failed with it. Rochoux (p. 648) and Costa (pp. 61, 62) do not hesitate to attribute, in great measure, to the effects of bark and other tonic and stimulant remedies, so generally employed at Barcelona in 1821, the dreadful mortality which occurred there on that memorable occasion. And, assuredly, those who, while recollecting the appalling loss of life occasioned by the disease in other cities of Spain—at Malaga, Xeres, and Carthagen, for example—will be told that physicians had recourse to large doses of Peruvian bark from the outset of the attack, will admit that the result obtained was not encouraging. In cases, however, characterized from the outset by symptoms of prostration, weak pulse, and cool skin, or whenever it may be found necessary to impart tone and strength to the system and elicit reaction, the bark, in substance or tincture, finds its application, and has been repeatedly used with success.¹

By some physicians, the bark has been administered with a view to its tonic and febrifuge effects, as soon as the system presents signs of relaxation, with-

las Quinas Officinales; Arejula, pp. 15, 209, 291; *Ib.*, *Edinb. Med. and Surg. Journ.*, i. 450; Lafuente, *Observaciones Justificado, &c.*, Año 1805, p. 40; Bobadilla, in Bally, p. 536; Fellowes, p. 318; Blin, p. 12; Burnett, p. 258; Leblond, pp. 12, 113; Lefoulon, *Preface and Introduction*, also, pp. 84, 106, 329; Pugnet, p. 367; Valentin, p. 202; Savarésy, p. 301; Pariset, p. 587; Cassan, v. 97; Daniell, p. 121; Audouard, pp. 279, 288, 299, 301; Madrid, Pt. ii. 4, iii. 18–20; Guyon, *Réponse à M. Lefort*, p. 20; *Ib.*, *Arch. Gén.*, xii. 634; Kuhn and Stevens, *vide* Rush and Currie; Tully, p. 332; François, *Corvisart's Journ.*, vii. 12, 13; Repey, *Dissertation sur la Fièvre Jaune des Antilles*, p. 33; Moreau de Jonnés, p. 131; Gonzales, p. 322; Davidson, *Med. Reposit.*, i. 166; Alf. de Maria, p. 98; Sarravia, Lagasca, in Chervin, *de l'Identité, &c.*, p. 88.

¹ Currie, p. 52.

out, however, waiting for a complete remission.¹ Others, and by far the greater number, both in this city and elsewhere, do not prescribe it until a complete remission has been obtained by antiphlogistic or other means; while others, again, limit its employment to the last or sinking stage—giving it by the mouth, or, when the stomach is irritable, as is almost invariably the case, in the form of injection.

If this substance can, at the present day, be admitted at all as an appropriate therapeutic agent in the treatment of the yellow fever, it must only be so at the latter period, when, from its tonic, astringent, and perhaps antiseptic virtues, it may contribute to impart strength to the sinking powers of life, arrest the bloody exudation, and correct the putrefactive tendency of the system, if such exists. Hence it has been used at this period, not only by those who resort to it in the early stage, but by many who shun it at that period.² The practice of administering it in large dose immediately on the accession of the remission or metaptosis must, unless the disease be marked by extreme prostration, be, to say the least, a hazardous one.

It has already been stated that, as a general rule, the treatment at this period should be of the expectant rather than active kind. Everything tending to irritate or oppress the stomach should be avoided; and, without doubt, the Peruvian bark, in infusion, tincture, but more particularly in substance, is well calculated to produce these effects. In the more severe forms of the disease, when the remission is not complete, and is likely to be followed by symptoms of malignancy, such an irritation or oppression will, in general, be productive of injury—bringing on vomiting, and hastening the results it is intended to avert; while, in the mild forms, the agency of bark is not requisite, and it may be doubted whether, in these or any other cases, it possesses the power of preventing those morbid changes which constitute the last stage of the disease, and impart danger to the case. If these views be correct, little or nothing can be expected, in the stage of remission, from cinchona, so far as its febrifuge virtues are concerned; and if in any instance it is admissible, it can be only as a mild tonic, in small doses. In others, and in stronger doses, it will be apt to produce bad effects; and we hence find it opposed by several,³ and omitted by many more in the enumeration of remedial means proper for that stage. In the more advanced stage, the condition of the patient is different, and the remedy may be given in as large quantities as the stomach will bear, in substance or tincture; or, if injections are required, in strong decoction.

¹ Musgrave, ix. 138; Bertulus, Vanauld, and Joubert, quoted by Chervin, *de l'Identité des Fièvres*, &c., pp. 72, 74; Dutroulau, *ib.*, p. 61; Deveze, p. 271; Flores, see Fellowes, p. 312; Barnwell, p. 382; A. Hosack, p. 39; Moseley, p. 460; Holliday, p. 32; Bancroft, p. 75; Grant, p. 42; Gillespie, p. 80; Humboldt, pp. 780, 781; Gilbert, p. 83; Bruce, p. 281; H. McLean, p. 122; Caillot, p. 299; Powell, p. 14; Hunter, p. 88; Wood, i. 313; Tully, pp. 325, 332; Bally, p. 562; Mackittrick, p. 66; Shecut, p. 123; Seaman, pp. 44, 45; Hume, p. 228; Dariste, p. 197; Desperrière, p. 80.

² Nassy, p. 36; Dickson, *op. cit.*, p. 280; Wood, i. 313; Davidge, p. 122; Merrill, iii. 227; New Orleans in 1839, p. 339; Hillary, p. 156; Moseley, pp. 460–465; Evans, pp. 303–5; Arnold, p. 54; Monges, *N. A. Journ.*, ii. 97.

³ Chisholm, p. 365; Rush, p. 227.

CHAPTER XXXVII.

TREATMENT—CONTINUED.

Sulphate of Quinia.—Since the introduction of the sulphate of quinia in medical practice, this important therapeutic agent has been substituted for the Peruvian bark in the management of the yellow fever. Like this medicine, it has been used in the several stages of the disease. In the last stage, it recommends itself in virtue of its tonic and astringent properties, and doubtless is then productive of beneficial effects. By many and high authorities it is strongly recommended as a remedy of great and unusual efficacy when resorted to towards the close of the first, and during the second or apyretic stage.

Writing in 1825 or '6, Dr. Lefort, of Martinique, whose field of observation, as chief physician of a very large military hospital, was very extensive, in describing the state of asthenia occurring at the period of the remission, and acknowledging the failure of other remedies, states that he was led, from his great success with the sulphate of quinia in other fevers, to resort to it in the yellow fever. "Its effects have surpassed the hopes we had entertained of it. We have given it in doses of two or three grains, dissolved in the smallest possible quantity of water, and the stomach has retained it. This dose was repeated every two or three hours; and the vomiting has gradually lessened, and finally ceased altogether." "The absorption, or rather the digestion, of this remedy," Dr. Lefort adds, "is instantaneous—the patient experiencing, soon after its ingestion, a sensation of heat at the epigastrium, which irradiates and extends sympathetically to the other viscera. Under the influence of the sulphate of quinia, continued during two, three, or four days, and given three or four times in the twenty-four hours, the sanguine exhalation ceases, and the tongue, which was still loaded in the centre, cleans off, becomes covered with a white mucus, and returns to its natural state." Other physicians, cited by Dr. Lefort, have employed the quinia successfully under like circumstances.¹ It was used advantageously in New Orleans in 1837 and 1839, and also in other places.²

But though so highly extolled when used at this period of the disease, the sulphate of quinia has not proved equally successful in the hands of other

¹ De la Saigné et du Q., p. 570.

² Thomas, pp. 105, 125; Rapport, p. 254; Lallemand, p. 147; Wallace, xlv. 280; Kelly, xiv. 392; Dariste, p. 197; Wood, i. 313; Imray, liii. 87; New Orleans in 1839, p. 339; Chisholm, Charleston Journ., x. 452; Barbe, in Thomas, p. 137; Halphen, p. 254; Chabert, Reflexiones Médicas y Observaciones sobre la Fiebre Amarilla, p. 187; Chevé, p. 29; Arnold, p. 54; Magruder, N. O. Journ., iv. 693; Joubert, p. 975; Fever of Cayenne, 1850, p. 181.

physicians in different seasons and places. Dr. Harrison, of New Orleans, states that when administered by him on the third or fourth day of the disease, it invariably produced an injurious effect—increasing the irritability of the stomach, and causing the tongue to become dry (p. 331). During the epidemic of Dominica, in 1838, Dr. Imray found the quinia, after the febrile symptoms had subsided, and the state of collapse came on, completely inert, or exercising no appreciable influence on the system (liii. 88). In 1841, the same writer was still unsuccessful in the use of the remedy (lxiv. 329); and Dr. Thomas, of New Orleans, who used the sulphate efficaciously in 1837, found it much less beneficial in 1839 and 1841 (pp. 105, 125).

In the year 1821, the sulphate of quinia was used, though unsuccessfully, by Audouard (*Pariset*, p. 585) in the early stage of the disease at Barcelona. Dr. Daniell employed it in like manner at Savannah before 1826. Dr. Chevé used it in the epidemic of Goré and St. Louis (Senegal) in 1830; administering large doses after obtaining an artificial remission by means of bleeding, revulsives, &c.¹ In New Orleans, Dr. Halphen was the first to use the sulphate of quinia before the close of the first stage. This was during the epidemic of 1837. The practice, as we are told, was *regularized* by Dr. Lambert, and “produced a true revolution in the therapeutic of the yellow fever.”² Dr. Beugnot, of the same city, used it at first in large doses—twenty-six to thirty-six grains in twenty-four hours—after producing, like Chevé, an artificial but well-marked remission by means of large bleedings, &c. Finding that the remedy produced bad effects if the patient complained of *malaise* and his skin was hot and dry, he subsequently used it immediately after one syncopal bleeding, and before the system had reacted.³

Dr. Levacher, who practised at St. Lucia, and the second edition of whose work appeared in 1840, employed the quinia before the accession of the metaptosis, as soon as the force of the circulation had been reduced—or immediately, if the loss of blood was not called for. Dr. Levacher combined the quinia with calomel and camphor; and commencing with doses of three grains, pushed the remedy to twenty-five, thirty, forty, and fifty grains in the twenty-four hours.⁴ In the disastrous epidemic of Georgetown, Demerara, in 1837, large doses of quinia, combined with almost as much calomel, and given at the outset, constituted the favourite treatment. Dr. Blair tells us that when a physician was called to a case of yellow fever in the formative stage, or within a few hours after the development of the first stage, if he prescribed twenty grains of calomel and twenty-four of quinia, and in six hours followed it by a large dose (ʒiij) of castor oil, he would, in perhaps nine cases out of ten, immediately arrest the disease. If this dose was unsuccessful, the calomel and quinia were repeated every six hours, as long as there was no contraindication, and as long as the first stage existed. “Very early in the first stage,”

¹ Relation de deux Epidemics de Fièvre Jaune qui ont régné à Gorée et à Saint Louis pendant l'hivernage de 1830, p. 29. Paris, 1836.

² Rept. on Epid. of 1839, p. 254; see also Chervin, Gaz. Méd. de Paris, 27 Jan. 1838.

³ New Orleans Med. and Surg. Journ., i. 26, 27.

⁴ Guide Méd. des A., p. 97.

says Dr. B., "it might be designated a specific, I believe" (p. 108). The treatment was prejudicial in the second and third stages (p. 109). The practice has since been pursued with slight modifications by Dr. Cummins,¹ Dr. Paton,² and Dr. Finlay.³ The latter has given one hundred and thirty grains of quinia, and the same quantity of calomel, in thirty-six hours.

In 1839, the practice of giving quinia in the opening stage of the fever, was introduced at the Charity Hospital of New Orleans, by Dr. Mackie, at the suggestion of Dr. Hunt. The latter was led to believe in its efficacy from the report of Dr. Maillot on the fever of Bona in 1832 and 1835, in which he administered the quinia in large doses at the outset of the disease. The mode adopted by Dr. Mackie "consisted in waiting for no abatement of the fever, but in promptly giving the sulphate of quinia as soon as possible after the attack. The dose varied from twenty to eighty grains, given in a little cold water. If the stomach was very irritable, it was given by injection. When the practice was first introduced, a cathartic was generally first given, and the administration of the quinia deferred until the bowels were moved. This practice was afterwards abandoned on account of the loss of valuable time. In strong and robust constitutions, the lancet was employed for the purpose of producing a temporary remission, during which the quinine was given." "If the first dose failed in eight or ten hours to produce an apyrexia, a second was given. The earlier it was given the better; but it should not be given after the second day." "In addition to the sulphate of quinia, other remedies were used, according to the circumstances of the case."⁴

This mode of treatment was adopted and highly extolled by other physicians. Dr. Harrison says: "The fever in most cases was cut short as if by enchantment. I shall never forget the surprise I felt the first time I witnessed its effects."⁵ Dr. Fenner, in his description of the fever of New Orleans, in 1849,⁶ states that, in furtherance of the views set forth by him in his account of former epidemics,⁷ he trusted fully to the "*abortive method by quinine*," and with results entirely satisfactory. He assures us that when called to a case within twenty-four or thirty-six hours of the attack, he seldom failed to cut short the fever by large doses of the sulphate of quinia in combination with opium or morphia, frequently followed by a little blue mass or calomel.

Dr. McCormick commences the treatment with a purgative enema and a mustard foot-bath. These are followed by fifteen to thirty grains of quinia, to *subdue fever*. If the pain in the head is very violent, bleeding by means of the lancet or cups to the mastoid processes is resorted to—but not otherwise. When the quinia has subdued the excitement, fifteen or twenty grains of calomel, with or without as much quinia, are administered.⁸

In the hands of Dr. Wedderburn, of New Orleans, the sulphate of quinia

¹ Lond. Lancet, 1853, ii. 218, Am. ed. ² Ibid., ii. 288.

³ Ibid., i. 469.

⁴ Harrison, N. O. J., ii. 331.

⁵ Op. citat., p. 332.

⁶ Southern Med. Rep., ii. 117, 118.

⁷ N. O. Med. and Surg. Journ., iii., iv., and v.

⁸ Ibid., v. 209.

plays a conspicuous part from the outset of the case. After an enema, consisting of a tablespoonful of mustard and a quart of water, and a hot foot-bath, a powder composed of ten grains of rhubarb, two of ipecac., five of calomel, and twenty of quinia, is administered at once. Sometimes, fifteen or twenty grains of quinia, with thirty drops of laudanum, are given, followed immediately by the above powder. This purges freely in six or eight hours; and then the quinia and laudanum are repeated according to the pain and fever. If the attack is very severe, and the patient suffers violent pain in any part, twenty or thirty grains of quinia, with forty or fifty drops of laudanum, or two or three grains of opium, are administered. This rarely fails to extinguish both fever and pain in a few hours. Bleeding is never, and local depletion seldom, resorted to.¹

Dr. Fenner gave twenty or thirty grains of the remedy, with a fair dose of opium or morphia—using anteriorly a mustard foot-bath, and a purgative enema. “This would generally reduce the vascular and nervous excitement completely in the course of a few hours, throw the patient into a profuse perspiration, relieve all pain, and produce sleep. The bowels were kept open by some gentle means, and more or less quinia was repeated as occasion required. We recollect but one fever patient that required cupping, and we did not have a single one bled from the arm.”²

In Martinique, also, during the epidemic of 1840, Dr. Rufz, who, the year before, had derived but doubtful benefit from the sulphate of quinia, used it advantageously. He gave it in doses of nearly fifty grains in twenty-four hours—not, however, without premising venesection. The epidemic, it must be added, was in its decline, and the cases were of a mild character (p. 55).

The practice appears to have proved successful at Mobile, in 1853, in the hands of several physicians. Dr. Anderson says: “Your reporter, and his associate in practice, being much prejudiced in its favour, from the experience of former years, used it from the very commencement of the epidemic to its close. They treated upwards of eleven hundred cases, and, without claiming any superiority in success, they feel sure that their tables of mortality will compare favourably with those of any other physicians in the city. They used quinine in almost every case, regardless of age, sex, idiosyncrasy, or any other circumstance. They have every reason to be pleased with their manner of treatment, and, with their present experience, would not exchange it for any other that they have heard of. The marked and almost magic effect of a large dose of quinine at the outset was so apparent, that they would have considered it little short of trifling with human life to have adopted any other treatment. They will not deny that there were cases in which it did no good; in fact, in those cases where there was, at the commencement, decided congestion of the brain, it may sometimes have done harm, but such cases were very few, and could hardly have been aggravated by any medicine that could be given.”³

¹ New Orleans Journ., v. 209.

² Southern Med. Reports, ii. 117, 118.

³ Anderson, Fev. of Mobile in 1853, Trans. of Ala. Med. Assoc. 1854, p. 45.

Dr. Anderson used quinia with calomel or blue mass—twenty-five grains of the former, and fifteen or twenty of calomel. If rejected, the medicine was repeated at once. It was again resorted to in twelve hours, less five grains of the quinia, and five or ten of the calomel. If the second dose produced bilious discharges, the following was ordered:—

R.—Quinia, 25 grs.; calomel, 10 grs.; blue mass, 15. Make 10 pills, three of which to be given every two hours.¹

It is scarcely necessary to remark that the propriety of the practice of the early exhibition of the quinia—apart from the encouraging effects said to have been observed from it—is principally based on the opinion entertained by many that the remedy exercises not only an antiperiodic agency which renders it suitable to the management of all diseases arising from malarial contaminations, but a sedative effect on the system at large. But the latter opinion, which, originating in Italy some thirty years ago, has received the support of several French, English, and American physicians,² is not, after all, sufficiently authenticated to justify a resort to the remedy during the state of excitement of febrile diseases—be their nature what it may—on the score of that supposed agency. Hence no fault can be found with those who, in the present state of the question at issue, are skeptical as to the wonders related of quinia, and feel disposed to question the propriety of administering very large doses of it at the commencement of a febrile attack, without waiting for a marked remission, and especially during the continuance of high arterial action and symptoms of local inflammation or irritation.

It can scarcely be necessary to remark that this practice—for which Dr. Dundas³ claims credit, but for which the profession in this country was long before his time indebted, some say (for this important point, if important it really be, has not yet been satisfactorily settled, there being several claimants in the field) to Dr. Thomas Fearn,⁴ of Alabama; others to Dr. Metcalf,⁵ of Miss.; and some, again, to Dr. Perrine,⁶ of the same State—this practice, I say, though enumerating warm and respectable advocates both on this and the other side of the Atlantic, cannot be said with certainty to have proved, in the hands of every good and safe practitioner who has tried it, as advantageous, unless, perhaps, under exceptional circumstances, as its originators and partisans so confidently assert.

To whomsoever the bright idea may have suggested itself among us, the

¹ Anderson, Rept. of Alab., &c., p. 46.

² Bailly, *Traité des Fièvres Interm.*, 1825, p. 424; Guersant, *Dict. de Méd.*, xxvi. 564; Geromini, *Annali Univ. de Méd.*, March, 1841; Blair, p. 107; Drake, p. 746; Bell's *Lectures*, ii. 782; Boling, *Am. Journ.*, N. S., viii. 89; N. O. *Journ.*, v. 208; *Ib.*, ix. 318, &c.; *Ib.*, *Southern Reports*, ii. 349; McCormick, *N. O. Journ.*, ii. 175; Holmes, A. J., N. S., xii. 304; Merrill, *N. O. Journ.*, vii. 161, 163; Upshur, *Stethoscope*, ii. 437; McCaw, *ib.*, 666, &c.; Desiderio, *Comptes Rendus de l'Acad. des Sciences*, ix. 509; Bally, J., *Gén. de Méd.*, Oct. 18, 1829, p. 7; Mérat et De Lens, *Dict. de Mat. Méd.*, v. 607; Jacquot, *Arch. Gén.*, 1845, vi. 76; Briquet, *Traité Thérapeutique de Quinquina*, pp. 21, 42, 107, 123.

³ *Sketches of Brazil*, pp. 287, 291, &c.

⁴ *Fenner's Southern Reports*, ii. 346.

⁵ *Ib.*, i. 352.

⁶ *Amer. Journ.*, xi. 250.

practice is evidently but a revival of the one suggested and resorted to as early as the close of the seventeenth century, by Morton—adopted not long after by Torti, Burserius, Werlhof, Trnka, and other physicians of the times, and highly eulogized, so far as regards its applicability to yellow fever, by Arejula, Sarravia, Lafuente, Bobadilla, and the majority of Spanish physicians, as well as by Valentin, Cassan, Lefoulon, Guyon, Savarésy, Stevens, Kuhn, &c., and others already mentioned—of administering the Peruvian bark in the largest possible doses, in all stages of the disease, after little preparation, or without any preparation at all; and we all know that the success obtained from it, though satisfactory in a few cases, has not been such as to encourage its general adoption.

The theoretical views upon which the practice was predicated were, doubtless, somewhat different from those by which the advocates of the quinia treatment, above referred to, are guided; but the effects claimed are similar. Forget, for a moment, that the writers cited are speaking of cinchona, and not of quinia, and you may fancy that the latter remedy is the subject of their remarks. Upon the administration of a large dose, it was said, the pulse is reduced; the skin cools and moistens; thirst, if it existed, disappears; the tongue becomes clean and moist; in a word, fever vanishes as by enchantment; and if the patient experiences a little uneasiness about the head, a little dizziness, perhaps a slight buzzing in the ears, the whole soon subsides, and convalescence follows. Such were the effects claimed for bark, in the class of diseases in which the quinia, used in the way mentioned, is said to act as by magic. Is not the fate of the former practice calculated to raise some doubts as to the permanence of reputation of the latter?

Let it be said, *en passant*, that experience will doubtless one day demonstrate, to all whose eyes are not blinded by theory, that more noise has recently been made about the abortive plan of treating yellow and other fevers by means of scruple or even larger doses of quinia, given, with or without preparation, at the outset of the attack, with the intention of arresting their progress, than is warranted by the nature of the results obtained. Entitled as the authority of many of the advocates of the plan may be to our respect, it may be fairly suspected that, in singing its praise so loudly, and proclaiming its superiority over every other heretofore pursued, they have allowed their imagination to get somewhat the better of their judgment. To this conclusion I am the more inclined, because it is yet to be proved that these fevers, when once established, can be arrested in their course; and because the practice has failed, and even proved detrimental, in the hands of other observers, so far especially as regards the yellow fever.¹

Dr. Kelly, of Mobile, states that his experience is not in favour of the practice. "I have," he says, "employed the sulphate of quinia in every stage of the disease and in sufficiently large doses, but with very doubtful advantage excepting during the apyrexia, and in cases uncomplicated with

¹ Stone, N. O. Journ., ii. 184, &c.; Dickson, Charleston Journ., i. 14; Lewis, N. O. Journ., i. 425, 427; *Ib.*, iv. 174; N. O. Journ., x. 279.

any local determination. When injudiciously administered, it appeared to have hastened the occurrence of the black vomit." Others have, likewise, found it hurtful.¹ In the last epidemic of Barbadoes, the quinia and calomel treatment, though not injurious, failed, except, perhaps, in some of the earlier cases.²

In one of the latest accounts we have of the yellow fever in the West Indies,³ it is said: "Quinine appeared to be powerless; it was tried extensively from scruple doses downwards, and latterly was given only as a tonic in small doses in convalescence. What effect can quinine have in a toxæmic epidemic? Philosophy must repudiate it." The following remarks from a physician of New Orleans will show that even in that city the opinion respecting the superiority of the quinia treatment is not unanimous. "Since the introduction of the appropriately named *abortive treatment* by enormous doses of quinine, conjoined with powerful narcotics, the patients who recover from yellow fever frequently remain miserable valetudinarians, while blindness, deafness, and insanity, too frequently follow in the dismal train." "Under the modern or abortive practice, our watering places in the neighbourhood have been crowded with emaciated convalescents from yellow fever, many of whom have cause to lament, in a ruined constitution, or impaired faculties or intellect, the pernicious consequences of the abortive practice."⁴ Even physicians who had, at one time, been the strongest advocates of the quinia abortive treatment, found reason at last to change the burden of their song. From 1847 to 1853, Dr. Fenner⁵ says, he was able, promptly, to cut short nearly all the cases of yellow fever he treated; there was but one year (1851) in which there was not a good deal of it to be treated. Several other highly intelligent physicians of New Orleans, he adds, did the same. Their chief remedies for this purpose were large doses of quinia and opium, given at the onset of the fever. In 1853, however, this mode of treatment was soon found not to display that controlling influence over the disease which it had done during the preceding six years.

In this city, during the epidemic of 1853, and the slighter visitation of the next year, it was fairly tried in large and small doses during the early stage as an abortive, and during the second stage as an antiperiodic, and finally during the last as a tonic. But I cannot now recollect a single case in those I saw, or that were reported to me, in which the remedy had the slightest beneficial effect in the first of these capacities. It failed completely, however combined, to control effectually the disease. In some of the milder cases, especially some which occurred in 1854, it appeared, when administered at the close of the first stage to act advantageously; and, in a few instances, it produced a good effect as an adjuvant to other means resorted to with the view of supporting the sinking powers of life. But, even in such instances, its effects were not superior to many, and, indeed, inferior to several others.

¹ Penell, of Pinckneyville, Stone, p. 186.

² Davy, Notes to Blair, p. 109.

³ Furlong, Fever of Antigua, in 1853, Lancet, Dec. 1853, Am. ed., p. 441.

⁴ McFarland, List of Interments in all the Cemeteries of N. O., 1853, p. 9.

⁵ Fever of 1853, p. 59.

It is far from being certain that in cases in which the quinia is supposed to have produced the miraculous effects attributed to it, the apyrexia would not have taken place and the cure been obtained had the remedy not been used. We have seen that Dr. Rufz succeeded with it only in mild cases, and that similar results were obtained in this city—in a word, it is apparently successful only in the class of cases which, in the hands of other physicians who do not use the remedy, seldom prove fatal, or do not in a larger proportion than when treated otherwise. Nor is it certain that the cessation of febrile excitement obtained through its instrumentality—supposing the agency undeniable—should be regarded as the harbinger of cure in cases of a severe kind; for, as Dr. Harrison himself remarks, the disease runs its course even after all febrile symptoms have subsided, having, for what we know, succumbed to the overpowering action of the quinia. If such be the case, and if death occurs at the usual period, notwithstanding the subsidence of the fever, we cannot be certain that the recovery of the patient, when it does take place, is truly to be ascribed to the sedative or antiperiodic, or any other virtue of the quinia, and not to the other means employed, or the power of nature.

In view of these circumstances, and independent of other considerations already referred to, I cannot but entertain some doubt as to the very great superiority of the quinia treatment, and especially of what has been called the abortive plan. To this opinion I am the more inclined, seeing that several of its more zealous advocates would extend the practice to every other form of fever named in the books—typhus, typhoid, &c.—in which, when resorted to by other equally skilful physicians, in this country and elsewhere, quinia, as an abortive, sedative, or specific, has failed to produce the anticipated effect,¹ unless, perhaps, the disease had assumed a decidedly remittent or intermittent type, when it required no prophet to tell us it would be useful. It is true that, by the warm supporters of the plan in question, no heed is taken of the opposition it has encountered; but, on inquiry, we cannot find that any stronger reason has been assigned (and, taking all things into consideration, it cannot but appear extraordinary that something better could not have been offered) for the failure experienced, even with what might well be regarded as classical doses of the panacea, than that physicians who recount their ill success would have obtained opposite results had they only given the quinia a fair trial; in other words, administered it earlier and in larger quantities. To those who have no hobbies to ride, the fact of these repeated failures, to say nothing of certain analogies they may bear in mind, and sundry theoretical views they may entertain respecting the pathology of the disease and the mode of operation of the remedy, will be sufficient to deter them from joining in the hosannas sung in some quarters; while the allegation that southern and western physicians, who, we should think, cannot

¹ Gibbs, in Fenner, ii.; Boling, N. O. Journ., ix. 2, &c.; Maggibbon, N. O. Journ., x. 25, 36; Scruggs, N. O. Journ., x. 206; Gordon, *ib.*, pp. 146, 210; Fletcher, Braithwaite's Abstract, July, 1853, p. 264; Grant, Am. Journ., N. S., xxvi. 104; Barclay, Med. Times, Jan. 8, 1853.

be accused of over-timidity in the use of remedial agents—witness the history of calomel, tartar emetic, and the lancet, among them—have not been struck with the success of the abortive method, simply because they had not been heroic enough with quinia, which the most cautious among them daily use in doses which cause astonishment in excellent and skilful practitioners elsewhere, will appear passing ludicrous, and may recall to their minds, as it has done to mine, a certain passage in *Gil Blas*, in which the great Sangrado—the worthy prototype of more than one physician of an era not very remote from our own—accounts for the loss of the Canon Sedillot, whom he had, as a matter of course, bled profusely and deluged with warm water. I quote the original: “Comme il rendait les derniers soupirs le médecin parut, et demeura un peu sot, malgré l’habitude qu’il avait de dépêcher ses malades. Cependant loin d’imputer la mort du chanoine à la boisson et aux saignées, il sortit en disant d’un air froid qu’on ne lui avait pas fait tirer assez de sang ni fait boire assez d’eau chaude.”

By Dr. Wallace it is, in congestive cases, given conjointly with mercury (lxvi. 280); but, though probably useful in some cases, too much reliance must not be placed upon it. Dr. Imray found it inefficacious in arresting the progress of the malady, if given at the commencement of those cases which were accompanied with extreme debility and vital depression.

Muriated Tincture of Iron.—During the epidemic which prevailed at Savannah during the summer and autumn of 1854, the muriated tincture of iron was extensively used and highly commended as an invaluable remedy in the treatment of the disease by Dr. Wildman, and subsequently by Dr. S. N. Harris. The latter, in a communication on the subject, says:—

“It has been observed by some of my medical brethren that its use was at variance with all our preconceived ideas of the pathology of yellow fever, and I confess myself to have entertained similar views when it was first suggested to me, in consultation with another practitioner, as a remedy for the fatal black vomit. But I am sure that many of the articles of our *materia medica* have been from time to time improperly classified; and, from the experience, during the last two or three years, of tincture of iron in erysipelas, it cannot reasonably be considered as contra-indicated by the existence of inflammation. I am confident, from close observation of its effects in a large number of cases of yellow fever, that its action is that of a refrigerant diaphoretic; that it allays pain and produces sleep, and, by preserving the integrity of the blood, enables the system to resist the depression so universally attendant upon the second stage of the disease. It presents, in fact, a very happy combination of hydrochloric acid, in excess, with iron, the former of which, it is well known, has been given with great success, by the celebrated Dr. Paris, in malignant forms of fever; while the latter (iron) has been universally acknowledged as an incomparable tonic from time immemorial.

“I concur entirely with Dr. Wildman, that it should be exhibited at the earliest possible stage of the fever, and, when possible, without awaiting the preliminary action of any other medicine. It is well known that the revulsive influence of a salivation has always been regarded as the great desideratum

of the mercurial treatment in yellow fever; and I am convinced that it will be no small recommendation of the muriated tincture of iron to the profession, when it is understood, as I am now prepared to assert, that it will produce salivation in a much greater number of yellow fever cases than calomel will. This effect, due to the free hydrochloric acid, is very far from being attended by the pain and discomfort of mercurial ptyalism, and is, moreover, not at all indispensable to a cure.

“With respect to the dose, it must necessarily be discretionary with the practitioner, and it cannot be necessary to remind the profession that even the *United States Dispensatory* allows a maximum of two fluidrachms. There cannot, I think, be any question of its decided utility; and, in conjunction with sinapisms and blisters, it will be found to diminish the mortality to a very inconsiderable proportion, when brought into action before the super-vention of black vomit.”

In a private letter on this subject, under date of September 5, Dr. Harris states that his practice was to give twenty drops of the remedy every two hours, from the very onset of the disease. If symptoms are threatening, he increased the dose to a fluidrachm every two hours, or even every hour. If the head became affected—that is, if there was stupor or delirium—he left it off and purged. Out of two or three hundred cases treated in this way, Dr. Harris states that he lost but six.

This report, if true, was certainly encouraging. Unfortunately, the remedy, though so advantageous when administered to the sufferers of Savannah, was powerless in the cases of those to whom it owed its celebrity, for both Dr. Wildman and Dr. Harris fell victims to the prevailing epidemic. The muriated tincture of iron does not appear to have made many converts in Savannah. In Baltimore it was resorted to by Dr. Monkur in the treatment of some cases which occurred there in September and October, 1854. Laying aside all other treatment, the patients were placed under 20 to 40 drops of the tincture, diluted in a wineglassful of cold water, repeated every three hours. When hemorrhage of the mouth and gums was present, a stronger solution was used as a wash, by means of sponge-moss. The sick were made to take beef essence as nourishment, as freely as possible. Where the surface was hot, cold water sponging was faithfully carried out. *All these unfavourable cases recovered.*

Other patients, with severe attacks of the fever, were seen by Dr. Monkur on the day of the invasion. One, a robust youth, previously perfectly healthy, a blacksmith by occupation, on the first day of his fever, had acute delirium. He was bled from the arm, copiously; this was followed by cups to the spine, and mercurial and saline purgatives were administered to free purgation. Notwithstanding this activity of treatment, he had black vomit on the third day. The previous treatment was discontinued, and the iron administered in fifty drop doses, repeated every three hours, with a few doses of charcoal in water. This checked the vomiting, when the iron was continued in reduced doses and at more extended intervals. He recovered. The remaining cases, when first seen, were actively purged by calomel, rhubarb, and saline aperients.

Immediately thereafter the iron was freely and repeatedly given. "During the prevalence of the fever," Dr. M. adds, "there were a number of fatal cases. I did not witness an unfavourable termination, and attributed the success to the iron. I am not informed of the iron having been used by other practitioners. You will have perceived that in my cases it was largely administered, internally and as a wash for the mouth, and was the only remedy prescribed, except the purgative preparation."¹

Having never seen the iron used in more than some three or four cases, and then unsuccessfully, I have nothing to say in reference to the above statements. Happy the physician who can boast of such a success! Results of the kind are not every day obtained in the yellow fever.

Oil.—Encouraged by the reported benefit of olive and other oils—administered internally and applied externally—in the cure of the oriental plague and other forms of pestilential and malignant diseases,² not a few physicians have resorted to them in the treatment of the yellow fever; and, if we are to credit the reports published on the subject, the effects obtained have been of the most satisfactory kind. Oil frictions were originally employed by Dr. Keutsch, of Santa Cruz, and in his hands succeeded admirably.³ But it is mainly by Spanish physicians, both in Europe and South America, that the remedy has been used and eulogized. Among the former, Dr. Juan Arrias, physician to the Saint Charles Hospital at Carthage, stands prominent. According to the published account of the result of his practice, it would appear that of seventy cases, in which these frictions were used three times a day over the whole body, sixty-eight recovered. Subsequently, Dr. A. repeated the experiment on one hundred individuals, admitted in an advanced stage of the disease. Of these, five died on their way to the hospital, and two on the day after their arrival. All the rest recovered. In 1803, forty patients were landed from a frigate coming from Cadiz. They all recovered under the use of oil frictions.⁴ The same mode of treatment is reported to have been used, with success, by Dr. Ximenes, of the Havana,⁵ and various physicians in Vera Cruz.⁶ Of thirty-six patients affected with black vomit, who were admitted into the hospital of that city, thirty are said to have been cured by frictions with heated oil.⁷

All this, if correct, is certainly encouraging; but notwithstanding the imposing array of names cited in its favour, the great utility of the practice in yellow fever is not placed beyond a possibility of doubt. Not a few, indeed, think that the statements of the unsurpassed success obtained from the means in question must be received with some degree of suspicion. Dr. Rush (iv. 30) and Dr. Bally (p. 548), though having themselves no personal experience with the remedy, have nevertheless attempted to explain its mode of opera-

¹ American Med. Gaz., vi. 104.

² See St. James, v. 14; Celsus, lib. 3, cap. 7; Louis of Pavia, Bontius, Desgenettes; Chateaubriant Itinéraire, ii. 25; Valentin, p. 232.

³ Luzuriaga, ii. 218.

⁴ Prépétit, Fièvre Jaune, p. 21.

⁵ Luzuriaga, ii. 227.

⁶ Humboldt, p. 782.

⁷ Bally, p. 547. Copland, iii. 181; Dariste, p. 207; Kéraudren, p. 8.

tion. I shall not follow them into the consideration of the subject, or inquire how far we may depend on the statements referred to, thinking, as I do, that it would have been far better to establish the fact of the efficacy of the remedy before undertaking to account for it, and that few physicians, in this country particularly, will feel disposed to trust to a remedy of the kind in the management of so formidable a disease. It may be stated that Dr. Bally does not, from all he could gather in Spain, place much confidence in oil frictions, and states that at the hospital of Eciija they did not prove successful in the hands of Dr. Payra Sarravia (p. 548). Dariste, at Martinique (p. 207), was equally unsuccessful with them.

By other physicians, much dependence has been placed on the internal use of oil, with a view to calm irritation and destroy the corrosive effect of the poison. By Dr. Dalmas, small doses of fresh castor oil are highly spoken of to remove the acrid and burning sensation complained of in the fauces or œsophagus. He refers to my father, who resorted to the same means in the various epidemics of this city (pp. 183, 184). Dr. Barnwell used the oil of almonds, in mixture with molasses, in doses of a teaspoonful, frequently repeated, to soothe the burning heat of the stomach (p. 379). Olive oil, in large doses, often repeated, has been much used in Mexico, as well as in the Havana and other Spanish colonies.¹ The practice acquired some reputation in Spain, during the epidemic of Barcelona, where it was somewhat largely employed by a monk of the name of Father Constans. At the onset of the attack, he administered two ounces of the oil combined with hot marshmallow or violet flower tea. The dose was repeated two or three hours after. At the end of the fourth hour, a purgative enema was administered. This was followed, an hour after, by large and repeated draughts of hot marshmallow tea, sweetened with vinegar syrup. The patient was then left quiet during two hours, when the hot drinks were again resorted to, and repeated every two hours for a short time. The effect is said to have been a copious perspiration and quick recovery.² Dr. Audouard seems to place some reliance on this mode of treatment; but Dr. Rochoux, who was then at Barcelona, so far from coinciding in sentiment with his countryman, very properly regards the reputed success of the oil as chimerical and unworthy of attention. If Father Constans derived benefit from any of the means employed, it was probably from the warm drinks he administered so plentifully, and which brought on a copious perspiration, and not from the olive oil.

Melambo.—Audouard³ speaks in terms of praise of the effects of the bark of a tree denominated, in Spanish, melambo, which grows near Carthagena, in South America. The article is stated to possess a strong aroma, which is more especially sensible to the taste, and approximates to that of the *calamus aromaticus*. This is succeeded by a strong bitterness, resembling closely that of the Peruvian bark. Trials with it were made by Messrs. Columbi and San German, during the epidemic of Barcelona, and the success, according to Audouard, was very encouraging. It was also administered, with reputed

¹ Humboldt, p. 782; Bonnardet, in Kéraudren, pp. 8, 9; Dariste, p. 207; Bally, p. 547.

² Audouard, p. 307.

³ *Ibid.*, p. 304.

good effect, in one of the convents of that city. The remedy, on further trials by other physicians, was, however, clearly shown to be destitute of the remedial virtues attributed to it.¹

Charcoal.—Powdered charcoal has long been employed as a useful remedy in intermittent as well as in some forms of gastric fever and dysentery, and also to relieve nausea and vomiting in the ordinary fevers of warm climates or seasons. The same remedy was tried in yellow fever by a Dr. Merly, of Barcelona,² and forms the subject of a little volume published in this city some years ago by Mr. Plantou.³ No one, so far as I can discover, not even Dr. Merly himself, derived such benefit from this remedy as to tempt others to repeat the experiment; while a perusal of Mr. Plantou's publication will soon convince the reader that his testimony must be received with many grains of allowance.

Moral Treatment.—So far we have dwelt on the medical treatment of yellow fever. But, important as this must ever be considered, the physician who would, while attending to it, neglect to devote attention to what may be regarded as the moral management of the sick, would fail to perform a very useful part of his duty, and run great risk of neutralizing the effects of his remedial means. In no disease is it more necessary to keep up the spirits of the patient. We have seen that the eyes of the patient have a peculiar glistening appearance; his features indicate the existence of mental anxiety; and he looks as though he would endeavour to read in the countenance of his physician or attendants the probable result of his case. Unless this condition is removed, remedies are of little avail; for every day's experience teaches that individuals armed with a large share of moral courage, and who feel no apprehension relative to the issue of their disease, have a far better chance of recovery than those who labour under the opposite condition of mind. Hence, the propriety of the physician visiting his patient with a cheerful countenance. Should the latter be alarmed at his condition, he must be told that his fears are groundless, that there is no reason to apprehend danger, and that all will be right again in a short time. "I have always observed," says Dr. Finlay, "that by keeping the patient's spirits up, and, if possible, having some one to draw him into conversation, so as to make him forget (in a manner) his illness, I gained a great deal of ground towards establishing his convalescence" (p. 12).

Diet.—I have now finished all that need be said relative to the medical treatment of the yellow fever; and passed in review the various means employed for that important purpose. It remains to add something on the subject of the diet to which the sick should be subjected—of the course to be pursued during convalescence, and of the prophylactic measures suited to guard against the spread of the disease.

As may readily be foreseen, from the result of experience, in other fevers of kindred character, the diet, in the first stage of the inflammatory form—

¹ Pariset, p. 589; Rochoux, p. 640.

² Rochoux, op. cit., p. 639.

³ Observations on the Yellow Fever, with an Account of a New Mode of Treatment, &c., p. 10, &c.

when the irritation of the system at large, as well as of the gastro-intestinal mucous membranes is prominent, and the means calculated to reduce that irritation are called for—should be of the blandest, simplest, and least nourishing kind. Abstinence, indeed, in very acute cases, must be enjoined, or, if anything in the way of nourishment is allowed, it should consist of gum-water, rice, thin barley-water, apple-water, &c., while, in mild cases, when the inflammation does not run very high, arrowroot, sago, Indian or oatmeal gruel, or the like, may be allowed in small quantities, and at reasonable intervals.¹

That Savarésy and others should have prescribed, under the circumstances in question, a nourishing and stimulating diet, cannot be a matter of astonishment. Brunonians as they were, they could see in the disease nothing but the result of a debility, direct or indirect, of the vital forces. They discarded all idea of inflammatory excitement, and discovered no sign of gastro-enteric irritation. They could scarcely have been expected to starve their patients, while gorging them from the outset with tonics and stimulants.

In the early epidemics of this city, the feeding plan at first found advocates. But physicians, and Dr. Rush particularly, were not slow in discovering their error in that respect, and in adopting an opposite course. “The strict abstinence,” says Dr. R., “which I imposed upon my patients, did not escape obloquy; but, the benefits they derived from it, and the ill effects which arose in many cases from a contrary regimen, satisfied me that it was proper in every case in which it was prescribed” (iii. 155).

When the activity of the pulse has been reduced, and the skin softens and loses its excess of temperature; when the gastric irritation lessens also, and especially when the stage of metaplasia sets in with fair prospects of increasing amendment, the diet should be made more nourishing. Dr. Warren informs us that fearing, at that period, lest his patient should sink under the “expensive sweats” he was subjecting him to, he freely allowed him chicken-broth as often as desired, and found it “always useful and necessary thus to support Nature’s strength by a nourishment so safe, smooth, and digestible, and of such ready assimilation with the blood.” For the same reason, he allowed “now and then a glass of Canary or Madeira wine, a little diluted (p. 56), besides gruels and panadas, with wine added to them, sack-whey, made richer than ordinary, and such kinds of light refectory nourishment, which now prove of great emolument and service” (pp. 56, 57). Nor is Warren singular in prescribing such a diet; for rice, vermicella, or bread soups, boiled barley and panada mixed with wine and the like, besides wine, brandy and water, wine-whey, are not unfrequently mentioned as proper, under like circumstances, not only by writers strictly Brunonians, but by others who, though entertaining different sentiments respecting the nature of the disease, think it, nevertheless, necessary to support the system at this stage, and counteract any tendency to further prostration.

¹ Rochoux, p. 607; Bally, p. 567; Rush, iii. 155; Dariste, p. 195; Barnwell, p. 382; Hillary, p. 180; Hosack, iii. 440; Chisholm, i. 415.

But whatever be the signs of existing debility at this period, or the apprehension of greater prostration in the subsequent stage; and, however appropriate and called for articles of the kind may appear, it is perhaps safer and more advantageous to use animal food, whether in the form of soup or in substance, with great circumspection, or even to exclude it altogether, and restrict the patient to farinaceous and vegetable articles. Thin rice and bread cream, arrowroot, sago, Indian mush, oatmeal gruel, slightly sweetened and aromatized, thin panada, weak coffee, tea, or chocolate, milk and barley-water, answer well, when given in small quantities at a time, and at short intervals.¹ They are better suited to the existing condition of the digestive powers, which, as I need not repeat, are often considerably impaired. Under circumstances of the kind, animal soups, particularly when not carefully freed from fatty matter, so far from being properly digested and affording nutriment, only tend to awaken or produce gastric irritation. They frequently excite vomiting,² and either increase the violence of the disease, or retard the accession, or lengthen the duration of convalescence.

At the same time, wine and other spirituous liquors, except when the debility is unduly prominent, and all signs of inflammatory irritation have subsided, must be avoided as unnecessary and often hurtful.³ Nor is it less true, that considerable attention is required as to the quantity in which food is to be allowed—much mischief being often done by allowing a patient to indulge largely in even the most bland and proper articles. As a general rule, it is safer to give but a small portion of nourishment at a time, and to repeat it in larger quantities.

As febrile irritation and gastric irritability further subside, and the favourable crisis approaches, the food must be given at shorter intervals, and rendered more nourishing. It may now consist of the same articles prepared in a more generous manner, or of milk, stewed fruit, chicken or veal broth. When the debility is considerable, beef-tea, animal jelly, &c., may be allowed. This choice of aliments, graduated in the way mentioned, to the condition of the digestive powers and of the system at large, must be persevered in to the period of convalescence. It is equally well, if not better suited when the disease, instead of progressively subsiding without the occurrence of bad symptoms, passes to the third stage, or that of depression; for it is generally in vain to expect a recuperative effect from a tonic and analeptic regimen. Strong food is less apt to be digested than it would have been during the period of the remission, and more likely to irritate the stomach, excite vomiting, and—if not worse—retard recovery. When, however, the disease assumes a malignant character, and symptoms of positive prostration call for tonics and stimulants, nourishing food, in the form of strong broths, essence of beef, animal jellies, administered often and in small bulk, may be tried as long as the stomach remains quiet, and has sometimes proved advantageous.

¹ Rush, p. 155; Dariste, p. 212; Rochoux, p. 607; Hosack, iii. 440; Valentin, p. 222.

² Rochoux, p. 607; Dariste, p. 210; Bally, p. 567.

³ Barnwell, p. 383; Rush, iv. 28.

In congestive cases, little or nothing can be done in the way of alimentation so long as reaction has not taken place, the patient being then in no condition to profit by any food swallowed. When reaction has been brought about, the alimentation must be regulated in the manner already adverted to.

Treatment of Convalescence.—When the disease has been subdued by art, or the efforts of nature, the greatest care and circumspection, on the part of the medical attendant, are still necessary. The force of the morbid action has, it is true, been broken up, appetite and sleep have returned, convalescence is established, and the progress towards perfect recovery is, in inflammatory cases particularly, assured and rapid. But, in emerging from an attack of the disease, the functions of organic life most concerned in the recuperative process, though tending to a quick restoration, are as yet but imperfectly re-established. The susceptibility of the system to the impression of morbid agencies continues great, and the functions in question are consequently easily thrown into disorder. In fact, though an individual who recovers from an attack of yellow fever is generally restored rapidly to health, there are few diseases in which the stage of convalescence requires more cautious management. The very rapidity of the convalescence in most cases becomes, at times, a source of difficulty; for, instead of urging the assimilating functions to the performance of their duty, the physician finds himself often under the necessity of restraining them within just bounds.

Such being the case, great attention must be paid to what has been denominated the non-naturals during the progress of this stage, as any transgression in regard to them will almost surely be followed, if not by a relapse, at least by an accession of disease which is generally fatal.¹ Hence, while diminishing gradually the use of the remedies employed in the preceding stage, the patient must be allowed more and better food, but the transition to the diet of health must, as Dr. Blair properly remarks, be gradual (p. 112); care must be taken to select none but light and nutritious articles, and to avoid indulging in these frequently, and especially overloading the stomach. For some days, unless the debility be great, and all signs of gastric irritation completely subdued, the patient had better abstain from animal food, and limit himself to the use of eggs, bread, rice, bread and milk, panada, and oysters.² As strength returns, and the functions acquire energy, he may be allowed chicken, veal, mutton, or beef broths, prepared with a large proportion of rice, barley, and other vegetables. Poultry, game, and fish may follow; and finally, the more substantial meats may be allowed.

But necessary and indispensable as this gradual return to the diet of health undoubtedly is, the physician does not always find it possible to enforce compliance; for there is often such a sudden revival of the appetite for animal food at the commencement of convalescence that it is difficult to restrain the patient within proper bounds. Dr. Rush alludes to the case of

¹ Ralph, ii. 90; Wilson, p. 38; Blair, p. 112.

² Chisholm, i. 415, 416; Rush, iii. 155, iv. 27; Dariste, p. 211; Bally, p. 569; Hosack, iii. 440; Rochoux, pp. 608-9.

a young man who was upon the recovery, and died in consequence of supping upon beefsteaks ; and he states as his belief, that “many other instances of the mortality of this fever from a similar cause” occurred which were concealed from the attending physician (iv. 27).

The same distinguished physician (iii. 227), as also Dr. Caldwell (p. 115) and others, have regarded a nourishing and gently stimulating diet as preferable, at this period, to a tonic course. But, although this may be true as a general rule, cases not unfrequently occur, in which, from feebleness and languor of the stomach, or a state of general debility, mild, and even strong tonics and stimulants are called for. Under these circumstances, the infusion of bitter plants, of Peruvian bark, of serpentaria, or the sulphate of quinia, proves serviceable ; while malt liquors, wines—claret, sherry, Madeira, hock—and even brandy and water, find a successful application.¹

Dr. Dariste, whose practical skill in this disease was undisputed, while on the subject of these means, makes the remark, the correctness of which has often been verified in this and other places, that it is necessary to watch the effects of these articles with great attention, and to suspend their use on the first manifestation of gastric irritation, which is not unfrequently excited in those who have suffered much from vomiting. In such cases he advises tonic remedies to be administered by friction. “In this way, they impart strength without irritating the stomach—they promote perspiration, and aid in dispelling the jaundiced discoloration of the skin (p. 212). With the same view, frictions, either dry or with warm salt water, or aromatic substances, have been found useful” (*Ib.*, Bally, p. 569). At the same time that efforts are thus made to strengthen the system, care must be taken, while avoiding powerful cathartics, to insure a regular and daily action of the bowels.² On the other hand, diarrhœa must be suppressed.³

Hemorrhages occasionally occur during convalescence. At this period they are seldom, if ever, beneficial. Indicating, as they do, great debility of the system, and an atonic condition of the vessels, and tending to increase, by the loss of blood they occasion, the very cause on which they depend, they must be checked with the least possible delay. In these cases, acids internally, and nitrate of silver, creasote, or the tincture of iron externally, cool air, as well as remedies calculated to impart tone to the system at large, are employed with advantage. Dr. Ralph states that in one case of alarming hemorrhage from the fauces, an exposure of the patient to the sea air on the beach, checked it when every other remedy had been tried in vain (ii. 91). Quiet and cheerfulness of mind must be secured,⁴ and sleep must be encouraged.⁵ They are essential to a rapid and complete recovery. So long as debility is prominent, bodily exertions must be avoided ; but, as soon as the strength admits of it, exercise, alternating with rest, and graduated to the condition of the

¹ Bally, pp. 569, 570 ; Chisholm, i. 416 ; Rochoux, p. 655 ; Dariste, p. 212 ; Valentin, p. 222 ; Deveze, p. 276 ; Barnwell, p. 383.

² Ralph, *op. cit.*, ii. 90, 91 ; Deveze, p. 276 ; Bally, p. 570.

³ *Ibid.*

⁴ Wilson, p. 39.

⁵ Ralph, ii. 90.

patient—short, and frequently repeated—must be enjoined.¹ The venereal act—to which, as we have seen, convalescents are prone—should be carefully avoided, as always highly detrimental, and often, when indulged in to any excess, of fatal tendency. (*Dariste*, p. 213.)

Great stress must, at the same time, be laid on cleanliness and free ventilation, but more particularly on change of locality, and removal from the infected to a pure and cool atmosphere.² Dr. Chisholm says: “As long as the patient remained in the infected room or house, although all the symptoms of the disease had disappeared, the progress of his recovery was remarkably slow. Even the instant the patient was carried into the open air, he was sensible of a wonderful degree of refreshment; and the purer the atmosphere, and the higher the situation of the place—provided there was no dampness—the speedier was his acquisition of strength” (i. 406, 409).

When circumstances will permit, and convalescence is long and tedious, a sea voyage, and a residence in a cold climate, must be recommended.³ In the West Indies, it is sufficient to remove the convalescents from one island to another.

CHAPTER XXXVIII.

PROPHYLAXIS OF THE YELLOW FEVER.

It may be regarded as an axiom, the truth of which must appear evident to all, that the more dangerous a disease, and the greater its disposition to spread, the more necessary it becomes to discern the means calculated to prevent its development and diffusion, and to place individuals exposed, as much as possible beyond the reach of its baneful influence, or to lessen among them the virulence of its effects. From what has been said of the character and tendencies of the yellow fever, it is plain that it enters within the category of diseases of most serious import, and for the prevention of which every effort possible should be made. Hence, from an early period, attention has been directed to the subject. That, so far, the attempts made in regard to it, have not been crowned with entire success, is a fact which none can deny; but experience has shown, at the same time, that some of the means devised, though uncertain, in their results and often unsuccessful, have been sufficiently useful to deserve the attention of the medical inquirer; while much that has been done is founded on views of such doubtful correctness respecting the origin and mode of propagation of the disease, and is

¹ *Dariste*, p. 212; *Chisholm*, i. 416; *Wilson*, pp. 38–9; *R. Jackson*, ii. 223.

² *Deveze*, p. 276; *Blair*, p. 112; *Pugnet*, p. 375; *Dariste*, p. 212; *Bally*, p. 568; *Blair*, p. 112; *Wilson*, p. 38; *Rush*, p. 159; *R. Jackson*, ii. 206; *Chisholm*, i. 410, 411.

³ *Chisholm*, i. 417; *Savarésy*, p. 317; *R. Jackson*, ii. 224.

not only so perfectly useless, but so evidently calculated to lead to detrimental consequences, as to call for serious examination.

The prophylaxis of the yellow fever is divided into that which has reference to the community at large, and that which concerns each individual in particular. The first, which forms part of the domain of public hygiene, has for its object the consideration of police and municipal measures calculated to prevent the outbreak and arrest the diffusion of the disease in a given locality; while the second consists of the hygienic and other means required to shield individuals, placed within the sphere of the morbid influence of the efficient cause from an attack of the fever, or lessen its virulence and danger when they are once attacked.

Public Prophylaxis.—The means employed for this object may be divided into, 1, those resorted to in places where the fever has appeared at previous seasons, or where, from their geographical position, or the nature of their climate and localities, its occurrence is feared, for the purpose of guarding against its ingress or development; 2, those requisite to limit the extension, or altogether arrest the course of the fever, when it has already broken out; and 3, those necessary to prevent its recurrence when it has ceased to prevail. On each of these topics it is necessary to enter somewhat in detail.

Mode of Prevention.—It has been seen, from all that precedes respecting the usual mode of development, progression, and cessation of the disease, and from statements accumulated in the preceding chapters, that the yellow fever is the offspring of a special miasm resulting from the decomposition of vegetable or other substances, and not the product of a specific contagious poison transmissible from individual to individual, or by means of surrounding objects. Viewing the cause of the disease in this light, the conclusion is natural, that to attain the object in question it is indispensably necessary to effect the removal of all local nuisances, before the accession of hot weather—of everything likely to prove a source of infection or exhalation, and thereby to generate or aggravate the fever. Experience, indeed, has taught that such a course, which is advocated not by non-contagionists only, but also by some opponents of the doctrine of local origin, who hope by that means to lessen the activity of the contagion and prevent its localization, is the only true safeguard against the production of other diseases of kindred character.

The necessity of destroying local nuisances holds good, whether on land or on shipboard. In the former case, wharves, docks, or other localities liable to be visited by the disease, courts, yards, gutters, cellars, should be kept perfectly clean; all accumulations of filth and stagnant water should be carefully and thoroughly removed; streets, courts, and alleys should be paved, cleansed, and well watered, and privies should be emptied and purified. The crowding of vessels at the wharves or in the docks should be prohibited, and cargoes of perishable articles should not be allowed to remain there long; piles of wood or lumber should be forbidden on or near the wharves; vessels in a foul state should not be admitted, or quickly removed.

Nor should less care be paid to prevent accumulations of people in houses situated in narrow streets and alleys—especially near wharves or other localities where the fever has already prevailed, or is likely to prevail. Means must there be taken to insure proper cleanliness or ventilation, and to purify the atmosphere by the usual means employed for that purpose.¹

On board of ships, whether at sea or lying to in yellow fever regions, and during sickly seasons, the same preventive means are requisite. All sources of noxious effluvia must be guarded against; and if any exist, must be removed. The holds of such vessels must be kept clean and pure, and all foul bilge-water carefully pumped out. The decks must be washed and thoroughly dried—fumigated and properly ventilated, &c.

That such measures are often adequate to the preservation of vessels, even under unpromising circumstances at sea, or in port, may be inferred from the fact that, as regards the first, the fever has often been traced to the baneful influence of such sources of infection located in the timber, in the contents of the hold, or in the cargo; that other vessels, though situated close by those infected, but free from such sources of exhalation, have remained exempt from the complaint, and have arrived healthy in our and other ports, and moreover that, in other instances, the course of the disease has been arrested, and vessels restored to a healthy condition by a resort to measures of expurgation, whatever be the place whence such vessels came. Nor is it less true that the preservation of many localities on land has been often due to the same precautionary measures. Indeed, the fact of the disease being the offspring of local sources of impure exhalations, and, when it breaks out, of its prevailing most in situations where those exhalations exist, leads to the conclusion that the absence of the fever will, in great measure, depend on the absence or removal of materials to the decomposition of which the impurity of the atmosphere is due. To this cause may fairly be ascribed, if not wholly, at least in part, the freedom of many tropical places, or of some of our southern cities; while to the ameliorations which have taken place in regard to the medical police of, and the greater attention paid to the rules of public hygiene in, cities formerly more or less frequently visited by the disease, has been properly referred in part the exemption from it they have for some years enjoyed. After the dreadful epidemic of 1821, the port and streets of Barcelona, which, prior to that year, had been, according to M. Barcell,² in a discreditable state of impurity, were thoroughly expurgated. The same took place at Alicant, in 1804, and in neither of these cities has the disease again made its appearance.³

From the year 1820 to 1853, the city of Philadelphia, though experiencing in summer a thermometrical range as high as it had formerly, remained free from epidemic manifestations of the disease; and we are justified in attributing

¹ Academy of Medicine on Fever of 1798, p. 10; Letters on Fever of Baltimore in 1819, pp. 29, 30; S. Brown, p. 72; Seaman, p. 19; Currie, p. 13; Gilbert, p. 102; Rochoux, p. 659; Valentin, p. 224; Savarésy, p. 453; Report on Quarantine, pp. 5, 6; Dariste, p. 224; Caldwell, pp. 73, 76; Levacher, p. 94; Deveze, p. 290.

² Espargo y Desinfeccion di Barcelona, p. 177. See also Pierquin, p. 10.

³ Rochoux, p. 660.

much of this improvement to the favourable change that has taken place in the material and hygienic condition of the localities heretofore the seat of the pestilence. The wharves have been paved; the shipping is less crowded than it was in former days; the avenue along the river has been widened and straightened; the stores facing it have been reconstructed in a way more conducive to health; accumulations of filth are more carefully removed; the streets adjoining, and the courts and alleys there and in other parts of the city, have been paved; brick-kilns, tanneries, morocco, glue, and other manufactories of like kind, have, for the most part, been removed beyond the limits of the city proper; and about the docks, as everywhere else, greater attention is paid, at all times, but particularly at the approach of the sickly season, to cleanliness and the removal of nuisances.

Such has been the long interval of exemption enjoyed by the inhabitants of Philadelphia; and if in the last-mentioned year the disease once more made its appearance, and assumed, to some extent, the character of an epidemic, the result was evidently due to local causes, consequent on a neglect, in the places infected, of those hygienic measures to which I have referred. Let it be added that, compared with occurrences of a former date, the visitation of 1853 was of a trifling kind, and took place under the influence of an epidemic constitution of atmosphere of greater diffusion and malignancy than had been witnessed before. Let not the reader suppose that I ascribe the exemption in question to the above sanitary regulations alone, and that I view the improvements effected in the hygienic condition of yellow fever localities as having been carried as far as could be desired. Much, in regard to the former point, is doubtless attributable to modifications in the local features of those localities, to atmospherical changes, and to the absence of an epidemic constitution over which man has no control; while nowhere is the amelioration referred to so complete as to insure, without the concurrence of other agencies, entire security, under all circumstances, against a return of the disease. But, however insufficient these ameliorations may still be to prevent completely the outbreak of the fever, they must tend, when the latter appears, to lessen its wide diffusion and excessive virulence—effects fully appreciated by contingent contagionists, who, while opposing the doctrine of domestic origin, lay much stress on the necessity of improving the medical police of cities,¹ and the removal of impurities, which, according to them, are necessary for the germination of the specific virus and the spread of its morbid product.

But, though entertaining this opinion of the influence of an impure atmosphere, both the contingent and pure contagionists regard the yellow fever—the former generally, the latter always—as of exotic origin, tracing it to certain portions of tropical regions; holding that, when it occurs in temperate climates, it is the result of importation, through means of the sick, of wearing apparel, of articles of merchandise, or of vessels; and that even within the tropics, where it originates, it is in like manner carried about from one locality

¹ Hosack, *Med. Police Essays*, ii. 39, &c.

to another. To guard against these direful results, and inverting the proper course of proceeding in matters of legislation, by enacting laws without first ascertaining the real existence of the evils they are intended to remedy, they maintain that the only safe measure within our reach consists in an absolute interdiction of communication, direct or indirect, of the sick with an unaffected community; and, to effect this object, they advocate, and heretofore have succeeded in many places, in enforcing, a system of quarantine, applied as well to individuals as to vessels and cargoes, during a period sufficiently long to allow the vitality of the poison to be exhausted, and its germs to be weakened by diffusion and thereby rendered harmless, or to afford time to destroy them by artificial means.

The system of quarantine devised to guard against the introduction, and prevent the dissemination of the yellow fever at Marseilles, Genoa, Leghorn, Venice, and in various parts of Spain, is a literal copy of that in force against other diseases really or reputed contagious, and the counterpart of that established formerly in the old world against the oriental plague.¹ The necessity of the most stringent measures is loudly proclaimed by many European and some American physicians—Pariset, Bally, Hosack, Pym, Townsend, Strobel, Carpenter, Hume, Palloni, Fellowes, Currie, and Robert—and all the opponents of those measures are stigmatized as visionaries, theorists, or even worse. In our various States they were early prescribed. New York commenced so early as May, 1784, by “an act to prevent the bringing in and spreading of infectious distempers;” and the laws then enacted will be found scarcely inferior, in point of severity, to those of other countries. This act was confirmed, amended, and extended by others, passed March 27, 1794, April 1, 1796, February 10 and March 6,² 1797, March 30, 1801, April 2, 1803, February 28, 1804, March 9, 1805,³ and March 21, 1823.⁴ These laws were further revised at subsequent periods, particularly in 1836, 1839, 1842, and, finally, in 1846, when the system at present in force was adopted.

With a view effectually to attain the desired object, it is ordained that all vessels direct from any place where the yellow fever (and other diseases reported contagious) existed at the time of their departure, or which shall have arrived at any place and proceeded thence to New York, or on board of which, during the voyage, any case of such fever shall have occurred, arriving between the 31st of May and the 1st of October, shall be quarantined for at least thirty days after their arrival, and at least twenty days after their cargo shall have been discharged, and shall perform such further quarantine as the health officer shall prescribe. All such vessels, arriving between the 1st of April and the 1st of November, exclusive of time before specified; all vessels from a foreign

¹ Robert, p. 525.

² See “The Case of the Manufacturers of Soap and Candles in the City of New York stated and examined, to which are prefixed the Laws of the State of New York concerning Infectious Diseases,” pp. 7–23. New York, 1797.

³ Health Laws of New York, pp. 3, 27, 33, 37. New York, 1805.

⁴ An act to provide against infectious and pestilential diseases, passed 21st March, 1823. New York.

port, on board of which, during the voyage or while at the port of departure, any person shall have been sick, or from any place in the ordinary passage from which they pass south of Cape Henlopen, arriving between the 31st of May and the 16th of October; and all vessels from any place (including islands) in Asia, Africa, or the Mediterranean, or from any of the West India, Bahama, Bermuda, or Western Islands, or from any place in America from which they pass south of Georgia, arriving between the 1st of April and the 1st of November, shall be subject to such quarantine and other regulations as the health officer shall prescribe.

While this is done in respect to such vessels, the health officer has power to cause their cargoes to be discharged at the quarantine-ground, or some other suitable place out of the city, for the purpose of purification; to cause the vessels, cargoes, bedding, and clothing to be ventilated, cleansed, and purified; and, if he shall judge it necessary to prevent contagion, to destroy anything on board deemed incapable of purification; to prohibit and prevent all persons arriving in vessels subject to quarantine from leaving quarantine until fifteen days after the sailing of their vessel from her port of departure, and fifteen days after the last case of fever that shall have occurred on board, and ten days after her arrival at quarantine, unless sooner discharged by him; and to permit (with the sanction of the mayor and the commissioners of health) the cargo of any vessel under quarantine, or any portion thereof, whenever he shall judge the same free from infection and contagion, to be conveyed to the city of New York, or such place therein as he may designate.¹

As regards Philadelphia, the first essays at establishing precautionary laws of the kind mentioned commenced early, the first enactment of a law for the protection of Philadelphia from *contagious* diseases dating as far back as the 12th William III., 1700. It provided that no unhealthy vessels, coming from an unhealthy or sickly port, should come nearer than one mile to any port within the jurisdiction of the then colony of Pennsylvania, with a foul bill of health, or land any person without permission. An act of 16th George, 1742, authorized the purchase of *Province Island*, and the erection of a lazaretto thereon.² In 1774, the Colonial Assembly passed an act embracing several new regulations. But these were annulled in 1803, when a board of health (under that title) was created; and in 1818 it was modified. Since then, those laws have been several times amended; but though by these amendments the system has become less stringent than it was in the origin, passengers and crews from sickly ports or places where the fever is supposed to originate or exist are still detained a certain length of time. Vessels are kept a long while at a distance from the city, especially when they have had sickness on board, or have arrived from a sickly port, and are cleansed, fumigated, and otherwise purified.

Nor is this all. Not content with interdicting intercourse with sickly

¹ An act concerning quarantine, or regulations in the nature of quarantine, at the port of New York, Report., pp. 51-53.

² See Amer. Journ., i. 169, 170.

ports, or vessels coming from distant parts, quarantines equally stringent are established, here and in other cities, to guard against the introduction of the disease when it prevails in neighbouring places. What is done here and in New York, is done in most other cities of this country—even in those where the fever often prevails and undeniably originates—and when, perchance, any of these quarantines are abolished from a conviction of their inutility, they are almost sure to be re-established on the occurrence of a severe epidemic.

After what has been said in former chapters relative to the mode of origin and propagation of the yellow fever, I need scarcely say that quarantines, as at present organized, can meet with no favour among individuals well versed in those matters. They are based on the assumption of the contagiousness of the disease, or on an hypothesis, equally groundless—the transmissibility of self-propagating germs—and hence may have suited, at the origin of their establishment, when the belief in the spread of this, as of other kindred disease, through means of contagion, was almost universal among well-informed professional men. But, as I have shown, sentiments have since somewhat changed; and it must be admitted, that against a disease not endowed with such properties, quarantine regulations can be of little avail, whether applied to goods, clothes, or the sick, and especially to individuals in health.

Admitting even, for the sake of argument, that the fever of this country or Europe were endowed with the property in question—which, of course, I am not disposed to do—there can be no doubt, from the almost unanimous testimony of the most experienced authorities, that such is not the case in tropical climates; and how a disease, not contagious in one region, can be transported elsewhere, is a question of medical philosophy which few will be disposed to entertain seriously. In no case, not even the one so much talked of, of the *Eclair* at Boa Vista, has the introduction of the disease from abroad been made out. In none, therefore, could quarantines have prevented its advent; while, during every period of exemption, quarantines are so frequently violated, and so imperfectly enforced, as to render the utility ascribed to them more than problematical; inasmuch as, if the fever could be introduced from abroad, and if, when it occurs, it is the effect of importation, it would, from the repetition of the infractions in question, show itself oftener than has hitherto been the case.

But quarantines are open to more and serious objections. That they are the source of very great inconvenience to individuals, arriving in health or disease—obliged, as they are, to remain on board or in uncomfortable quarters—has, and can, never be denied. Nor is it possible to controvert the fact that, through their instrumentality, great, and often irreparable losses, in a commercial point of view, are sustained. This applies to communities which, in their endeavours to protect themselves by shutting out vessels arriving from infected places, or tropical latitudes—but more particularly to those which, being infected, or likely to be so, are excluded from all communication with other ports. Retaliation on the part of other states or cities takes

place; or the example of the one is followed by others. "All are now aware," says an intelligent English writer, "of the value of speed in the transit of goods and persons by sea as well as by land. By the amount of those commonly recognized advantages may be estimated the disadvantages and losses to which the consumers generally, and merchants frequently, are subjected by the interruptions of the commerce of this country, and that of Europe, by quarantine. It frequently occurs in this country, that the expenses of the twenty or thirty days' detention of a cargo are equivalent to the expense of the whole voyage home. It is estimated that the expense on cotton goods is fifteen per cent."¹

Sir W. Pym, as superintendent of quarantines in England, objected to the immediate landing of the sick from the *Eclair*. Scarcely three weeks after this occurrence, the Board of Health of Naples, after full deliberation on that unfortunate case, decided that vessels arriving at Naples from any part of the coast of England between Portland and Dover, would be refused admittance; and imposed on vessels from all other parts of England a quarantine of twenty-one days. Nor was this all. Malta and Gibraltar were put under partial quarantine at the same time, in consequence of their communication with England. These regulations were continued in force for upwards of six weeks after the arrival of the *Eclair* in England, to the serious detriment alike of the commerce of Naples and England (*Report*, p. 83). And yet the *Eclair* had not communicated the disease to a living soul in England, except the luckless individuals who had been on board; and they, when on shore, gave it to no one.

Quarantines, moreover, are mischievous, and often dangerous, in their tendencies—more so, as conducted in some countries than others; but always sufficiently so to make them a subject of more than distrust. They substitute false for real securities; for, while aiming at effecting that which cannot be obtained, measures proper to prevent the outbreak of the disease are often, and in many places generally, neglected. True it is, some advocates of contagion enjoin attention to measures of cleanliness and purification; but, as is well remarked by a judicious writer, "they take away the most powerful motive for watchfulness when they declare that there is no danger of the appearance of the disease so long as the avenues of its importation are effectually closed."² And let it be remembered, that the number of places where attention is paid to these hygienic measures is but limited; that it not unfrequently happens that, while quarantines are strictly enforced, those measures are little thought of, and that the danger of importation, in the only mode it can take place, is left unguarded against, for the simple reason that no one sick with the fever, and capable, as it is thought, of introducing the disease, has been found on board or has died during the passage; or because the vessel has not sailed from an infected port. Great suffering, and even the loss of life, have resulted from the manner in which quarantines are in some places enforced.

¹ Report on Quarantine, p. 76. London, 1849.

² North American Review, x. 411.

The case of the *Eclair* is in point. On arriving at Portsmouth, her sick were prevented from being landed at the hospital. They, as well as the crew, were kept on board for some days, with the effect of aggravating the disease—hastening the death of the former, and of promoting the spread of the fever among the latter. The regulations enforced in Spain, in Italy, and, until lately, in France, as set forth in Fellowes, Maclean, Arejula, Berthe, Robert, and others, could not help being equally detrimental. The mind revolts at the idea of the sufferings of the inhabitants of an infected city or districts, while under the restraints of quarantine laws such as existed very generally in former days, and such as exist now in some places. Forced to remain in the focus of infection, they continue exposed to the effects of the morbid agent; the dread of sharing the fate of those around them increases their liability to an attack; the disease there assumes its more malignant character, and not only does the number of sick increase, but the mortality among them increases tenfold; and while this takes place, the whole incarcerated population is liable to experience privations which tend to enhance their susceptibility to morbid impressions, and aggravate their sufferings.

The dreadful loss of life in Spain no doubt was owing to the system of seclusion there adopted. They are the source of much expense to the community. "Millions of dollars have been wasted by them;" and these could have been applied to better purpose. "Distressing as these evils are," says Dr. Rush, "still greater have originated from them; for a belief in the contagious nature of yellow fever, which is solemnly enforced by the execution of quarantine laws, has demoralized our citizens. It has, in many instances, extinguished friendship, annihilated religion, and violated the sacraments of nature, by resisting even the loud and vehement cries of filial and parental blood."¹ They beget cruelty; for, to apply them rigorously to the full extent that would be necessary to insure success—supposing them of real utility—severe and harsh treatment must often be resorted to.

Even in this country, where a more humane but less consistent system is pursued, the hampering of individuals in the narrow space of an often impure vessel, or the compelling them to put up with bad accommodations on shore, away from friends, and deprived of means of diversion, and under the belief of their carrying about them, or in their effects, and apparel, the seeds of a contagious disease which in a moment may attack them, cannot fail to act in an injurious manner (*Dariste*, p. 223). Even if it were true, that the yellow fever is a contagious disease, quarantine, as established, could be of little use. We have seen that, with the exception of a very few cases, the incubative process is not of more than about fourteen days. Some of the hottest contagionists make it out much shorter—one, two, three, or four days. If so, the disease, if imbibed in an infected part of the West Indies or this country, must, as a general rule, develop itself before the individual can reach Europe, and hence, when such individuals arrive in health, it

¹ Rush, iv. 138; Means of Preventing Summer and Autumnal Diseases.

is scarcely to be feared that they will sicken after landing. Quarantines, therefore, so far as they are concerned, are of no use. To a certain extent, this applies also to individuals arriving in this city from the West Indies, and other southern ports; for though the passage is often shorter than fourteen days, it is sometimes longer. At worst, those arriving before fourteen days, ought not to be detained any longer than is necessary to complete that number of days; while those who arrive *after*, are not dangerous to the community.

In view of such inconveniences, pecuniary injuries, and personal dangers—to say nothing of the strong objections to which quarantines are open on the score of the non-contagiousness of the disease—nothing but an absolute evidence of their necessity and efficiency could justify their adoption and rigorous enforcement; and, on the other hand, if the public health were secured by such an enforcement, no commercial advantages that could result from the omission or abrogation of measures of seclusion should prevent their adoption. But have circumstances established beyond doubt that necessity and success? So far from this, experience has abundantly proved the uselessness of the measures in question, and, as Maclean long ago said, they have no quality or qualities capable of counterbalancing, in any degree, the enormous mischiefs which they occasion to society. The disease has spared many localities during a series of years, notwithstanding the absence or neglect of quarantine regulations. Some commercial cities in which no quarantines have existed, or where they have been so slight as to be merely nominal, have been as strikingly exempt as others where they were enforced most rigorously—while the localities which were at one time free, have been, at other periods, visited even frequently with the fever in the teeth of the most stringent system of seclusion.¹

When the yellow fever broke out in New York, in 1791, the city had been exempt from it during a space of forty years; and as the first quarantine laws were enacted in 1784, the inhabitants, though unprotected, were free from the pestilence for the long space of thirty years; while, under the protection of such laws, and others of equal potency, it was visited at the period mentioned, as also in 1798, 1805, 1819, and 1822.

Similar results have obtained in Philadelphia. The history of our early epidemics shows that whatever may have been the quarantine laws enacted prior to 1793, they could not, from their restricted character and imperfect administration, prove serviceable for the object in view. And yet, from 1699 to 1741, from 1747 to 1762, and from the latter year to 1793, the city remained exempt from epidemic visitations of the disease, although the intercourse with the West Indies, during the greater part of that time, remained uninterrupted; whereas, from 1793 to 1820, inclusive, the fever raged with more or less violence eight several times, though the regulations then in force were of a stringent character. “Why,” says Dr. Rush, “was not the fever imported oftener before the year 1791? It is seldom

¹ Caldwell's Oration, p. 19.

absent from the West Indies. It raged in most of them during the two wars previous to the present, and yet but one instance occurred of it in the United States in those two periods of seven years each, and that was in Philadelphia in the year 1762. The intercourse between our cities and the islands during that time was extensive and constant, particularly in the war between the years 1756 and 1763. Quarantine laws then existed in but few of our cities, and where they did they were feebly executed, or eluded every day."

Dr. Ferguson, writing a few years after, says that at Martinique, where a strict quarantine, particularly against Guadaloupe, was established, "they have been consumed with yellow fever; but at Dominica, Tobago, St. Vincent's, where were established none at all, they have not had, as far as I could learn, a single case."¹ Dariste informs us that during nine years, 1806–1816, no yellow fever occurred at Martinique, though the island was not protected by quarantine regulations; and yet, during the whole of that time, the number of Europeans and North Americans was very large. In 1816, the government having been informed that the disease prevailed extensively in the city of Point à Pitre (Guadaloupe), bethought itself to establish a quarantine. The most stringent measures were devised, and carefully enforced; nevertheless, the fever broke out and raged extensively (p. 22).

The sanitary restrictions established at Tortosa, in 1821, were remarkable for their rigour, and for having been imposed a considerable time not only before the apprehension of contagion was officially proclaimed by the Junta of Barcelona, but before any suspected deaths or sickness had taken place at Tortosa itself; yet no place has ever been more afflicted than that unfortunate city.² In 1805, New Haven, Providence, and Philadelphia, had the disease as soon as, or sooner, than New York. The communication between those places and the latter was open, and the access easy, and yet no one appeared to have thought of tracing it to those places—all looked to the quarantine ground. Quarantines were in force to prevent the introduction of the contagion by sea, but were not attended to on the land side.³ All masters of vessels, passengers or seamen, were at that time landed from the quarantine ground at Whitehall, and the sick, who were sent to the Marine Hospital, embarked there, and necessarily came in contact with the people around.⁴

After the epidemic of Baltimore in 1819, the health authorities, at the suggestion of the District Medical Society, modified the regulations then existing, and deprived them of their pristine severity against passengers and crews, who, in the language of the Society, "should not be deprived of the privilege of free communication with the city,"⁵ and yet no fever of any account has since occurred in that city, and such cases as have appeared were clearly

¹ London Medical Gazette, p. 22.

² Maclean, *Evil of Quarantines*, p. 162.

³ Rodgers, *Documents relating to Board of Health*, p. 18.

⁴ *Ib.*, p. 21.

⁵ Letters, &c., addressed to the Mayor on the Origin, &c., of the Epidemic of 1819, p. 186.

proved to have arisen from local sources of infection. That this was not the result of non-exposure to the pretended contagion, is fully shown, not only by the fact that such crews and passengers arrive as frequently now as they did then, but by the occurrences of the next year. "During the existence of the yellow fever in Philadelphia and New Orleans in the summer of 1820, the Board of Health of this city did not interrupt the intercourse with either. With the former, there was a daily communication without any injury resulting therefrom."¹

Dr. Fenner, whose recent exhibition of proclivity towards the contagionism of the yellow fever, cannot but be a matter of regret to many of his friends, and who has conclusively proved the local origin of the disease on several occasions in New Orleans, remarks on the subject more particularly before us: "As for the old delusion, that yellow fever is brought from the West Indies, Vera Cruz, or any other place, and might be kept away by quarantine, I need only say that the experiment has long since been fairly tried, and it signally failed."² Even while the present sheet is passing through the press, we learn that in the city of New Orleans, where stringent quarantine laws have once more been established and enforced, the disease has broken out, and prevails epidemically.

It is not to be forgotten, in connection with the subject, that the yellow fever has, on some occasions, as in this city in 1819, broken out at a period when, from the complete absence of any arrival from the West Indies, or other parts within the yellow fever zone, the disease could not be thought to have reached us from abroad. From such facts we may infer that, in other instances, when arrivals take place, or the health laws are violated, the appearance of the disease is not necessarily the result of such arrivals or violations, and hence the advocates of quarantine must admit that, in some instances, at least, we might get along just as well without as with their favourite system.

Nor is it less certain that the application of quarantine is often impolitic—generally ridiculous. Intent on shutting out a contagious germ or virus, importationists overlook circumstances on which the existence and extension of the disease really depend. Take, for example, the health laws of Boston, as they existed some years ago: "If a vessel arrive during the summer months, between the 20th of May and the 20th of October, from one of the West India Islands after a passage of less than twenty-two days, coming from a healthy port, with a crew in fine health, and a cargo in a fresh and sound state, she is detained at quarantine until twenty-five days are completed from the time of her sailing. Her cargo, in the mean time, if composed of fruits, or other perishable articles, is suffering from the detention, and she leaves the quarantine ground in a much worse state than she entered it. But suppose this vessel, instead of being carried forward by refreshing and invigorating

¹ P. Reigard, Sec. to the Board of Health of Baltimore, Letter to Chervin, *Opinion des Med. Am.*, p. 128.

² *Trans. of Assoc.*, 1849, p. 625.

breezes, to have been retarded by calms and fogs, until twenty-three days have expired, or any longer time; her cargo injured by the length of time and the badness of the weather, so as to be in a putrid state, and she will be detained but three days at quarantine before she is permitted to discharge her cargo in any part of the town. The circumstances which should operate as a motive for additional precautions is the reason for diminishing them, and that which is the best ground of safety is made a reason for increasing them."¹

These laws, as established with us and in other places, clash with the very principles upon which they are founded, and, in their execution, are likely to prevent the attainment of the object in view. Thus, the medical officer appointed to examine vessels reaching the quarantine station, is continually liable to visit one arriving from a sickly port, and containing the germs of the disease, or individuals actually labouring under it, and immediately after, to examine other vessels differently circumstanced and coming from northern ports, and which, being in good condition, are allowed to proceed to the city. Again, health officers or commissioners are sent down to the Lazaretto to inspect the whole establishment—infected vessels, sick, and all; aiding, perhaps, in autopsic examinations; and in the evening return to the city to mingle with its inhabitants.²

It is evident, that if the disease were sufficiently contagious to be communicated by the sick, by goods, and articles of clothing exposed to the miasm, giving rise to it, &c., and if a person in health arriving from a sickly port, must, for this reason, be shut out by measures of sequestration, the virus would be likely to be imbibed by the visiting physician during his minute survey of the sickly ship; by him transmitted to the other vessels, and by them to the city. The operation of quarantines at Natchez may here be referred to. They were established in that city to preserve it from the fever then prevailing at New Orleans. But they were soon evaded. Passengers on steamboats landed some miles below, and entered the city by land; while the cargoes were landed on the opposite side of the river and were soon after conveyed over in ferry and other boats.³

¹ North American Review, x. 413.

² Mitchell, T., Remarks on the Quarantine System of the State of Pennsylvania, and Med. Rep., Sept. 1818, p. 356.

³ At Barcelona, in 1821, the cordon was a matter of traffic, as the following statement will show:—

“At a village called Orta, through which it passed, there was a house at once—*sucio y limpia*—foul and clean; the foul part being supposed to be within, and the clean part to be without, the cordon; but, of course, having free communication with one another. At this house resided the medical superintendent of the cordon, who did not neglect to reap a famous harvest. Passports for Barcelona for the day were currently given by the *alcaldes* of the villages without the cordon; and the bearers of them went freely to that city, where they spent the day, and returned in the evening to the outside, to which they might have conveyed pounds of contagion if they could have procured it. In short, it was in derision called by the people the half *picetta* cordon, half a frank being the current price of liberty of infraction. Mr. C—y, an English gentleman, assured me, that

In saying thus much on the subject of the objections to which quarantines are open, it is very far from my desire to maintain the propriety of dispensing altogether with precautionary measures. That vessels and their cargoes are at times themselves sources of infection, is a fact already adverted to, and of which I have offered many illustrations, some of which are derived from the history of the fever in our own city. Nor is it less true that such instances are more frequently encountered in vessels engaged in the West India and southern trades, owing both to the greater injury experienced there by the timber, and to the perishable nature of the cargoes. But nothing proves that the infection on board, though such vessels may arrive from sickly ports, depends on the presence of a contagious miasm received there. It is due to the emanations formed on board, from the decomposition of the materials present.

Such vessels—all those which, from the foul condition of their decks, holds, or cargoes, become objects of suspicion—should be debarred admittance in port so long as that condition endures. They must be cleansed, ventilated, and purified by every available means—which may be effected sometimes in a comparatively short time; their cargoes, if necessary to insure perfect security, must be discharged;¹ it may even be proper, as an additional, but not absolutely required, precaution, to wash and air the bedding and clothing of the passengers and crew. All this is perhaps more generally and effectually done at this than at other ports, in accordance with a rule adopted in 1820, at the instigation, I believe, of Dr. Jackson, then President of the Board of Health, *i. e.* that no vessel, from any port, shall be allowed to come to the city until she has been washed out completely—until all the bilge-water has been removed, and water pumped in and out till it return pure from the hold of the vessel; an operation which commences as soon as the vessel receives her pilot, and through means of which she is made sweet before reaching the lazaretto.

But, while all this is carefully attended to, no restrictive means should be used, so far as the individuals on board are concerned. The very sick or convalescents, if such exist, may be landed and received in hospitals or private houses with perfect impunity; for if the disease cannot, as we have seen, be propagated within a few rods of an infected district, in times of epidemics, it is not to be expected that a different result will accrue, during healthy seasons, from the presence of a few sufferers discharged from a vessel. In other words, quarantines, to be applied to their legitimate ends, must, as regards yellow fever, have for their object the exclusion of filth and deleterious miasmata, not of contagion; and their means of defence must be directed against impure vessels and putrid cargoes, not against infected goods and distempered

for one dollar he received a certificate of his having resided twenty days beyond the cordon, although he had been only one day from Barcelona.”*

¹ Caldwell (1805), p. 73; Essay, p. 151; Deveze, p. 301; Dariste, p. 226; N. A. R., x. 414; Rush, iv. 138; Pascalis, v. 254.

* Maclean, pp. 171, 240; Rochoux, p. 665.

individuals.¹ They should not, therefore, be regulated in reference to the ports from which vessels arrive, but to the state in which they reach a healthy spot. In the words of a sensible American writer: "There is no cause for detaining a ship, on account of the danger of yellow fever, which is itself in a pure and healthful state, from whatever port she may have sailed, nor however sickly that place may have been. On the other hand, no ship that is foul and offensive, or that has a cargo in a putrid state, although the place from which she sailed, or the persons on board, be ever so free from sickness, ought to be permitted to approach the town until she has been thoroughly cleansed and purified."²

For reasons similar to those adduced in reference to the passengers and crews of vessels arriving from sickly ports, as well as to untainted cargoes, whencesoever they may come, we must adopt conclusions of like nature as to the uselessness of quarantines established against individuals arriving by land from infected places.

To medical readers, who have kept pace with the state of professional opinion respecting quarantine regulations, it need not be remarked that a considerable change has taken place, within even a few years, on the subject of their efficacy in regard to this, and indeed to some other diseases. In a preceding chapter, attention was called to the almost universal abandonment of the belief in the spread of epidemics by contagion, among those who have had an opportunity of investigating the yellow fever, and have examined the subject with a clear understanding of all the facts, with unbiased judgment, and in a philosophical spirit. Concurrently with this belief must be that respecting the utility of sanitary measures intended to prevent the importation of the disease from infected places.

Nor has this change been less apparent, at times, among non-professional persons engaged in commercial and public life. "The Spanish Cortes, in 1822, by a considerable majority, in direct opposition to the unanimous opinion of all the physicians who were members of their own body, being nine in number, as well as to the general belief of the physicians of Spain, rejected the project of a code of sanitary laws founded on contagion, which had been prepared by three successive committees or commissions of public health."³ This code had reference principally to the yellow fever. In England, unless a vessel arrives with the yellow fever on board, as was the case with the *Eclair*, individuals reaching there from the West Indies and this country, whatever be the condition of the ports whence they sail, are not (nor their effects) quarantined. And yet no bad effects have ever resulted from the practice. Cotton is never purified; and though nearly a million of bales are annually introduced there from our southern ports, no case of infection has ever occurred. In the report of the committee on quarantine regulations, read to the annual general meeting held on the 4th of May, 1846—George Berry, Esq., chairman—quarantine regulations were declared to be unnecessary as regards Great Britain.⁴

¹ Caldwell, Essay, p. 151.

² N. A. R., x. 414.

³ Rep., p. 20.

⁴ Edinburgh Journal, lxvi. 515.

In France, in 1828, the formation of sanitary establishments, projected by a law passed on the 3d of March, 1822, and the object of which was to shield the country from the introduction of the yellow fever, was forcibly abandoned, by the refusal, on the part of the legislature, of funds necessary to carry it into operation; this refusal being the effect of the unanimous opinion expressed on the subject by the committee of the Academy, appointed to report on documents submitted by Dr. Chervin to government, and by it referred to the Academy. In 1835, the government exempted from quarantine all vessels having clean bills; and cotton from America was admitted without landing at lazarettos, and without being opened or purified. In 1839, the system of suspicious bills (*patente suspecte*) was abolished, and quarantine imposed on vessels having foul bills was considerably reduced—*i. e.* from twenty to thirty, it was fixed at from five to fifteen days in the seaports of the Atlantic and in the Channel, and from ten to twenty in those of the Mediterranean.¹ Finally, after years of opposition on the part of the government, and to satisfy public opinion—which there was guided by professional men, headed by one whose name will pass to posterity, Dr. Chervin—the sanitary laws relative to the disease have been abrogated;² and while in some of our cities, as New York, quarantine laws of a strong character are still in force; and while, in New Orleans and Charleston, a few physicians, backed by a portion of the public—an unsafe guide in such matters—have revived a strong feeling in favour of the re-esta-

¹ Chervin, Rep. in Rufz's Mem., pp. 108, 109.

² It argues not a little in favour of those who believe in the inutility of quarantine regulations relative to yellow fever, and the absence of all proof of contagion derived from their results, that in France, where they were long maintained at great cost and sacrifice; where, during many years, the subject of contagion and non-contagion was discussed with unparalleled perseverance, and, at times, with acrimony; and where government evinced the greatest reluctance to admit the propriety of a reform in the sanitary laws, enacted under the influence of ultra contagionists—to say nothing of motives of interest—a complete revolution, as regards quarantines, has lately been effected. The medical reader can scarcely be ignorant of the unprecedented efforts made by the late Dr. Chervin to illustrate the necessity of a change of policy in regard to yellow fever, and of the truly gigantic investigations and numerous remarkable publications of that eminent physician on the mode of propagation of the disease. To those efforts, and the facts and experiments he adduced, several modifications adopted during his life were due; but he died too soon to witness the triumph of the cause for which he had so long combated. "On Monday, the 14th of April, 1845 (two years after the death of Dr. C.), the superior Council of Health assembled, under the presidency of the minister of commerce. It was well known at this meeting that the modification required in the quarantine laws was to be made a subject of discussion; but no one entertained an idea of the extent—I had almost said the radicalism—of those reforms, and, what is more extraordinary, it is the government itself which was to take the initiative in those propositions. The minister, after having shown, in a short introduction, the urgent necessity and legitimacy of those reforms, and, commencing with our relations with the Antilles, did not hesitate to declare that science had arrived at a definite conclusion, and that it was no longer allowable to believe in the contagious character of the yellow fever! The question of the suppression of quarantines, so far as regards the yellow fever, being now put, was carried unanimously!"*

* Life of Chervin, by Dubois (d'Amiens), Mem. of the Acad. of Med., xii. 57.

ishment of quarantine regulations, and obtained the enactment of a stringent system—in some places they have never been established, and enumerate but few advocates; in others, they have been abolished more or less completely as regards passengers and crews, as also vessels and cargoes in a pure state; and in others, again, they have been greatly modified.

CHAPTER XXXIX.

PROPHYLAXIS OF YELLOW FEVER—CONTINUED.

Means to Limit or Arrest the Spread of the Disease.—The next object to which attention must be called, is the method best calculated to limit or arrest the progress of the disease, or mitigate its malignancy, when once it has made its appearance in any locality on land or on shipboard. To attain this object, many and diversified means have been tried in the United States and elsewhere, according to the various views entertained respecting the nature and mode of propagation of the disease. By those who believe in its contagious character and transportability, the principal object is, or formerly was, to separate the sick from the rest of the population.¹ What took place here will suffice as an example. In August (26), 1793, the College of Physicians issued a set of “directions for preventing the further progress of malignant contagious fevers,” and therein recommended that all unnecessary intercourse should be avoided with such persons as were infected by it; and that a mark be placed upon the door or window of such houses as had any infected persons in them. It is evident from this that if the sick were left in

¹ The means of prevention proposed by Dr. Currie, in 1794, will serve to show the views entertained at that time by our most prominent contagionists in relation to this matter.

1. Prohibit all intercourse between the sound and the infected, and commit the diseased to the care of persons commissioned for that purpose. 2. Remove both sound and sick from the house visited to other apartments at some distance from the town, and at a considerable distance from each other. 3. Purify the house where the disease has appeared. 4. Infirmaries to be built on dry and elevated situations, particularly on the high and gravelly banks of rivers, or constant streams of water, open on all sides to the free access of the air, and provided with every necessary. 5. The affluent to be allowed to retire to private infirmaries, provided they conform to the rules of public establishments of the kind. 6. Every family in the neighbourhood where the infection prevailed, should also be obliged, under a heavy penalty, to remove to another part, till all danger from contagion has ceased. 7. No person to return to an infected house, or to use any article where the disease existed, till these have been purified. 8. No one who has recovered to return to the city or to his own house, till he has (after recovery) repeatedly undergone ablution of the warm and cold bath, changed every article of his clothing, and performed a quarantine of at least fourteen days.*

* Currie, Fever of 1793, pp. 75–77.

the houses where they had taken the disease, and as every one else who chose to remain with them, or in adjoining houses, could do so, they were, as a matter of course, as liable to sicken as those already affected.

On the 17th of August, 1797, the same body issued, in the form of a letter to the Governor of Pennsylvania, a second set of directions. Here, again, the advice is given to shun the sick. "Let all unnecessary intercourse be suspended with that part of the city where the disease first appeared; but such of the sick and families who reside in any part of the city where their residence may prove hurtful or dangerous, be immediately removed; when the disease is ascertained to exist in several houses near to each other in any part of the city, let all the neighbouring families who have escaped infection be removed, and all communication between the infected families and the city be suspended by preventing any person, except those whose visits are essentially necessary to the sick, previous to this removal, from entering into that part of the town; for this purpose mercantile business must of course be suspended there, and the vessels removed from the adjoining wharves." Again, on the 27th of the same month, the College recommended that those affected with the disease should be removed from town, *when they were willing*, as well as their families, or those who had visited the sick. When this was not practicable, it was advised to remove the neighbours; again, in a letter addressed to the governor, Nov. 1795, the College, finding that their directions were disregarded, recommended that the Board of Health should be invested, concurrently with the governor, with power to cut off intercourse with infected persons and places.¹ Finally, on the 28th of June, 1799, on the breaking out of the fever, the College advised the removal of all vessels, as well as the inhabitants, from the infected parts, and the prevention of intercourse between the inhabitants of the infected parts and those that were still healthy (*Currie*, 1799, p. 10). In all these instances, the directions mentioned, which, as far as existing laws allowed, were carried into effect, or earnestly recommended by the city and health authorities, and facilitated by the erection of tents, &c., in the country for the reception of the poor (*Currie* and *Folwell*), were accompanied with others relative to the necessity of cleansing and purifying the rooms, clothing, and effects of those who had recovered or died.

In other cities of the United States, kindred regulations have been enjoined. In Spain, a similar and even more severe system of sequestration was put into operation, and enforced to the fullest extent; and in writings of influence on the disease, we find it recommended with great earnestness.² The effects may be foreseen. In our cities, the infected districts being considered safe, provided the sick were avoided, continued to be visited, and the disease naturally extended among those who penetrated into them; a large number refusing, or being unable to leave them, were exposed to the infection, sickened, and died—while the sick, whether they remained in the district, or, having taken the disease there, removed elsewhere, were often abandoned and

¹ Facts and Observations by College of Physicians, p. 12.

² Bally, p. 577.

shunned by friends, nurses, and physicians, thus giving rise to all the ill effects already noticed while on the subject of quarantines.¹

Much more useful will it be to obtain at once the complete evacuation—by advice if possible, by force if necessary—of all the inhabitants of infected spots, if their limits are well defined, as is the case in most northern cities; providing, at the same time, accommodations for the poor in proper and healthy situations, allowing others to seek lodgings where they please, in the rest of the city or in the country—and affording to those who wish, or are obliged to avail themselves of it, proper and efficient medical aid in convenient and well-appointed hospitals; and, as soon as the sickly spot is emptied, to barricade the streets leading to it, and prevent access to it until after the occurrence of frost to all but individuals whose presence is there absolutely required. In places where the residents are exempt from the disease by acclimatization—as in the West Indies, and in our southern States—it will be sufficient to remove and exclude strangers and children from the infected districts.

This plan of arresting the progress of the disease was long ago suggested by Dr. Meade² in reference to the plague, and was even before his day practised with success by the conquerors and early settlers of America.³ In its application to the yellow fever, the plan may be said to be strictly American. It differs materially from the one suggested by some writers, who, though advocating the emigration of the inhabitants of infected localities, leave it optional with them to remove or not—forbid their being received in neighbouring villages—only permit them to pass the barrier after their undergoing a complete purification, and some days of probation—compel all the sick to be sent to hospitals situated out of town—recommend the enforcement of isolation of the sick—and to these directions add others predicated on the doctrine of contagion, and intended to prevent all communication between the sick and the well.⁴

In 1798, the Academy of Medicine of Philadelphia, among other recommendations to the public respecting the checking of the fever then prevailing, suggested “the removal of all the families from those parts of the city where the disease, from the contamination of the atmosphere, appears chiefly to exist, and the preventing those parts being visited by the citizens.”⁵ The College of Physicians also recommended to the Board of Health to procure the “removal of all the families that were situated between Walnut and Spruce Streets, and the east side of Front Street and the river.” In consideration of

¹ Let one example suffice. Of 5,500 inhabitants, which constituted the population of Barcelonette, prior to the outbreak of the epidemic of 1821, 2,000 left the place before the establishment of the barrier or barricade. With very few exceptions, they all remained in health. On the other hand, out of the 3,500 who were compelled to remain and breathe the air of the infected place, 1,300, or 1 in 2.7, perished! (*Rochoux*, p. 665.)

² On the Plague, p. 36; Collected Works, p. 204.

³ Herrera, lib. 3, cap. 6; Rochefort, Hist. Nat. et Morale des Iles Antilles—12mo., ii. 475–6.

⁴ Bally, p. 577; Audouard, p. 449.

⁵ Currie (1798), p. 13.

these suggestions, the health officers earnestly recommended to the inhabitants of the parts specified, "and also those immediately in the vicinity thereof, to remove without the bounds of the city and liberties as speedily as possible."¹ The emigration, however, was far from general. The Board of Health had not, or perhaps did not think it possessed, the power to compel the inhabitants to evacuate the district. The greater part of these heeded not the recommendations of the Academy, the College, or the Board of Health, and remained in their habitations. During the epidemic of 1805, the Board of Health made an attempt to effect the evacuation of the sickly district; but meeting with opposition on the part of the public—and, strange to tell, of some professional gentlemen of good standing—and having no power to carry the plan into effect forcibly, abandoned it.² What has been done in regard to that measure, in Philadelphia and other cities of America and Europe, the grounds upon which it is founded, and the results of its application in contrast with those that have accrued from the adoption and enforcement of regulations of a different character, have already been adverted to somewhat in detail in a former chapter of the present volume.³

This measure, which is recommended by the advocates of the domestic origin of the disease,⁴ on the ground of its preventing exposure to localities rendered impure by infectious exhalations, and by some contagionists, on the plea of removing individuals from exposure to an atmosphere impregnated with the seeds of contagion,⁵ may be viewed as the only one likely to give rise to positively advantageous results. It has afforded the means of arresting the course of the disease, or greatly limiting its progress. The epidemic of 1820, in this city, set in with as much violence as had the memorable visitations of former years. But instead of some eight or ten thousand cases, and near four thousand deaths, which took place in 1793 and 1798, the cases reported in 1820 amounted to one hundred and twenty-five, and the mortality to eighty-three. In New York, in 1798, the number of deaths amounted to more than two thousand; while in 1819 there were only about one hundred and fifty cases, and fifty deaths.

Nor is it less true that in these instances the extent of the disease, and consequently the mortality accruing therefrom, would have been even more limited, had the measure been adopted at an earlier period, and carried into effect more energetically and on a larger scale. But everywhere the procedure has naturally met with so much opposition, as to compel the health officers to act with the greatest circumspection, to evacuate only such spots as were strongly and undeniably infected, and to limit as much as possible the extent of the emptied district. Owing to this, the disease, in New York, was allowed to keep "the start of the barricades that had been interposed, until the general panic which seized all the lower part of the city, about the

¹ Currie (1798), p. 11.

² Caldwell, *Fever of 1805*, p. 49.

³ Chapter xix. pp. 346–349.

⁴ Deveze, p. 302; Rochoux, p. 663; Maclean, p. 139; Dariste, p. 224; Wood, p. 314; N. A. R., p. 412.

⁵ Townsend, p. 220; Robert, p. 616; Pariset, p. 609.

1st of September, caused the inhabitants to abandon their homes *en masse*, and served to put an entire stop to the progress of the disorder, except among those few who wilfully exposed themselves by remaining in their houses, or by going too far down into the most infected streets."¹ In Philadelphia, much the same results were produced; the evacuation was effected too late, and was not sufficiently complete to prevent the appearance, for some time, of fresh cases; and the access to the infected spots, over the barricades or otherwise, was not as completely prevented as was necessary to insure a decided check to the disease. I need scarcely remark that a similar plan is applicable, and has often been applied, to infected ships, by the removal of whose crews the disease has been quickly arrested.

Method of Purification.—The benefit thus obtained from emigration, whether extended to the whole population of an infected district, as takes place in extra-tropical cities; or to the whole crews of infected vessels; or only to the unacclimatized and unprotected, as occurs in southern latitudes, where such a class exists, is produced not by the destruction or removal of the cause of the disease—which, necessarily, continues to be evolved—but by diminishing the number of those exposed to its deleterious agency. But when this has been done, and more particularly when the measure cannot conveniently be adopted, care should be taken to avoid every circumstance calculated to increase the malignancy or favour the dissemination of the poison. Large assemblages of people should be prohibited or avoided, so also crowded houses or rooms; and free ventilation should be enjoined in hospitals, in all public establishments, and in private dwellings. Nor is this sufficient; attempts must, at the same time, be made to purify the place, and destroy the morbid poison to which the disease owes its origin, or at least to mitigate its malignancy. For this purpose, the same means employed to prevent the outbreak of the disease in any place must be resorted to; streets, alleys, gutters, sinks, and sewers must be cleansed and expurgated; all sources of noxious effluvia must be removed; vessels must be transferred to other wharves, or kept at anchor in the stream; and the docks and wharves must be purified. If the disease proceeds from noxious exhalations issuing from a vessel lying in port or elsewhere, individuals residing or working in the vicinity must be protected by the removal of the vessel to some other place, or by clearing the neighbourhood of its inhabitants. The cargo, if any exists, must be discharged, and, if necessary, aired, purified, or destroyed; and the hold, timber, and other parts of the vessel must be cleansed and purified also.

Of the means of purification to be resorted to, I need not say much in this place. Where the circumstances of the case will admit, as on shipboard and in public or private dwellings—when the sources of infection are localized in them, or when these are in a condition to increase the virulence of the poison originating in the vicinity—heat may be, and has been, employed with advantage. Of the disinfecting or deodorizing power of fire or heat, much has been said by philosophers and physicians in olden times; indeed, if the claims

¹ Townsend, p. 221.

of any hygienic means to our respect could be enhanced by its antiquity, few would be more entitled to it than the one in question. It was recorded long before the days of Pliny, who speaks of it as of a thing well known: "There exists in the very fires a remedial power against pestilence occasioned by obscuration of the sun by clouds, and by an excess of moisture. Fire, by its fumigation, certainly assists in many ways. Empedocles and Hippocrates have demonstrated this amply."¹ Poets, too, recognized the benefits derived from that agency. "Or all noxious principles," Virgil says, "may be dried out of them by fire, and useless moisture driven out:—

. "Sive illis omne per ignem,
Excoquitur vitium, atque excedat inutilis humor.'"

The reader will doubtless recollect that it is reported of the father of medicine that he changed the morbid state of the atmosphere at Athens, during the plague described by Thucydides, by kindling large fires.² The same is related of Acron of Agrigentum.³ At a less remote period, the city of Rome furnishes a striking example of the benefit which was supposed to arise from the same practice. "Rome," as Lancisi remarks, "an unwholesome region, may, thanks to her furnaces, be inhabited with safety." Monfalcon informs us that when the French troops occupied the Mantuan, during the early Italian campaigns of Bonaparte, they were forced to encamp on the marshy surfaces which abound in that province, and in consequence exposed to malarial fevers. Bonaparte succeeded in preserving the health of his men, by ordering them to keep, day and night, near large fires, kindled for that purpose.⁴

Sir Gilbert Blane speaks in the highest terms of heat as a purifying agent on board of ships, and states that nothing contributed so much to disinfect the filthy French ships captured by Admiral Rodney in the famous battle of 1782, and sweeten the air in them, as burning fires in the hold.⁵ And we all know the advantage resulting, in all malarious localities, from warming and drying the houses, especially early in the morning and at the approach of night.

Unslaked lime, strewed over the surface of the streets and over sources of inhalation, has long been recommended. The plan was tried on a large scale in New York in 1822, at the recommendation of Dr. Akerly and others; but, as it would seem, without the least success.⁶ "The progress of the disease towards the north," says Dr. Townsend, "had already been arrested by the desertion of the lower part of the city before these substances (lime, ashes, and other alkalies) began to be scattered through the streets, and the

¹ Cap. 27, lib. 36.

² Galen, *Therap. ad Pison*; Aëtius, v. 94.

³ Plutarch, *De Iside et Oriside*, see Adams's *Trans. of Paulus Ægineta*, i. 274; *Ib.*, *Tr. of Hippocrates*, i. 12.

⁴ *Traité des Marais*, p. 201.

⁵ *Diseases of Seamen*, pp. 117, 287. See also Blane's *Dissertation*, i. 220; Folchi, *N. A. Med. and Surg. Journ.*, vii. 252; J. Clark, p. 67.

⁶ See *Med. Reposit.*, ii. 216; Valentin, p. 235.

contagion (?) continued to spread through Lombardy and Cheapside Streets, though the lime was applied, and though most of the inhabitants moved away immediately after the first cases occurred there. Besides which, a number of persons employed in carting those substances took the disease and died" (p. 223).

Besides these means, others have been recommended and resorted to; the lighting of large fires in the streets and public places—the discharge of artillery¹—the burning of pitch, tar, gunpowder, &c.; but, so far as I can ascertain, no benefit has been obtained from their use. Indeed, it is doubtful whether much advantage ever accrues from any of the means employed to limit the progress of the disease when once it has appeared in any locality and assumed an epidemic form. That ships may be occasionally purified and rendered habitable, even in southern latitudes, or in the summer season, everywhere, is doubtless true. Examples of this are mentioned by authors, and have been referred to in the present work;² but they are comparatively rare. More frequently the result is not obtained; and the disease continues to prevail until the vessel reaches cold latitudes or experiences the beneficial change of a winter atmosphere. On land, the effect, if ever obtained, is so still less frequently. The fever does not feel, to any great extent, the effect of the means employed to arrest its progress; we may mitigate its violence, or guard against its occurrence; but when once the poison has commenced to be evolved, they seldom do much towards putting a stop to that evolution. In a word, when the disease once invades a locality, it usually sets human opposition at defiance, so far as respects its march and duration—spreading and maintaining its ground until it is overpowered by cold weather, or it wears itself out. Under these circumstances, we are forced to rely on the only measures within our reach for limiting the number of its victims—the willing or compulsory emigration of the inhabitants of the infected region, and the closing up of the latter till the accession of frost or cold weather.

Means for Preventing a Return of the Disease.—As regards the means to be employed for the purpose of preventing the return of the fever after it has ceased to prevail—which forms the third division of the subject of the present chapter—little need be said. When the place infected—be it a ship, or a locality on land—has been completely vacated of its inhabitants, it should, prior to reoccupation, be subjected to a thorough purification. It is true that a cold atmosphere, and especially a severe frost, will, by destroying the poison giving rise to the disease, not only arrest its progress but prevent its return, even when no precautionary means have been employed. But experience, at the same time, shows that such has not always been the case—that the disease has broken out afresh on the return of the inmates of a place, or the crew of a vessel that had been thought perfectly free from contamination. Hence it will be safer not to trust exclusively to the expurgative influence of natural means, but to aid these by a recourse to artificial ones calculated to

¹ Deveze, p. 294; Dalmas, p. 45.

² Charleston Journ., iv. 4; Chisholm, i. 413, 414.

destroy the remaining germs of the poison—cleaning, washing, free ventilation, fumigations, and the like.

In some cases, it may be useful to submerge, or cover over with earth, the local source of infection, as was done at Gallipolis in 1796;¹ or, when that is impossible, recourse may be had to ordinary disinfectants.

Chlorine, and other fumigations, destroy ammonia and organic bodies with more or less facility; and there are not wanting facts to show that they exercise a salutary influence in mitigating or arresting the progress of fever.² The same may be said of chloride of lime, whose efficacy, though doubted by some observers,³ is highly thought of by reliable professional authorities in this and other countries;⁴ of chloride of zinc, which is highly extolled by competent judges;⁵ of smoke, which has been successfully employed in Germany, France, and elsewhere, on land and on shipboard;⁶ as well as of the sprinkling of lime,⁷ and of ozone, the most powerful of disinfectants.

But, while thus pointing out the various substances that have been used and recommended for the purposes in question, it is not to be concealed that, extolled as some of them have been by respectable authorities, their efficacy has been greatly doubted—after extensive trials, too—by experienced observers. We find no one disposed to sing the praise of chlorine as loudly as was done in former days by Guyton Morveau; or to look, with Carmichael Smith, to the nitrous acid gas as an effectual destroyer of the fever poison. In Spain, especially, experiments on an ample scale were made with disinfectant

¹ Potter, on Contagion, p. 16.

² Savarésy, p. 451; Dariste, p. 227; Bally, p. 591; Valentin, p. 233; Arnold, p. 18; Robert, p. 574; J. Johnson, Charleston Journ., iv. 717; Gimbernay y Grassot, Descripción y uso de un nuevo metodo para preservacion del contagio, &c., Barcelona, 1821; Bally, p. 591; Rochoux, p. 671; Deveze, p. 296; Townsend, p. 223; Cullom, p. 365; Roberts, p. 574; Guyton Morveaux, Carmichael Smith, Playfair, Graham, Hoffman, Cooper, in Sec. Rept. of London Commissioners, 1848, p. 32, &c.

³ Bowie, in same work, p. 83.

⁴ Johnson, Some Account of the Origin and Prevalence of Yellow Fever in Charleston, Charleston Journ., iv. 164; Southern Agriculturist, iv. 250, 417, as quoted by Dr. Johnson.

⁵ Bryson, p. 225.

“But there is one substance that remains to be mentioned, *i. e.* the chloride of zinc, which possesses, at least so far as has been yet ascertained, all the requisite properties. Without being in any way unpleasant to the senses, or injurious to health, it has the power of most completely destroying the effluvia arising from animal or vegetable substances in a state of decay. By numerous experiments performed in various vessels in different parts of the world, which, but for the time and space they would occupy, might be enumerated, it appears that the instant the slime or rubbish in the timber of a ship becomes saturated with a solution of the salt, the process of decomposition—putrid fermentation—is instantly arrested; and, consequently, also the liberation of mephitic gases.” (*Bryson.*)

⁶ Zimmerman, de l'Expérience, notes by the translator; Bonnet, Essai sur la Purification de l'Air, p. 17; see also Hoffman and Van Swieten; Galeron, Mém. de la Soc. Roy. de Méd., iii. 44; Lind, on Seamen, p. 74.

⁷ Forbes's Review, July, 1844, p. 196.

vapours, but the result was of an unfavourable character, and they were laid aside.¹

Personal Prophylaxis.—It remains now to pass in review the means required to shield individuals, exposed to the action of the morbid poison, from an attack of the fever, or to mitigate its virulence and danger.

We have seen that in tropical regions, and in our southern cities, individuals acclimatized, by continued residence or by having passed through the disease, are exempt from the latter, and continue, with few exceptions, to breathe the tainted atmosphere with perfect impunity. All others—strangers, and children, and such of the adult natives and long residents as have lost their acclimatization by a sojourn in a cold climate—are, to a greater or less extent, liable to the disease. The same danger is incurred by the entire population of the northern cities of this country, as well as of those parts of Europe where the disease occasionally prevails, excepting the few who come from southern latitudes, and have not as yet lost their acclimatization, or who have passed through the disease at some former period.

But though individuals, unprotected in the ways mentioned, are all more or less liable to the morbid effect of the poison, and cannot shield themselves completely, and without fail, from an attack of the fever—and though, so far, no antidote against the poison has as yet been found—yet experience has everywhere shown, that, by proper precautions, some are enabled to effect that object, and that a larger number succeed in mitigating the virulence and danger of the attack. Before enumerating the precautionary means required for this purpose, it is proper to remark that, though they should in no instance be neglected, some danger is incurred by devoting an exclusive, incessant, and very minute attention to them. By such a course, the individual's mind is kept alive to the danger of his situation—his nervous susceptibility is aroused or increased, and he is thereby rendered more liable to an attack than he would otherwise be were his mind occupied with more agreeable objects.²

In preceding chapters of this work I have entered in detail on the causes—remote, predisposing, and exciting—of the disease, and pointed out their respective importance. It will at once be perceived, that the great object for all susceptible individuals must be to avoid the operation of those causes; that by so doing they will shield themselves from the disease; and that hence it might be sufficient to close the subject before us with the mere statement of the fact, that it behooves all individuals to place themselves beyond the reach of such influences. Nevertheless, the importance of the subject, and the attention paid to it by the majority of writers, combined with the fact that, while some things are to be avoided, others are to be done, induce me to devote a few pages to its consideration.

1. Individuals exposed to the infection of the yellow fever must avoid

¹ Arejula, Memoria sobre la ninguna utilidad del uso de los Gases Acidos para la disinfeccion ó purificacion de las materias contagiosas y de los contagios, &c., 1821.

² Dariste, p. 214; Bally, p. 585.

everything calculated to lessen the cheerfulness of their spirits; they must seek all rational means of diversion and amusement. They must preserve a calm disposition; resist every feeling of fear and terror; shun all subjects of grief, and avoid strong mental emotions.¹ At the same time, excessive courage is often to be deprecated, as leading to indifference and acts of imprudence.

2. Individuals living in an infected atmosphere are benefited by encouraging a due proportion of sleep, and avoiding night watching.² They should retire and rise early. By the first means they enjoy a more refreshing sleep; by the second, they have the benefit of some hours of fresh air, which enables them better to support the forthcoming heat of the day. At the same time, they should resist the tendency to prolonged sleep, which often prevails in hot weather, and which, when indulged in, serves to induce a state of torpor, followed by debility, and, as a consequence of this, an increased power of absorption.

3. They should promote a gentle perspiration, and moderate the discharge if too copious, fatiguing, and debilitating. For this purpose, resort may be had to dry frictions and to tepid baths, if these usually agree. When used, they should be followed by several hours of repose, and some refreshing drink, inasmuch as otherwise they excite perspiration beyond the desired point.³ They are especially called for when the climate and weather are dry—they act in several ways: by cleansing the skin, but moreover, like aqueous drinks, by furnishing materials to the blood, which, under such circumstances, are especially needed. Like drinks, they calm general irritation, produce a feeling of relaxation in the whole organism, and appease thirst. (*Celle*, pp. 374–5.)

4. Cold or river baths, so grateful to many in the hot season during which the yellow fever prevails, have found but few advocates—and, indeed, are disapproved of by competent authorities (*Levacher*, p. 94). They should be avoided, more especially in the cool of the evening, when reaction is less likely to occur, and still more particularly when the body is covered with perspiration. As a general rule, they ought never to be used by debilitated persons, convalescents, or those who are subject to special visceral diseases. If any chill, or sensation of coldness is experienced after their employment, they must be abandoned, and tepid water substituted (*Daniell*, p. 24). Williams recommended them to persons too much relaxed (p. 53). They are equally injurious if partial—the mere wetting of feet is conducive of much mischief.⁴

¹ Williams, p. 54; Deveze, p. 300; Savarésy, p. 448; Dariste, p. 214; Valentin, p. 225; Lempriere, ii. 114; Audouard, p. 436; Rochoux, p. 619; Bally, pp. 585–6; Wood, p. 314; Gilbert, p. 82; Caillot, p. 274; Doughty, p. 83; W. F. Daniell, *Quarterly Journal of Public Health*, i. 28 (1855).

² Lempriere, ii. 114; Valentin, p. 225; Rochoux, pp. 618, 624.

³ Williams, p. 53; Moultrie, p. 23; Leblond, p. 109; Gilbert, p. 81; Bally, p. 589; Levacher, p. 94; Rochoux, p. 624–5; Savarésy, p. 447; S. Brown, p. 76; Caillot, p. 268; *Celle*, pp. 374–5; Daniell, *op. cit.*, p. 24.

⁴ *Celle*, p. 273; Levacher, p. 94.

5. On the other hand, sea bathings, when they can be procured conveniently, are highly commended. They give tone to, while they cool, the system, and enable it to resist the morbid action of the poison.¹ It should not be forgotten, however, that when the distance to the sea is considerable, the fatigue of walking to it, and particularly that of returning, is calculated very greatly to mar the advantage derived from the bath (*Rochoux*, p. 625). In individuals of a sanguine temperament, and of plethoric habit, in whom the power of reaction is considerable, this means is of doubtful utility.

6. Care must be taken to avoid exposure to rain, to currents of air, to atmospheric vicissitudes, and to everything likely to check perspiration suddenly. Should the individual be exposed to a shower of rain, his clothes must be changed; ablutions of cold sea-water, if possible, will be useful.²

7. Still more essential is it to avoid prolonged exposure to night air, which, as we have seen, has ever been considered—especially during sleep—a fruitful cause of the disease.³

8. The practice of wearing flannel, or coarse cotton muslin next the skin, has been very properly recommended. For the same reasons, cotton shirts are preferable to linen shirts. By means of these, a gentle irritation or warmth is kept up on the skin, perspiration is absorbed, and the individual is less affected by vicissitudes of temperature. It is proper, also, to avoid very thin garments, and to wear cloth coats in preference to those of cooler materials, which are generally found inadmissible in tropical regions during several months after arrival. Hot mattresses, feather beds, and pillows, should be avoided; thick mosquito bars also. The beds should be placed in a well-aired part of the room—not in corners, or close up to a wall. (*Celle*, pp. 356–7.⁴)

9. Exercise should not be neglected; but it must be moderate, and taken morning and evening. It promotes the activity and healthy performance of the digestive functions, equalizes perspiration, and is always followed by a feeling of relaxation and comfort. At the same time, rapid walking should be as much as possible avoided, in order to prevent the general excitement most usually produced by it. On this subject, strangers in tropical climates, as well as in our southern States, cannot do better, during the period of sickness, than imitate, as has been recommended, the practice of West Indians, who are noted for the slowness of their movements.⁵

¹ Levacher, p. 94; Bally, pp. 586, 9; Grant, p. 58; Deveze, p. 298; Caillot, p. 266; Despérières, p. 65.

² Rush, Fev. of 1793, p. 161; Rochoux, pp. 617, 618; Wood, i. 314; Dariste, p. 217; Caillot, p. 267; Celle, pp. 275, 277, 281.

³ Rush, iv. 18; Grant, p. 58; Moultrie, p. 62; Caldwell, p. 77; Valentin, p. 228; Lempriere, ii. 113; Wood, i. 314; Dariste, p. 217; Gilbert, p. 81; Caillot, p. 260; Arnold, p. 60; Celle, p. 292; Daniell, p. 25.

⁴ Grant, pp. 57–8; Savarésy, p. 449; Lempriere, p. 115; Valentin, p. 229; Levacher, p. 95; Rochoux, pp. 617, 618; Arnold, p. 60; Caillot, p. 265; Celle, pp. 262, 285, 354; Daniell, p. 24.

⁵ Grant, p. 58; Valentin, p. 225; Savarésy, p. 447; Dariste, p. 217; Rochoux, pp. 615, 623; Moreau de Jonnés, Milat. des Ant., p. 24; Caillot, p. 255.

10. Excessive corporeal fatigue, however produced, must be avoided, as almost sure to be followed by an attack of the disease.¹

11. The greatest care, also, must be taken to avoid exposure to the direct or indirect action of the sun, or to excessive heat, by whatever means produced. The injurious effect of such an exposure is always considerable in the unacclimatized and unprotected; but is still greater in those unaccustomed to it, or the natives or long residents of cold regions.² For this reason, in the West Indies, the unacclimatized are advised to keep the house from ten in the morning to three in the afternoon, and always to use a parasol.³ Hence, also, it is prudent not to pass from a cold to a hot climate, or to places where the fever is endemic, during the summer season. Danger accrues from the rapidity of the transition, as the system has not had time to become inured to the effect of a high thermometrical range. By reaching such places at the close of the hot, or during the course of the cool season, the system has time to experience a sort of acclimatization, which renders it more apt to resist the depressing action of the forthcoming heat, and thereby to escape the disease altogether, or in its worst form.⁴ It is from observing this, that the practice of not sending troops directly from Europe to tropical climates, but to make them first sojourn in some less hot localities, has been advocated and pursued.⁵

12. During the prevalence of yellow fever, very nourishing, gross, spiced, and stimulating food must be laid aside, and nutritious articles of easy digestion, composed principally of the light meats, farinaceous vegetables, eggs, milk, and the like, substituted.⁶ The necessity of abstinence from the former increases in proportion to the dryness of the atmosphere; for when the latter is both hot and humid, exciting food and condiments are often beneficial in exciting the tone of the stomach.

Dr. Rush (iii. 160) reminds us, and Bally repeats the remark without acknowledgment, that Socrates, in Athens, and Justinian, in Constantinople, were preserved, by means of their abstemious mode of living, from the plagues which occasionally ravaged those cities. And there is but little doubt that the same results have obtained as regards the yellow fever in tropical climates and this country, and that the more ready escape of the French, Italians, and Spanish is in part ascribable to their less addiction to the use of stimulating, gross, and highly nutritious food. To this cause, indeed, has been attributed the entire escape of the French refugees among us;⁷ but we have seen that the

¹ Moultrie, p. 23; Rochoux, p. 618; Wood, p. 314; Dariste, p. 217; Celle, p. 295.

² Moultrie, p. 23; Grant, p. 58; Levacher, p. 94; Rush, iii. 161, iv. 18; Rochoux, p. 615; Lempriere, p. 115; Dariste, p. 217; Caillot, p. 260; Arnold, p. 60; Daniell, p. 25.

³ Rochoux, p. 615; Caillot, p. 260.

⁴ Dariste, p. 215.

⁵ Dariste, p. 218; Savarésy, p. 445; Bally, p. 607; Caillot, p. 256; Cornuel, Ann. Marit., 1844, ii. 737.

⁶ Moultrie, p. 23; Rush, iii. 160, iv. 18; Caldwell, p. 77; Bally, p. 588; Levacher, p. 95; Rochoux, pp. 621, 622; Savarésy, pp. 448-450; S. Brown, p. 73; Valentin, p. 225; Williams, p. 54; Caillot, p. 263; Audouard, p. 439; Dariste, pp. 216, 217; Gilbert, p. 81; Arnold, p. 60.

⁷ S. Brown, p. 75; Webster, p. 221.

circumstance is more probably due to their power of resisting the effect of heat and the action of the poison, acquired in the West Indies.

While selecting articles of food of the kind mentioned, care must be taken not to indulge in anything to excess, and to avoid copious meals, particularly at night. Everything must be taken sparingly.¹ The hours of meals must be so arranged as to allow time to the stomach to be perfectly emptied before being again filled.² At the same time, it would be wrong to go to an opposite extreme—to make a radical change in the regimen used, and to adopt too abstemious a course. By this, the system at large, as also the tone of the digestive organs, would be enfeebled, and greater scope allowed to the action of the poison.³

13. Still more necessary is it to abstain from the use of alcoholic liquors. To those habituated to these, it would doubtless be imprudent to avoid them entirely, and cease their use suddenly; but, even in such cases, they should be allowed sparingly. By all others they must be shunned, unless it be to correct debility or the effects of the water, if the latter disagrees with the digestive organs, or is of an impure quality. Wine—as Madeira, sherry, hock, or claret—is less objectionable, and, in some instances, particularly in hot, damp weather, is absolutely required. But it must be used with moderation, as all excesses in articles of the kind, as in alcoholic drinks, especially if several times repeated, are almost sure to be followed by a fatal attack.⁴

That a contrary course—high living, both as regards food and drink—has been recommended, is true. It is predicated on the idea of the debilitating effects of the heat of our summers and of tropical regions, and on the supposed adynamic nature of the disease. But, as we have seen, such views are the results of untenable hypotheses, and the practice founded upon them is not only erroneous, but highly dangerous, and has led, especially when the atmosphere is dry, to the most fatal consequences, here and elsewhere.⁵

14. If the system is overheated or plethoric—as is very generally the case with northerners, whether from Europe or America, on their first arrival in tropical regions, and new-comers during our summer months—this excitement or plethora must be reduced by proper means—by cooling medicine, abstemious diet, but particularly by the use of the lancet or topical means.⁶ Even

¹ Caldwell, p. 77; Bally, p. 588; Valentin, p. 225; Audouard, pp. 439, 440; Rochoux, p. 618; Lempriere, p. 110; Arnold, p. 60.

² Celle, p. 348.

³ Lempriere, p. 110; Valentin, p. 225; Wood, p. 314; Caillot, p. 263; Celle, p. 220; Williams, p. 54.

⁴ Rush, iii. 82, iv. 18; Caldwell, p. 77; Moultrie, p. 23; S. Brown, pp. 73, 74; Bally, p. 589; Levacher, p. 95; Grant, p. 58; Valentin, p. 226; Audouard, p. 439; Wood, p. 314; Rochoux, p. 627; Caillot, p. 363; Arnold, p. 60; Celle, pp. 235, 241; 331; Robertson, *Hist. of Atmosphere*, ii. 399; Daniell, p. 27.

⁵ Rochoux, p. 626, &c.

⁶ Thion de la Chaume's transl. of Lind, ii. 31; Rush, p. 160; Williams, pp. 52, 53; J. Clark, p. 46; Lempriere, ii. 106; Rochoux, p. 631; Gilbert, p. 80; Caillot, pp. 275–277; Audouard, p. 442; Dariste, p. 219; Bajon, *Mémoires*, &c., sur Cayenne, i. 34; Celle, p. 228.

by practitioners who deprecate the use of the lancet in the treatment of the disease, venesection is regarded as a useful means of preservation in individuals presenting the symptoms mentioned.¹ But while under these circumstances, and especially when the atmosphere is hot and dry, the practice is appropriate and useful, it should not be resorted to indiscriminately in all cases, as has been occasionally done in the West Indies, both by physicians and the public at large.

15. Few things are more agreeable and refreshing to a very large number of individuals, during the hot days of summer, both in this country and in tropical regions, than cool or iced acidulated drinks, lemonade, orangeade, cold or iced water, acid fruits, &c. When taken in moderation, these articles may be allowed, and are even occasionally useful,² tending, as they do, to lessen the irritation of the gastric organs and of the system at large, which is apt to prevail at that time, and to furnish aqueous materials to the blood, which the increased excretions and secretions so greatly diminish at such periods, and the absence of which gives rise to the sensation of thirst. Nevertheless, they should not be indulged in to any very great extent, as they are apt, when so used, to reduce the tone and nervous excitability of the stomach, which it is so essential to preserve.³ They, besides, produce a bad effect, from the lowness of their temperature, when the body is warm and perspirable.⁴

16. Care must be taken to preserve a soluble state of the bowels, and to correct, by proper and gentle means, the tendency to constipation, which frequently prevails under such circumstances.⁵ For this purpose ripe fruits may be used—in tropical countries they are sometimes recommended—but care must be taken not to indulge in their use very plentifully, as they are then apt to be injurious.⁶

17. When, on the contrary, there exist indications of debility of the whole system, or an atonic state of the digestive organs, bitters, and even the stronger tonics, are called for, and will be found serviceable.⁷ There are not even wanting those who regard the use of bark and other tonics as worthy of usual application, affording, as these do, a proper stimulus to the stomach, and thereby preserving the system against the inroads of the disease.⁸

In a late publication,⁹ Dr. Cummins states that he has often used quinia as a prophylactic, with much success. “Indeed, I now make it a point of giving it to all who have recently arrived from Europe, especially midshipmen who have never been in the tropics before. I use it in the doses recommended by my friend, Dr. Campbell, of Kingston, Jamaica—three grains twice a day, taken as bitters.” Dr. Bryson may also be referred to on the subject. “From previous observations made on the coast (of Africa), fol-

¹ Savarésy, p. 446.

² Williams, p. 54.

³ Caillot, p. 364.

⁴ Celle, pp. 268, 350.

⁵ Rochoux, p. 631; Dariste, p. 219; Moultrie, p. 62; Arnold, p. 60.

⁶ Caillot, p. 363.

⁷ Rush, p. 82; Deveze, p. 296; Dariste, p. 220; Valentin, p. 226; Bally, p. 587.

⁸ Caillot, p. 264; Lefuente; Rollo, p. 147; Bryson, p. 219.

⁹ Clinical Remarks on Yellow Fever, London Lancet, 1853, ii. 220.

lowed by a careful review of the various circumstances connected with exposure on shore and on boat service, it is firmly believed that although neither bark nor quinia has the power of preventing the germs of fever from lodging in the system, where they may lie dormant for a period of from fourteen to twenty days, or even longer, nevertheless, from their peculiar antagonistic properties, they most decidedly have the power, in many instances, of preventing their development in pyrexial action. Hence the frequently supposed failure of the medicine is undoubtedly to be attributed to its use not having been persisted in for a sufficiently long time after exposure to the exciting causes—namely, throughout the entire probable period of incubation. It is therefore suggested that it would be advisable not only to administer daily one of these febrifuges to men, so long as they are exposed to the influence of the land and the vicissitudes of the weather in open boats, but to continue its use for at least fourteen days after their return on board. As the sulphate of quinia is more certain in its action, infinitely less nauseous than bark, and therefore less objectionable to fastidious people, it should invariably be preferred for exhibition; whether it be given in wine, water, or rum, is of no great consequence; the latter will generally be the most acceptable to seamen, although they will seldom object to it in wine.” Useful as the practice may have been in the hands of some physicians, and particularly advantageous as it may have proved in the prevention of common intermittent and remittent fevers, to the latter of which Dr. Bryson’s remarks chiefly apply, it may be doubted whether the bark and its salts have generally served a good purpose as a prophylactic of true yellow fever.

18. Individuals exposed to the influence of an atmosphere tainted with the yellow fever poison, cannot be too guarded as regards sexual intercourse. Excesses in the repetition of the venereal act have the same fatal tendency in this disease as they have in the oriental plague; and, however inconvenient and painful it may be to adopt a life of complete abstinence on this point, such inconvenience and pain sink into perfect insignificance when compared with the danger accruing from a contrary course. Hence the aptitude of newly married people to contract the disease.¹

19. No attempt should be made to repel such cutaneous eruptions as may exist at the time; not even the prickly heat, which is very apt to prevail during the hot season; and is a common occurrence among new comers in tropical climates. All that can prudently be done, in regard to these eruptions, is to moderate the irritation they produce, if very troublesome, and especially when sufficiently intense to produce fever.²

Much importance was attached at one time to the prophylactic agency of setons, issues, or other exutories, or to any kind of suppurating sores, to wounds, erysipelas, inflammations, &c. Experience, however, has shown the inefficacy of such morbid states. But while having no faith in their beneficial agency, it cannot be denied that when sores, issues, ulcers, or the like, have

¹ Bally, p. 588; Rochoux, p. 618; Caillot, p. 275; Celle, pp. 360–61.

² Rouppe, p. 280; Rochoux, p. 628.

existed for some time, they should not be suddenly healed. Dr. Robert Jackson perceived, during the sickness which prevailed among the British troops in St. Domingo, that the healing of an ulcer was often followed by a fatal flux or dangerous fever.

20. Much was said, at one time, of the preservative agency of venereal discharges, not only as regards the yellow fever, but also the oriental plague. Ambrose Paré and Oræus were of opinion that individuals labouring under such complaints were less apt than others to suffer from the latter disease. During the prevalence of the epidemic of Nimeguen, Diemerbroeck made similar observations. In relation to the yellow fever, cases are adduced to show that individuals suffering from discharges of the kind have passed unscathed through the ordeal of an epidemic, or have had the disease in a mild form. But, whatever may have been the effect of the antagonism of the poisons, so far as relates to the plague, no facts of a satisfactory kind can be adduced to show that syphilitic affections have ever shielded against the yellow fever. The disease has often coincided in the same individual with venereal symptoms, and by them has not unfrequently been rendered more malignant; and as, in cases in which the result appeared to be beneficial, the individuals were subjected to a mode of regimen and treatment, the prophylactic effects of which are undoubted, we may reasonably refuse our belief in the protective power attributed to the complaints in question.¹

21. Mercury has been much celebrated as an antiloimic agent in this and other forms of malignant diseases. If the authority of former times can be of consequence in adding to its reputation in this respect, as Dr. Robertson observes, it will be found in the *Commentaria de Rebus in Medicina Gestis*, (vol. i.), that “in featribus malignis, mercurialia esse optima remedia,” &c. In the works of Paræus, also, we find it recommended as a preventive against the plague, on the authority of Mathiolus; and Diemerbroeck affords us the following practical instances of its good effects in this way: A young lady, who happened to be under the influence of mercury for the cure of the venereal disease, when plague raged with great violence in Holland, escaped its contagion, although three servants died of it in the house. Similar instances are said to have been observed in hospitals in the case of typhus.

We hear, also, of trials having been made with the remedy as a preventive of yellow fever; and there can be little doubt that if the reports in circulation be of a reliable character, the success obtained is such as to recommend it to our serious consideration.² In support of the prophylactic effect of mercury, the case mentioned by Dr. Walker, of Jamaica, has often been cited. At the taking of Fort Omoa on the Spanish main, a malignant fever broke out there and swept away great numbers of men from all the squadron, and was also so fatal on shore, that the 79th regiment alone lost eight officers. Among the ships which were captured in that harbour, one was loaded with quicksilver, for the use of the mines;

¹ Dariste, pp. 220–21; Rochoux, p. 628; Copland, iii. 151; Blair, p. 70; Savarésy, p. 257; Caillot, p. 278; Valentin, p. 254.

² J. Clark, p. 46.

and the vessels which contained it being broken by the shot of the *Pomona* (frigate), the mercury was found floating in large quantities, and a number of men were sent on board to collect it, which they did with their hands, by throwing it into pails or buckets. These men were continued in this employ during all the time they remained there; and not a man of them had the smallest complaint, though surrounded by disease and death.¹

It may be doubted, however, whether the mercurial plan can be safely trusted for the purpose in question. Many have taken the yellow fever, as well as other malignant fevers, and died of them while under the effect of a salivation for other complaints. Indeed, it may even be said that the disease has very generally appeared to be thereby rendered more severe.²

22. The prophylactic or disinfecting power of tobacco smoke has been extolled at various times and in various places. According to its eulogists, smoke acts not only by modifying the miasmal poison giving rise to the disease; but also by exciting the mucous and salivary secretions, which, when abundantly discharged, contribute very actively to the elimination of the injurious molecules floating in the system. Dr. Celle is of opinion that tobacco smoke produces a very good effect in hot and damp climates, and advises the use of a cigar whenever it becomes necessary to traverse, reside in, or visit an infected locality.³ The practice is recommended to those who cross the Pontine marshes.⁴ In relation to the yellow fever particularly, it has been disapproved of as ineffectual by respectable authorities.⁵ "It may seem like trifling to notice the use of tobacco in its various forms," says Dr. Bryson, "but as it has been supposed to possess prophylactic properties, and has been used for that purpose on the coast, it may be as well to state that there are not any just grounds for believing it to be of the slightest value in this respect, or, at all events, that its virtues are on a par with those supposed to belong to a camphor-bag, or a Krooman's gregory" (pp. 219, 220).

23. Oil frictions were early suggested by Williams (p. 53), and recommended or approved by Valentin (p. 232), Caillot (p. 272), and others, principally on the ground of their supposed usefulness in the oriental plague, and of their reputed success in ancient times. As Dr. Robertson remarks, there are many nostrums, whose efficacy depends entirely on this ingredient, sold in every country where pestilence frequently appears. The dealers' of oil in Egypt, are said to be exempted from the plague. But, however this may be, the efficacy of that means, as a preventive of yellow fever, is far from having been proved in a satisfactory manner. Oil frictions were extensively used in Spain in 1804. They failed in the hands of Dr. Payra Saravia at Ecija (*Bally*, pp. 546-48). Dariste was not more successful with them in the West Indies.

Nor can we at the present day attach much importance to the internal use of olive oil as a preventive means. It was extensively used as such

¹ Med. Repos., i. 497.

² Pignet, p. 377; Robertson, ii. 402; Rochoux, p. 629; Chisholm, i. 479, 482.

³ Hygiène des Pays Chauds, p. 366; see also Wallace, Ed. Journ., xlv. 282.

⁴ Pym, p. 19.

⁵ Valentin, p. 234; Caillot, p. 279.

at Alicant in 1804-5, by Dr. Alcares and others; by Dr. Verde at San Lucar de Barrameda, and by Dr. Delon at Cadiz (*Ib.*, p. 547). But, so far as I have been able to ascertain, the success obtained was in neither instance of a nature to justify a reliance on the remedy.

24. To the unacclimatized and unprotected—and in our middle States, they constitute a very large majority—I would emphatically say, remove to some high position—if possible, away from the infected district, or, still better, out of town; and, when there, do not venture back, even for a moment, until the complete cessation of the fever. It is the only good preventive. When you do venture into the sickly locality, let it be during the middle of the day—and do not *sleep* there.¹

24. If they remain in the infected locality, the unprotected must be sure to select for their habitations, and more particularly for their sleeping apartments, elevated, well ventilated, and dry rooms; for in these the matter of infection is less concentrated and active, and the disease, in those attacked or carried there, assumes a less malignant and more tractable form. They must, as much as possible, for reasons assigned, avoid exposure to night air, and when possible keep their bodies in motion.²

26. They must keep out of crowded assemblies when these take place under cover. They conduce to the impurity of the air, and by this means, localize the disease, as it were, and enhance the virulence of the poison.

27. They must not venture abroad very early in the morning, and especially on an empty stomach; and, when compelled to leave the house, or rise long before the morning meal, they must drink a cup of warm coffee, or tea, or some aromatic infusion, or swallow a dose of quinia, or even a little brandy, as a guard against the rapid absorption of the miasma which takes place during a state of vacuity of the gastric tube. From the advantage of this precautionary measure, has doubtless arisen the custom, so prevalent in some parts of tropical countries—in Martinique, Guadaloupe, Guiana, Cayenne, &c.—of taking a cup of strong infusion of coffee on rising in the morning. By means of this substance, the bitter principle of which bears some analogy to that of cinchona, intermittent fevers, as we learn from Moreau de Jonnes, are frequently prevented. The same effect is obtained by bark itself, or some of its salts taken in the same way. When the French army encamped in the morasses of Mantua, the soldiers were all made to take, at the dawn of day, a portion of tincture of bark, and from this precautionary measure, which was long ago recommended by Lind and other writers, the most beneficial results were obtained.³

¹ Rush, iii. 83; *Ib.*, iv. 164; Deveze, p. 299; Wood, p. 314; Grant, p. 58; Robert, p. 582; Levacher, p. 95; Rochoux, pp. 616, 617; Valentin, p. 230; Caillot, p. 256; Bally, p. 588, 590; Dariste, p. 217; Dazette, p. 43; Pugnet, p. 338; Gilbert, p. 80; Lempriere, pp. 108, 109; Savarésy, p. 446; Hunter, pp. 307, 308.

² Rush, iii. 82; Levacher, p. 95; Wood, p. 314; Audouard, p. 442.

³ Blanc, Dissertations, i. 221; Bryson, p. 227; Aubert Roche, Mém. sur l'Acclimatement, An. d'Hyg., xxxiii. 23; Monfalcon, op. cit., p. 232; Celle, Hygiène des Pays Chauds, pp. 347, 367; Moreau de Jonnes, Hygiène Militaire des Antilles, p. 67; Lind, on Hot Climates, pp. 317, 318; Thevenot, p. 95; Caillot, p. 264; Rollo, p. 147; Wood, i. 314.

But, while avoiding exposure abroad early in the morning, individuals must refrain from adopting the practice of isolation; for, as we have seen, no benefit accrues, or can accrue, from it. Let them not be deterred from nursing or visiting the sick, unless these are situated in more infectious places than those where they reside; but, under these circumstances, let their refusal be founded on the apprehension of the greater intensity of the miasma, not on that of any contagious poison exhaling from the bodies of the sick, or emitted from their garments or effects.

28. Lastly, they must not neglect to attend to the slightest indisposition they may experience—especially if this be characterized by a sense of heaviness about the head, flushes of heat, attended with a disposition to sleep during the hot hours of the day, and agitation and sleeplessness during the night, a bitter taste in the mouth, and a tendency to costiveness. By resorting to proper treatment, these symptoms, the usual forerunners of a decided attack, are often, though not always, averted. When neglected, as is but too often the case, they are apt to be followed by others of a more serious import.

In summing up the more essential of these hygienic precepts, I may say that care must be taken to preserve, as far as possible, the natural energy of the system, by giving to the body regular sleep, adequate exercise, and a moderately nourishing diet—avoiding extremes on either of those points. Equal care must be taken to furnish to the mind that degree of recreation and employment which will preserve it from a feeling of idleness and ennui, and dissipate all thoughts of danger. Individuals must protect themselves from the influence of the tainted atmosphere by emigration, or at least breathe it in an altered or diluted state. Their apartments should be well aired and kept dry, for which purpose, fire—morning and evening, except in hot and dry weather—is found highly serviceable; and, during the night, the windows of the sleeping rooms should be closed. Individuals residing in the infected locality ought, if possible, to be within doors by sunset, and when that is not possible, they should not stand or sit while abroad, but keep the body in a state of activity; and, finally, they must avoid, if possible, sleeping within the range of the poisonous influence.

A P P E N D I X .

AFTER what has been said in the preceding portions of this volume relative to the origin, causes and mode of propagation of the yellow fever and the laws by which it would seem to be governed, the reader need scarcely be told that I do not coincide in opinion with those who refuse to admit the local origin of the disease, or who, whether doing so or not, insist upon its power of transmission from place to place and its epidemic spread in the locality where so introduced. If from the facts and arguments to which attention has been called, we are justified in disbelieving the contagious character of the fever, and moreover in denying or greatly doubting the correctness of the hypothesis of the fermentative assimilation of the efficient cause—whatever this may be—there can be but little probability of the disease being imported from an infected to a healthy locality in a ship or otherwise, or of its spreading epidemically when the cause is introduced in the body of the sick, in the hold of a vessel, or packed up in a trunk of clothes, a box or package of merchandise, &c.

Such being the case, I might, without impropriety, dismiss the further consideration of the subject. But in view of the fact that the opponents of the local origin of the fever, as well as some who rank among the contingent contagionists, and a few who, though believing in that local origin and denying contagion, nevertheless admit that the cause may be transmitted from one place to the other, attach great importance to—indeed derive, in great measure, the foundation of their belief from—what appears to them evidences of transmissibility as exhibited in the outbreak, rise and progress of the epidemics of this city, of other parts of the country, of Europe and of tropical regions; seeing, besides, that facts which seemingly militate in favor of importation are held up in a tone of triumph by the advocates of contagion in proof of the correctness of their belief, and on the other hand that everything having the least semblance of proof of the communicability of the disease from one individual to another has been and continues to be scraped up to show its exotic origin, it becomes necessary to devote some pages to the analysis of the facts connected with the rise and progress of some of the more prominent among the innumerable epidemics that have been adduced here and elsewhere in illustration of that origin and of the power of transmissibility of the disease through the various channels of communication specified.

In this examination I shall principally dwell on the epidemics of this city. The reader will scarcely be disposed to attach much importance to the accounts we possess relative to the importation of the disease in its early visitations. When we find how difficult it is always to make out a plausible case of importation in modern times, with abundant means of investigation at the command of the inquirer and an easy access to ample testimony, we may be pardoned for hesitating to adopt

conclusions founded on evidences such as those that have been adduced in favor of the exotic origin of the fever in our early periods of calamity. How, for example, is its importation in 1699 made out? If the reader will turn back to what has been said on p. 53 of the 1st volume, he will perceive that the fact rests solely on the loose statements and hasty conclusions of individuals who wrote on hearsay evidence long after the occurrence of the event they undertook to record. Dr. McWilliams and Sir W. Pym themselves would, in all probability, hesitate before deciding as to the importation of the disease into a city merely on being informed by Mr. Pemberton that he recollected to have heard his father say, when he was but fifteen years old, that it came in a ship (or other sea vessel) from the island of Barbadoes—that the cargo of the said vessel consisted partly of cotton in bags, which was landed at a wharf between Market Street and the drawbridge, and there stored for sale; that the disease made its first appearance in the family of the owner or consignee and the neighbourhood, and thence spread through the city.

The story of the exotic origin of the fever of 1741 is not more satisfactory—whether we assume Lind's or Bond's version of the occurrence. For, according to the former, it was introduced in a trunk of clothes—how, and by whom, nobody can tell; whilst, according to the latter, the city was indebted for it to a number of convicts from the Dublin jail. Many evils come to us from that side of the water; but so far yellow fever has not been of the number. Nor am I sure that the impartial reader will be better satisfied with the importation of the disease in 1747. As we have seen (vol. i. p. 60), all the authorities we have in relation to this epidemic agree in ascribing its origin to importation from abroad. By one it is stated that the disease was communicated by the clothing contained in the chest of a person who died of the disease in the West Indies. One writer specifies Barbadoes, and states that the person or persons present at the opening of the said chest were the first to take the fever, and that the greater number of them were carried off by it; and that most if not all the professional men of the time believed the correctness of the statement. The writer gives the following statement as to the manner in which, as he was told two years after the occurrence, the fever was brought from the island: “Mr. S. Powel, who lived on the east side of Plum or Water Street, being part of his wharf estate, had sent a young man as his supercargo to Barbadoes, where he died of the yellow fever. His clothing and bedding had been put in a chest and sent back, many months afterwards, at the request of the lad's friends, who lived in North Wales. The young man's father, mother and aunt came to Mr. Powel's for the chest, one day, whilst Mr. Hatton was drinking a glass of wine with Mr. Powel, who, calling his cooper to come and open the chest, that the parties might see what it contained, asked Mr. Hatton to go over the alley with him to the store where the chest lay. The chest was opened, and the articles looked over in the presence of Mr. Powel, Mr. Hatton, the three persons from North Wales, the cooper, and a boy of Mr. Powel; who all of them sickened and died of yellow fever within a few days. Andrew Hamilton, brother of our late Governor Hamilton, with about forty others of those in the neighborhood of the bridge, died of the same fever.”¹

As may be perceived, the belief rests on the testimony of unprofessional individuals, whose pursuits and studies did not qualify them to form correct opinions in matters of the kind; to whom, as a matter of course, the idea of exotic origin was the most natural, and who could not conceive it possible to assign a home cause to a disease resembling one prevailing in tropical climates with which they were

¹ Letter of Thom. Willing to Dr. Sam. Griffiths—Ad. Facts, pp. 9, 10.

unacquainted, and which, while it spread desolation in all directions, was regarded as contagious by their medical advisers. The principal author cited in this case relates what was told to him two years after the close of the epidemic; but his account was written in 1802, or more than half a century after that event; while Mr. Chew's letter is dated 1805, consequently 58 years after the period in question.

1762. The importation of the fever of 1762 does not rest on better authority. Of this epidemic we are told that a ship from the W. Indies came to the Sugar-house wharf below Cedar Street; three men were landed from her and died of the fever, and communicated it to the neighbourhood.¹ Another authority, Dr. Redman,² who was in extensive practice at the time, and at the date of his communication presided over the College of Physicians, informs us that a sick sailor from on board a vessel from the Havana, where the fever then raged, was brought privately after night to a court near the new market, and was received at the house of one Leadbutter. He there died, and was secretly buried. "Leadbutter, with most of his family and many others in that court, soon after fell a sacrifice to the distemper; and from thence it spread rapidly, first affecting the houses nearest adjoining in Front and Pine Streets."

As the reader may recollect, the only testimonies in favour of this fact are Mr. Willing, a highly respectable merchant of this city, and Dr. Redman. From their accounts the fever would seem to have been introduced at two different points—first, at the Sugar-house wharf below Cedar Street, by three men who landed from a West India vessel, and who, dying of the disease, communicated it to the neighbourhood; secondly, by a sick sailor from on board a vessel from the Havana who was brought privately after night to a court near the new market, consequently above Cedar Street, and at some distance from the before-mentioned wharf, and who, in like manner, communicated the disease to the neighbourhood, whence it spread in various directions.

Mr. W.'s testimony is of no account in a case of the kind. It is only a reminiscence of popular reports or surmises of forty years back—his letter to Dr. Griffiths, in which this is mentioned, being dated July 21, 1802.

As regards Dr. Redman's, it was made thirty-one years after the occurrence he records, at a period when, as the author says, "his memory was much impaired by age and infirmities." Without impugning the motives and sincerity of Dr. Redman, we are justified in rejecting his statement, founded as it is on the faint recollections of an aged physician—whose mind, according to his own showing, was in no condition to afford accurate information on matters of the kind; and who, probably, had no time to make an attentive study of the laws of infection and contagion. The experience of modern times has led us to be sceptical as to accounts of the introduction and diffusion of the fever from causes such as those he assigns.

It may be remarked, in addition, that Dr. Redman, two months after the date of his communication to the College, dissented from the report of that body to the governor, in which it was declared that no instance, as far as they knew, had ever occurred of the disease called the yellow fever having been generated in this city or in any other part of the U. States; "but that there had been frequent instances of its having been imported."³

1793. Turn we now to 1793: We find that the exotic origin of the disease on this memorable occasion is endeavoured to be established on the following grounds:—

¹ Willing, 10.

² Ad. Facts, p. 21.

³ Rush on Fever of 1793, p. 146.

1. The yellow fever existed in several of the West India Islands a long time before its appearance here.

2. Various vessels from those islands arrived here in July.

3. Scarcely any precautions were used to guard against the disorder.

4. A respectable citizen of Philadelphia, supercargo of one of those vessels, saw in July six or seven persons sick of this fever on board a brig at Cape François bound for our port.

5. A vessel from Cape François, which arrived here in July, lost several of her people with this fever on her passage.

6. A person from Cape François died of this fever at Marcus Hook, and another at Chester.

7. The vessels in which those persons arrived, and which were infected with the effluvia of the sick and dead, came freely to our wharves, and particularly to the very one where the disorder made its first appearance.

8. Persons sick of the yellow fever have been landed in our city from vessels arrived from the West Indies.

9. Dead bodies have been seen deposited secretly on board some of those vessels.

10. There is the strongest reason to believe, that the beds and bedding of the sick and dead were not destroyed; but, on the contrary, brought into our city.

11. This disorder had every characteristic symptom that marked it on former occasions, when its importation was unquestioned.

Lastly. Of all the reasons advanced to support the opinion of its having been generated here, the only one that has even the appearance of plausibility, viz: the influence of a tropical season, such as we had last summer, is unanswerably refuted by the concurring testimony of Lind, Lining, Warren, and Bruce, who, in the most unequivocal manner, have declared that it does not depend on the weather.

It cannot escape the notice of the reader, that on grounds so vaguely adduced no one can feel disposed, at this day, to decide in favour of importation. Because the yellow fever is said to have existed in several of the West India Islands—because vessels from those islands arrived in July—because little precaution was used to guard against the fever—because some one saw some sick which he thought affected with the fever on board of those vessels at Cape François—because a vessel was said to have lost several, and a person was said to have been landed at Marcus Hook, where he died—and again, because it is averred that the vessel so infected came to the city; that persons sick were said to have been landed; that bedding was not destroyed, &c. &c.—therefore, we are to conclude that the fever did not originate in the city, but was imported from abroad—especially as Lind, Lining, Warren, and Bruce declare that it does not depend on the weather.

But the charge of vagueness is not the only one that can be urged against the history of the origin of this epidemic as made out by contagionists; for many of the facts adduced in support of the exotic origin of the disease are found not to be true—while some are not to be discovered in the more detailed relations we have of the epidemic.

1. Admitting, for the sake of argument, that the fever is communicable, and could have been brought from the West Indies, it may be doubted whether it came from Cape François. It is not certain that the fever existed there at the time. The testimony of the respectable supercargo mentioned above has no weight, for who will affirm he could decide on points of the kind? The same may be said of the statement about persons who died on board during the passage or were landed at Marcus Hook, &c. The authorities on which these statements are made are either

not given or are unprofessional, or, if professional, unskilled in the diagnosis of the disease. The loss at sea is perhaps a mere fable—and the disease under which persons landed at Marcus Hook, &c., laboured may have been other than yellow fever.

Carey, in order to show that the fever prevailed in several West India Islands, gives sundry extracts from newspapers, showing that it prevailed at Grenada, Antigua, Dominica. That it did so at Barbadoes, we all know from the writings of Chisholm, Clark, &c. But while these colonics were suffering from the fever, I cannot find that the true yellow fever—the *maladie de Siam*—that form of fever which the supporters of importation regard, right or wrong matters little now, as the true disease, and the only form of it possessing, according to them, a contagious character, and thereby susceptible of importation, prevailed at St. Domingo at the time of departure of the vessels supposed to have brought it to our shores.

2. Twelve days after the arrival of one of the culprit vessels—the *Amelia*—the disease had already assumed the garb of an epidemic. Her crew and passengers were in health at the time of arrival, and only sickened while she lay in the infected district.

3. The other accused vessel—the *Sans Culotte*—came to the wharf on the 29th of July, after having remained at anchor in the stream for some days; and was removed to Kensington on the 5th of August, on which day there were already several cases of the fever in the city. One of them was taken on the 2d and another on the 3d—or four and five days after the vessel had come to the wharf. Neither of them had communicated with her, while those who were on board were not attacked.

4. It is admitted by the advocates of importation that much uncertainty exists as to the vessel which was the channel of the introduction of the disease. Carey, in his 5th and last edition, published in 1830, acknowledges the existence of this difficulty (p. 68), and the others are constantly vibrating between the *Amelia* and the *Sans Culotte*, from which we may presume that neither was guilty of the mischief imputed to it.

5. Another of the accused vessels came from Nantes and a fourth from Glasgow, whence the disease could not very well be derived. Another again—the brig *Mary*—arrived from the West Indies. But she had no fever on board, had left none behind, and reached the city after it had broken out.

6. At Kensington, where the fever was said to have been introduced, numerous cases had occurred in the practice of Dr. Say *prior* to the arrival of the suspected vessel.

So unsatisfactory, indeed, was the account of the importation of the fever on the occasion in question, that Dr. Hutchinson, the inspector of sickly vessels, though a firm contagionist, says, in a communication dated August 27: “It does not appear to be an imported disease, for I have heard of no foreigners or sailors that have hitherto been infected; nor has it been found in any lodging houses; but it is principally confined to the inhabitants of Water Street, and such as have done business or had considerable intercourse with that part of the city.”

1797. Passing over the epidemic of 1794, as to the importation of which nothing can be made out, inasmuch as the vessel accused arrived after cases had occurred,¹ and proceeding to that of 1797, we find that the introduction of the disease in Kensington (then an adjoining village, but now constituting a part of the city) through means of the ship *Hind*, which was reported by the College and believed at the time to have been the channel of communication, cannot bear scrutiny.

¹ Rush, iii. 218.

The case, as related by the contagionists, ran thus: That year the disease made its appearance in the latter end of July, in the neighbourhood of a wharf below Pine Street, where three vessels lay. One of these had been in port all summer. Another, called the *Iris*, from Oporto with wine, took her station at the wharf on the 21st of July, while the third, called the *Arethusa*, arrived from the Havana on the 24th. The last mentioned vessel had been employed in transporting slaves from Jamaica to the Havana about the latter end of June. The carpenter and one of the mariners died with symptoms of the yellow fever on board during her voyage, according to the deposition of Mr. Kingston and a letter from Mr. Fitch, two respectable merchants who were passengers on board of the vessel to the Havana. The pilot, James Mulford, was attacked with fever on board the *Arethusa* on the night of the 23d near the Marine Hospital, and was taken to his lodgings immediately after his arrival in the city. On the 29th of July the mate and cook of the brig *Iris*, the vessel from Oporto, which lay in contact with the *Arethusa*, were attacked with fever. Three more of the same crew were attacked three days after the mate and cook. G. Latimer, who resided at Pine Street wharf, was attacked on the 29th; his servant man and maid a few days after. Four other persons, employed in stores near the wharf, were attacked on the 1st or 2d of August. Captain Gowan, who conducted the *Arethusa* down to the fort on the 21st of August, was attacked with the fever five days after. John Larchan, who worked on board the *Iris*, and lived near the wharf, was attacked on the 3d of August, and in the course of the week several persons were taken ill in the same neighbourhood.¹ "In this manner the disease continued to spread, so that by the middle of August, or within three weeks from the arrival of the *Arethusa*, above ten persons had died, who either lived or were engaged in business within 300 yards of the *Arethusa*, and this at a time when the other parts of the city were so healthy, that it is probable all the other deaths which occurred in it were not equal in number to those which occurred in this small district. After this the disease gradually extended itself to Southwark, and at the same time became thinly scattered through the city, where its destructive effects are but too well known."²

The College of Physicians, while attributing the introduction of the fever of 1797 to the ship *Arethusa*, were, as just seen, of opinion that another vessel, the *Hind*, had contributed to the same result. The *Hind* sailed about the beginning of July from Port au Prince, bound to Philadelphia with a cargo of sugar and coffee and 43 passengers. The vessel touched at Cape St. Nicholas Mole, where the passengers went occasionally ashore. She left the latter place between the 12th and 15th of July, and arrived at Philadelphia after a passage of 20 or 21 days. During this time some of the passengers were affected with fever, and even after the arrival of the vessel, about the 13th and 14th of August, several were sent to the Marine Hospital with what were supposed to be symptoms of the yellow fever.³ The introduction of the fever in Kensington was due to communication with the *Hind*.

To ascertain the truth in this matter, the Academy of Medicine set on foot a thorough investigation. The first case which occurred was a Mr. John Bruster, who was said to have derived the disease on board the *Hind*, and from whom the disease was supposed to have spread. "It is true," says the Academy, "that Mr. Bruster was at least among the first (if not, indeed, himself the very first) who was

¹ Currie on Bilious Fever, pp. 213, 214.

² Narrative of facts, &c., published by College—Rush on Fever of 1797, p. 65.

³ Facts relative to the sickly state of the ship *Hind*—Rush, Fever of 1797, p. 66.

attacked by this disease in Kensington, in the summer of 1797; but that he could not possibly have derived his illness from any intercourse with the ship *Hind* is a truth unequivocally established by the following document," prepared by Dr. Redman Coxe:—¹

1. The *Hind* came up to the fort on the 2d of August. It reached the city on the 3d, and was entered at the Health Office on the 4th. Bruster could not have taken it in the *Hind*, for he died on the 2d, after an illness of four days and four hours. This brings his attack back to the 29th of July—four days before the arrival of the ship at the fort. Besides, it is proved by the affidavit of Michael Lynn, that Bruster did not go on board of any vessel in a voyage down the river to Reedy Island.

2. The next patient, W. Reed, died on the 5th, after seven days' illness—this brings the commencement of the disease to the 29th of July, *i. e.* four days before the arrival of the *Hind*.

3. No cause can be assigned for the former case, but exposure to exhalations down the river, and imprudent exposure by sleeping upon the wet deck; and for the second local causes of infection, which every year exist in minor force and produce remittent and intermittent fevers, or one of kindred nature.

4. Other cases occurred in Kensington which could not be traced to the ship, or to intercourse with the sick.

5. The disease occurred in a straggling manner, which renders it more probable that it originated from local sources, than that it was introduced and spread through means of contagion.

6. Some cases were evidently traceable to emanations from the hold of the ship *Huldah*, which was in a most offensive state. But the ship *Huldah* was from Hamburgh, not from tropical climates; she could not, therefore, introduce the yellow fever already formed—but acted probably as an additional local cause.

Dr. Coxe, whatever be his opinion now, was then a contagionist. Nevertheless, he saw little on that occasion to justify an adherence to that belief, for he says: "The disease *possibly*, in some few cases, spread by contagion. King, a coffin maker, who assisted in putting the dead into their coffins, may have derived his disease, of which he died, from this source." But with characteristic good sense, Dr. Coxe adds: "It is, however, problematical; for he was, with others, exposed to those causes which produced it in them." (90.)

As regards the agency of the *Arethusa*, it may be remarked, first, that the island of Jamaica was healthy at the time the *Arethusa* sailed. This appears from the answers given by the captain of the said ship to the official interrogatories filed in the Health Office relative to this subject, and from those of Capt. Henry Latimer, of the brig *Maria*, who sailed from the above port about the same day. (70.)

2. We have no proof of two cases having occurred on the passage to the Havana. The statement of their occurrence is made on the authority of persons unconnected with the profession. Even if the disease had been yellow fever, it could not have been contagious, for it did not spread among the passengers or crew, who amounted to seventy, all of whom arrived in good health at the Havana on the 21st of June.

But supposing the disease to have been of a contagious nature, the precautions taken after the death of the individuals attacked would have been sufficient to destroy any remains of the contagion. From Mr. Brien's deposition, it appears

¹ Proofs of the disease occurring from exhalation in Kensington, from marshy grounds and from the hold of a ship, by Dr. Cox, in *Rush, Fever of 1797*, p. 82, &c.

that "the clothing, bedding, and articles belonging to the deceased were thrown overboard, and their berths cleansed and well sprinkled with vinegar. And we are authorized further to assert, that the ship underwent such a complete cleansing while at the Havana, after landing the slaves, as prudence would dictate to a ship-master in every similar case. The ship, moreover, after lying at the Havana fourteen days, during which time all on board remained well, arrived opposite the Health Office on State Island on the 18th of July. During the whole of the passage her hatches were constantly open, whereby the most ample means for a free current of air were afforded, which could not fail to dissipate any remains of contagion which could possibly have continued after her former purifications. The ship performed five days' quarantine opposite the Health Office on State Island, during which time the bedding was every day exposed upon deck, and was once washed by a rain. The crew, moreover, remained well, except the captain, who was affected with a rheumatism, and the mate with a lax, both of whom soon recovered."¹

"As to the pilot who conducted this ship, he was attacked on the 23d of July, and allowing three days for the time he had been exposed to the contagion before his fever appeared, there will remain forty-six days from the time the ship left Kingston till her arrival in our river. From the known laws of the *contagion* of the yellow fever, and the distance of time at which it usually appears, after persons have been exposed to the contagion, the Academy conceive it scarcely possible, if any portion of contagion had been left by the before-mentioned persons, that it would have remained inactive for above forty-six days, exposed as the crew were to the exciting causes of fatigue, night watching, and the vicissitudes of the weather. The perfect freedom from disease which all on board enjoyed must, therefore, be admitted as a proof that no contagion did exist; and consequently, that the pilot and others could not have derived their disease from that source."²

The crew of the *Iris* could not have been infected by the people from the *Arethusa* passing across her. If this had been or was possible, say the Academy, it must have been in one of the three following ways: 1. By the actual sickness of the crew. 2. By the contagion blowing off their clothes in passing over the decks; or 3. By the contagion which had adhered to the timbers of the *Arethusa* being conveyed by the wind over two intermediate vessels to the *Iris*.

It is not pretended that any of the crew of the *Arethusa* were indisposed; therefore the first supposition must be rejected. They could not have infected the crew of the *Iris* in the second mode, because it is not alleged that they stopped a moment when passing over the deck. But admitting they did, it cannot be believed that a disease could be conveyed, by their clothes, to the crew of the *Iris* in the

¹ Answer of the Academy of Medicine in Rush on Fever of 1797, p. 68.

² Report of Academy to Governor—Rush on Fever of 1797, p. 71.

The pilot's attack was probably derived (and this he believed himself, as we find in a letter by Dr. Mease) from a current of cold air during the night, while sleeping in the open cabin of the ship, after a warm day, which preceded that on which the quarantine of the ship was ended.* His indisposition came on the next morning, and soon after his arrival in this city a violent fever succeeded, of a kind which we every year observe in Philadelphia from sudden changes of the weather in the summer and autumnal months, and especially from similar exposure on the river. It may be added, that he was but a few days confined, and that none of his friends who nursed him, or others who daily visited him, suffered; neither were there any precautions taken to avoid the contagion, nor the least intimation of danger given to those who constantly attended him. Under all the circumstances which have been mentioned, it is impossible to believe that the pilot's disease was derived from an imported contagion.

* Report of Academy to the Governor—Rush on Fever of 1797, p. 72.

open air, when it is well known that those clothes were worn and even washed in confined lodging-houses afterwards, and did not infect a single person in any part of the city. Lastly, "it is highly improbable that the crew of the *Iris* could have been infected by the timbers of the *Arethusa*, because we have no proof that the contagion of the yellow fever ever adheres to wood; but admitting this to be possible, we reject the probability of it, because, as we before observed, the ship had been well cleansed and freely ventilated on her voyage from the Havana to Philadelphia. We are the more disposed to ascribe the destruction of contagion, if any had existed, to the pure air of the ocean, from having so repeatedly observed the effects of country air in weakening or destroying it in the United States. The Academy are, moreover, authorized by Dr. Stewart to assert, that none of the family with whom the five men of the *Iris* boarded were infected, but that they preserved their health the whole time of the prevalence of our late epidemic."¹

As regards the *Hind*, it may be remarked, that the fever had prevailed in the city several days previous to the arrival of that vessel. None of the persons supposed to have been infected by an intercourse with her, communicated their disease to any of their visitants or attendants.

The only evidence which the Academy of Medicine possessed, respecting the sickly state of this vessel, was derived from the affidavits of three common sailors belonging to her crew; two of whom were nothing more than boys. "In opposition to the evidence delivered in these affidavits, we would here beg leave to submit to the public the affidavit of the supercargo of the *Hind*. The report, delivered in his deposition, is farther corroborated by the joint testimony of three other respectable characters on board the same vessel. From this document it will at once appear on how equivocal a foundation the College have thought proper to rest this part of their investigation, respecting the origin of the disease in question."²

1798. In regard to the origin of the epidemic of 1798, the College of Physicians remark, that in the months of June and July of that year, twenty-seven vessels arrived from Cape Nicholas Mole, Jeremie and Port au Prince, in the island of St. Domingo—places which had long been garrisoned by the British forces, and of consequence more peculiarly adapted to the generation of pestilential diseases; that these places were evacuated in great haste, and that a considerable number of American vessels which lay there were employed to transport the British garrison—circumstances in which contagion is not only generated, but for a long time retained by clothing, bedding, tents and sails, even by walls and timber—that there was ample proof that the yellow fever prevailed in those ports while the above-mentioned vessels lay there; that on the 5th of July six or seven of these vessels, having a large number of passengers on board (and of course a quantity of clothing, bedding, &c., brought off in the greatest haste), who were exposed to all those circumstances which are generally allowed to produce contagious fevers, arrived at the fort, where the passengers were detained on board twenty days—an occurrence well adapted to heighten the violence of the contagion. On their arrival at the city, after this detention, they generally lay at the wharves between Walnut and Spruce Streets—at which parts of the city the fever first appeared, about the latter end of July and beginning of August, and whence it spread to almost every other quarter.

"We are further informed," continue the College, "that on the 8th of July the

¹ Report to the Governor—Rush on Fever of 1797, pp. 73, 74.

² Report of Academy to Governor—Rush on Fever of 1797, pp. 93, 94.

armed ship *Deborah*, Capt. Edward Yard, arrived at the fort from Jeremie."¹ She buried eight persons during her stay there and return, and sent six sick to the Marine Hospital. She was one of those employed to transport troops in the West Indies. After a detention of ten days, this vessel came up to the city; but it appears that some of her crew were brought up before her arrival.² "The death of the Marquis de Rouveray immediately after her arrival, of a boy that belonged to her a few days after, of a labouring man that had worked on board her, and of Alexander Philips, who had been down to her in a boat, while she was performing quarantine, and who died on the 5th of August—the sickness of a carpenter on the 26th of July, at Achison Thompson's, next door to Philips', who had been on board the *Deborah* while unshipping her cargo—the sudden death of a sailor in the same house on the 28th of July, and the numerous cases in the neighbourhood of Philips and Thompson, a few days after, not only increased, but directed the suspicions of all that were acquainted with these circumstances, to the ship *Deborah*."

Nor is this all. The *Deborah* was removed from the wharf near Race Street to Kensington, for the purpose of undergoing repairs, on the 28th of July; and there the fever followed her.³ It appears from facts, collected by Dr. Daniel de Benneville, and communicated in writing to Dr. Griffiths,⁴ that all the cases that occurred in that village were traceable to the *Deborah*, occurring in persons who had worked, or been on board, or resided in the vicinity of where she lay.

The reader must have perceived that the reason assigned by the College of Physicians, founded upon the state of the Islands, and the employment of vessels there in the way mentioned, cannot be adduced in proof of the introduction of the fever by those vessels in this city. Nor shall I stop to inquire how far their surmises about the bedding, tents, and sails, therein contained, are correct. Enough on these subjects has been said in former parts of this work.

The first vessels accused arrived on the 5th of July. They were detained twenty days at the fort, which is situated seven miles below the city, and therefore did not reach the wharves before the 26th. There was, of course, no one sick on board; and they had, besides, been purified. No proof is furnished of there having existed fever on board during the passage, which could not have been of less than fifteen days, making forty from the time of departure from the West Indies without disease; and yet we are told that the fever first appeared in the vicinity of a spot where these vessels lay about the latter end of July, or beginning of August. This is certainly quick work for the operation of a cause which did not affect the crews of the vessels themselves.

The *Deborah* arrived at the fort on the 8th of July, and was there detained ten days. She did not reach the city at Race Street wharf before the 18th or 19th, and left there on the 28th for Kensington.⁵ On the 17th of July, "Dr. Samuel Duffield, the consulting physician, and Dr. James Hall, the resident physician, reported that they had visited and carefully examined the state of the ship *Deborah*, Capt. Yard, from Jeremie, and had found the same remarkably clean, and the people on board in perfect health. Capt. Yard informed them that he lost

¹ While she was at Jeremie, the *maladie de Siam* was raging there with great violence. (*Condie*, p. 39.)

² Facts and Observations, pp. 21, 22.

³ Currie, Fever of 1798, pp. 130, 131. *Ib.*, Fever of 1799, p. 48.

⁴ Facts, p. 45, or extracts from the letter of Dr. B. to Dr. G. See also *Condie* and *Folwell*, pp. 31, 36, 45, &c.

⁵ *Condie* and *Folwell*, Fever of 1798, p. 38.

three of his people in Hispaniola, and two on his passage home—the last on the 24th of June—besides three passengers; one was a lady just from Europe. She died on the 2d of July of a fever, which terminated in constant purging, but without vomiting, or any uncommon yellowness of the skin. A white child died on the 26th of June, and a negro child died on the 7th of July; the latter supposed to be occasioned by a wound which it had received on the head by running against one of the pikes. That the captain was of opinion that all his people who died brought on their diseases by intemperance, and by exposing themselves, when in that state, to the rain and night air; that he further declared to them, that none of the people who died, either of his crew or of his passengers, had anything like black vomiting, extraordinary yellowness of the skin—or, as far as he was able to judge, any of the striking symptoms of the yellow fever; and that the diseases were not communicated to any other persons from any of those that died; that upon considering the report of Capt. Yard, and comparing it with the present very healthy appearance of the people on board—none of whom appeared to them to have been affected by the diseases of those who died—and the number of persons now on board (95), they were of opinion that the diseases (p. 39) of which the before-mentioned persons died, were not of a contagious nature; that they were more confirmed in this opinion from the state of the four persons belonging to that ship who were then in the Marine Hospital, none of whom exhibited any symptoms whatever of a contagious or malignant nature; that they were also of opinion, that the ship was now in a perfect and wholesome state; that the people were free from every appearance of contagion; that the bedding and the clothing of all the persons who died in the vessel had either been thrown overboard, or landed at the hospital.”¹

All on board then were healthy, with the exception of those mentioned as being otherwise. Is it not natural to suppose they would have been attacked if the vessel had really contained a contagious poison sufficiently powerful to affect persons situated at a distance of several hundred feet from it, and who had not been on board?

From the testimony of Capt. Yard, no boat was alongside of the ship. Nor did any boys come on board during that time (when she arrived in the Bay); nor did any boys or men come inside during the quarantine or afterwards.² It is shown that Philips had not been on board on the sworn testimony of Abraham Snell, who went down the river with him. Again, no sick persons had been brought to Philips' house, and none from the Deborah, except Jonathan Farthingham, who had been ill and discharged (cured) from the Marine Hospital, after the ship arrived in the city, and whose disease, it must be added, is not proved to have been yellow fever.³

The Marquis de Rouveray, and the boy mentioned, took sick and died *after* they had landed; and we are surely as much justified in attributing their disease to sources of infection located on shore as to a cause of a contagious and transmissible character, lurking in the vessel. In addition, it may be proper to mention that the fever existed here *before* the arrival of the Deborah. The Academy of Medicine, in a letter to the governor, enumerated several cases of what was regarded as true yellow fever, in June and July.⁴ But if we lay the former of these cases aside as being of doubtful character, we find that by importationists

¹ Condie and Folwell, pp. 38, 39.

² Condie, p. 44.

³ See testimony of Abraham Snell and Jane McFarlin, who lived in the house with Philips, Condie and Folwell, p. 45.

⁴ Condie, Appendix, p. 20.

themselves it was admitted that a fatal case of yellow fever was reported to the Board of Health so early as the 2d of July. It may be stated also that these cases did not occur near the wharf where the fever subsequently made its appearance, and that the quarantine laws, during the whole of this period, were enforced as stringently as could be desired by contagionists of the purest water.

Kensington.—The fullest, and, to my knowledge, the only account we have of the fever on this occasion, is contained in Dr. Benneville's letter to Dr. Griffiths, published in the appendix (p. 45) of *Facts by the College of Physicians*. It is favourable to importation and contagion, and, at this distant period, with the scantiness of the materials at our command, difficult to rebut. Hence, the contagionists have so far had it all their own way in that matter. But, assuming that their statements are correct, and that none of a different character can be opposed to them, the importationists have not made out their case, as will be seen if we analyze the facts adduced, and at the same time bear in mind what was said of the nature of the localities where they occurred, and that on former occasions the fever *arose* there from the operation of local causes.

The first case had appeared on board the *Deborah* before she came up to Kensington. The individual affected was taken on the 29th of July, and died on the 3d of August. He had, therefore, been in the infected district of the city, and might have taken the disease there, and not in the vessel itself.

The next individual attacked is not said to have gone on board anywhere; but he lived in the house adjoining the one where the first case died. He does not appear to have visited the sick. Under those circumstances, judging from what experience has shown of the limited sphere of the contagion—if it exist at all—this case must be admitted to be of doubtful character. Of the other cases mentioned—twenty-five in number—nine had been on board, and sixteen had not, but resided or worked in the vicinity. Of these sixteen, about one-half worked on the wharf one hundred yards from the vessel. All that can be shown in reference to this matter is, that the vessel itself was a source of infection; but in that case the effluvia must have been wafted very far to attack individuals one hundred yards distant, and that too while other persons working in the vicinity of, or going on board the ship, were unaffected. On the 8th of August the ship returned to the city wharf, and we hear no more of her foul condition and of the contagious effluvia lurking about her; no case being positively traced to her subsequent to that period.

1799. The occurrences of 1799 are not less calculated, according to contagionists, to confirm us in the opinion of the introduction of the yellow fever from a foreign port. It is recorded that a small sloop, called the *Molly*, prize to the *Ganges*, had lain at the wharf, near the place where the first cases of the fever commenced, from the 13th of May. Her cargo, consisting chiefly of coffee, was landed and stored near the wharf early in June; her hatches were kept shut till the 21st of June, at which time they were opened, on account of the stench proceeding from her; and, according to the account of James Fisher, who had the care of her, 500 buckets of water were thrown into her, to clean and sweeten her. The bedding of some of the seamen that came in her was left on board. She was employed in trading from Turk's Island to St. Domingo at the time of her capture. Many cases were traced to this vessel. Nor is this all. Mr. Hollingsworth's lighters also landed the cargoes of several vessels from the West Indies, while under quarantine, upon or near the same wharf, at different times, in the early part of June. Not far from the sloop *Molly* lay the ship *Aimable Créole*, from the Havana, where the disease pre-

vailed extensively at the time of her departure ;¹ and it is notorious that the health laws were repeatedly violated by persons coming up from vessels under quarantine in the night, and returning on board before morning.

The first cases, in the order of time, that occurred in the neighbourhood of the still-house (Southwark), were traced to the *Eliza*. They were two boys belonging to her. Both were attacked on the 24th of June, and sent down to the Marine Hospital, where one of them died. The owner of the still-house wharf was taken ill on the 26th of the same month. James Arthur, Brady, and McClary worked in the loft of the still-house. Arthur was taken ill on the 29th of June, and died with yellow fever. Brady and McClary were taken ill a few days later. The former died.² "At the time the *Eliza* came to the still-house wharf, which was on the 19th of June, two vessels lately arrived from the West Indies lay at it, the one a schooner, called the *Minerva*, on the 9th, the other a brigantine, called the *Abigail*, on the 17th of June. These being nearest the shore, the *Eliza* took her station alongside of them. Mr. Green, in a small sloop, with naval stores from North Carolina, came to the same wharf before the *Eliza* left it. The vessel commanded by Mr. Green left the wharf on the 23d or 24th of June, and proceeded on her homeward voyage, but was forced, on account of sickness which had seized himself and crew, to put into Milford, on Mushmelon Creek, in Delaware State," "where the master, one hand, and a passenger died."³

Whoever will read attentively the above account of the rise and progress of the epidemic of 1799, derived from Dr. Currie, and the avowed object of which was to prove the exotic origin and contagious character of the disease, will at once perceive that the object the author had in view was not attained. Dr. Caldwell, in reference to this season, says: "Neither at the commencement of our calamity, nor at any succeeding period of its progress, did a single vessel appear which, after the slightest examination, either credulity could be persuaded to regard as the vehicle of human contagion, or even prejudice itself dare charge with having imported the seeds of the disease." "In proof of the fallacy and confusion of public report on this subject, we may further observe that scarcely any two individuals attached suspicion to the same vessel. Some spoke of the disease having been introduced from Leghorn, some from Jamaica, some from the Havana, some from Surinam, and some from Hamburg. In short, there was scarcely a vessel from a foreign port, however healthy, and in whatever latitude the port might be, that was not looked on with an eye of suspicion."⁴

1. One of the first cases is said to have gone on board a vessel from Curaçoa. He came from the quarantine-ground on the 15th of June, and was attacked on the 17th. But no proof is given of (and, indeed, nothing said about) there being any one affected with fever on board of that vessel. Besides, two cases occurred in the vicinity—one on the 15th or 16th, the other on the 17th—in persons who had not gone on board of, or approached, the vessel.

2. As to the sloop *Molly*, which had lain at the neighbouring wharf since the 13th of May, she may have been in a foul state; but nothing shows that she could, at so early a period, have brought the fever, inasmuch as it is acknowledged that, at the time of her capture by the *Ganges*, there was no one sick on board, and that no one became sick while navigating her into port. There is also no proof that fever existed at Turk's Island.

3. As to the *Aimable Créole*, from the Havana, nothing can be made out of her ;

¹ Currie, *Fever of 1799*, pp. 6, 19.

² *Ib.*, p. 8.

³ *Ib.*, p. 9.

⁴ *Medical Memoirs* (1801), pp. 88, 89.

and Dr. Currie confesses that, respecting her, he has not been able to obtain any satisfactory information (p. 7). All that is said is that the fever prevailed at the Havana before it appeared in Philadelphia. This is stated on the authority of a letter. We also learn that goods were frequently landed from this sickly port, by the lighters (p. 19). Nor can I presume that more stress will be laid on the reported fact that "Mr. Hollingsworth's lighters also landed the cargoes of several vessels from the West Indies, while under quarantine, upon or near the same wharf, at different times, in the early part of June" (pp. 7, 8).

4. Of the *Snow*, from Hamburg, mentioned by Dr. Currie, nothing need be said; and our readers will not attach much importance to the agency of the *Eliza*, upon which so much has been said in regard to the importation of the fever, inasmuch as she came from Leghorn, where the disease did not exist at the time, and had as yet never occurred. Even were it otherwise, the disease could not have broken out at Leghorn early enough to allow the crew or effects of that vessel to communicate a contagious poison in this city in June. The *Eliza* arrived at the still-house on the 19th of that month. Allowing her a passage of forty days, she must have left Leghorn about the first week of May; and we know that the yellow fever of the South of Europe has seldom, if ever, commenced before August or September.

5. The vague assertion that the quarantine was repeatedly violated, by persons coming up in the night from vessels at the Lazaretto, and returning on board before morning (p. 19), affords no proof, even supposing such infringements harmful, which is not likely to be the case; for the disease, when once established, is not communicated by the sick from the infected district to other parts of the city, and there is no reason to believe that it would more readily be so by individuals coming from a ship than from a house or sick-room situated in a contaminated street.

6. The Board of Health, though possessing ample means of information, did not seem to believe in all the above statements respecting the importation of the disease, for, in a communication to the Board of Health of Baltimore, dated July 10, in allusion to the reports afloat, they say: "They originated from several persons in one square of the city having become sick—persons who had no acquaintance or connection with each other; each of whose indisposition can be attributed to distinct and different causes sufficient to have produced that effect; causes which would have operated similarly in every country and in situations equally warm. Their families, nurses, and attendants continue well."¹

7. One or two vessels were charged with having infected places at the distance of several squares from where they lay, while both they themselves and their immediate vicinity were in perfect health.²

8. Not a case of yellow fever appeared at the Lazaretto till after the commencement of its ravages in Philadelphia; nor did any one, suspected to have kept up the illicit intercourse with the shipping during their performance of quarantine, suffer from such conduct an attack of this disease (p. 90).

9. Throughout the whole of the season, they had not at the Lazaretto a single case of yellow fever from on board an inward-bound vessel. Every case of this disease, that appeared on the sick list of the establishment, was sent to them from Philadelphia, except one, which came from Port Elizabeth, in New Jersey (p. 91).

The most that can be said respecting these vessels is, some of them may themselves have acted as local sources of infection. Such was the case with the *Molly* (or *Mary*), the agency of which, in this respect, has already been adverted to (vol.

¹ Currie, p. 17.

² Caldwell, p. 89.

ii. p. 433). But in this we can discover nothing like contagion and importation, in the sense applied to these terms by contagionists.

1805. In his account of the rise and progress of the fever, in 1805, published by order of the College of Physicians,¹ Dr. Currie informs us that a young man of the name of Peter Young, and two boys, in the service of Samuel Crisman, were the first attacked. Peter Young, and one of the boys, named Tobias Smith, admitted that they had been to the quarantine station a few days before they were taken ill. Crisman also stated "that Peter Young, and more than twenty other persons, went with him down the river to Thompson's Point, on the Jersey shore, below the Lazaretto, the Sunday before he and the boy went down." (*Currie*, p. 86.)

According to Crisman's account, a few days before the sickness occurred in his house, three persons—who had deserted from one of the infected vessels at the Lazaretto—arrived at Catharine Street wharf, near which he resided, with a boat they had stolen from a shallop which lay a short distance from the vessel.² There were, at this time, at the Lazaretto, several vessels from different parts of the West India Islands, particularly from Havana, Martinique, St. Domingo, and St. Thomas. They were there performing quarantine, on account of having had sickness and deaths by the yellow fever on board.³

The second boy in the service of Crisman was taken sick the day after Peter Young. On the 2d of August, six days after the attack of the latter, another case occurred, which, like those that followed for some time after, were traced to communication, or close approximation with the former. From that time, the fever extended.

Such is the account we have of the supposed origin of the epidemic of 1805. On examination, we find when the fever broke out there was no infected or sickly vessel riding quarantine at the Lazaretto.

The only one that had arrived previous to this period was the schooner *Nancy*, from the city of Santo Domingo. She arrived on the 12th July, nearly two weeks before the appearance of the fever. "The captain himself and one seaman were the only persons sick on board, both of whom were removed, immediately on her arrival, to the hospital at the Lazaretto. The vessel was then cleansed and purified agreeably to the directions of the Board of Health." (*Caldwell*, p. 55.)

The mate and part of the crew that remained and slept on board during the detention of the vessel at the Lazaretto, continued in health; and some of the members of the Board of Health were themselves on board of her, and even passed through and examined all the most suspicious parts of her, without sustaining the smallest injury to themselves, or their families or friends at home (p. 56). This they did several times, and it is scarcely probable that the first three cases should have been attacked for being on board but a moment; while the five members of the Board of Health who went down several times, escaped—they could have "no charter of exemption" (p. 56).

Peter Young, who was sent down to the Lazaretto Hospital, "declared repeatedly to the officers of the Lazaretto, with the solemnity of a man conscious that he lay on his death-bed, that, previous to his sickness, he had never had the slightest intercourse either with the Lazaretto, or with any vessel lying at quarantine. Tobias Smith, the surviving patient, when examined on oath by certain members of the Board of Health, answered precisely to the same purport" (p. 57). He declared the same to Dr. Caldwell and others. His master declared, that for several weeks previous to his sickness, he had not absented himself from home

¹ Additional Facts, p. 85.

² *Ib.*, p. 87.

³ *Ib.*, p. 89.

long enough to have gone so far by water or land (p. 57). The mate, J. Williams (a man of good character), and a seaman who remained constantly aboard at quarantine, declared on oath that during the time of said quarantine, no person from Philadelphia, or elsewhere, except those concerned in the administration of the health laws, was either on board, alongside of, or even near to the vessel. So declared also Dr. Dorsey, the Lazaretto physician. John Davis sickened on 26th July, at Catharine Street wharf, *before* Young and Smith, and died shortly afterwards in the Almshouse with black vomit, yellow skin, and other highly malignant symptoms. He was by occupation a dealer in oysters, and had positively not been at the Lazaretto previously to his attack (p. 58). The subsequent cases that occurred in the families of Caleb Bickham and Captain Hosey, had not had the slightest intercourse with the families of John North, in whose house Davis resided, or of Samuel Crisman, when the disease broke out.

1. The account of the three deserters with the boat from an infected vessel, mentioned by Dr. Currie, on the statement of Crisman, rests on that statement alone. Besides, admitting that it be true, they communicated with no one.

2. The boys who went to the quarantine ground had visited *no* vessel there.

3. Indeed, the persons first attacked had, in all probability, not been actually at the Lazaretto. They could, therefore, not have there taken the disease. If they did go down to the Lazaretto, but did not visit the ships from the West Indies, the disease, or the contagious germs (supposing these to exist), must have been very rife on board, and very virulent, indeed, to have attacked persons on the water at some distance. The notion is irreconcilable with the views of contagionists generally and of Dr. Currie in particular, seeing that, according to most of them, the contagion is operative only in a foul atmosphere. And surely, the atmosphere in an open boat on the river is as pure as we could desire. Would the smallpox itself have spread to such individuals?

4. The infection of such individuals is incompatible with the exemption of the crews and officers, and attendants of the Lazaretto, among whom the fever was not found to extend. The same objection might be urged, had the boys actually gone on board; which, as we have seen, was not the case.

5. The excursion of Peter Young, and his twenty companions, down the river, cannot serve the purposes of the importationists; for Thompson's Point being on the Jersey shore, below the Lazaretto, the contagion—if contagion there be—could not have reached them. Crisman solemnly declared that none of them went near any vessel under quarantine that day (p. 87).

6. Persons subsequently attacked had had no intercourse with those primarily affected, excepting a woman who is *said* to have been present when the first patients, with their bedding, were taken away to the hospital. They may have walked in the neighbourhood, or lived near them—but they did not communicate with them. The contagion could not, any more than on former occasions, pass through walls—and we learn from Dr. Currie himself, that there is no danger in the streets, be the patients in the houses ever so numerous. (*Fever of 1798*, pp. 67, 68.)

7. The disease that year, as in others, did not manifest contagious properties, and did not extend beyond a limited district—even where the air could not be supposed to be pure—and if such was the case, we are at a loss to understand how it could be communicated from the Lazaretto by the boys mentioned.

8. In some, even many, cases that year, the disease could be reasonably attributed to nothing but local causes, existing in a spot of limited extent; and as all the cases were exposed to the same causes, and breathed the same polluted air, it

is more consistent with the rules of sound logic to attribute them all to the action of those morbid influences than to exposure to the Lazaretto (supposing there existed infection there), or to communication with patients labouring under the disease.

1819. I cannot find in the publications of the day, that the fever of 1819 was, by the few professional contagionists and importationists that remained among us, attributed to a foreign source, or to intercourse with other cities of the Union where the like disease prevailed. If it were so referred, the declaration of an eminent physician, the late Dr. Emlen, whose opportunities of information were ample, and who was as unlikely as anyone among us to allow himself to be biassed in his conclusions by preconceived notions—on a subject, particularly, which he had always viewed with perfect impartiality—will serve to satisfy the reader of the groundlessness of the opinion: “Having been a member of the Board of Health of this city in the year 1819, when the yellow fever made its appearance in several situations along the wharves of the Delaware, in conjunction with the President of the Board, it became my duty to inspect the premises of the sick and to make inquiry into the probable source of the disease. Acting at the time as Secretary to the Board, and during the quarantine season receiving daily reports by mail from the Lazaretto physician, of all vessels detained, as well as those permitted to pass to the city, I was already prepared to decide with some confidence as to the probability of the disease having originated from vessels which had arrived from a foreign port. I did not, indeed, hear of any vessel whatever being so suspected, and I am able to say with certainty as respects the first cases of the disease, which occurred in the ferry tavern on the upper side of Market Street, and two fatal cases which occurred in the buildings contiguous on each side, that there was not at the time, nor had been from the commencement of quarantine, any vessel near them from the West Indies, or any sickly port; and I had the assurance from the sick and their friends, that they had not been on board of any foreign vessel for some months. Not long after the occurrence of these cases, others were reported, some of which were in situations remote from each other, but all near to the Delaware, amounting to fifteen or twenty patients, and in no instance which came to the knowledge of the Board, was there any vessel suspected as the source of the disease.” “The Board (it is added in a note), in a reply to inquiries from the Board of Health of New York, stated that ‘all the sick mentioned were residents of this city; and that the disease had not been traced to any vessel, or any stranger arriving from abroad.’”¹

The fact that all the cases, or, if not *all*, at least the original ones, were residents, is important; for it is a strong indication that the disease was not communicated by any one from other towns infected. But, besides this, it has not been shown that they had communicated with any one from those places, and who could have brought the poison about them, without themselves being affected. It was not shown they had handled, touched, or been near clothes, merchandise, or other articles supposed capable of contamination, and brought from some infected locality; and lastly, it is not pretended that any of those attacked had themselves been in any other place in the United States where the fever prevailed, and had thence brought it here.

The fever, then—so far as human testimony can avail—was of a strictly local origin.

1820. Contagionists aver that facts sufficient may be collected to establish the exotic origin and contagious character of the disease which prevailed in Philadel-

¹ Emlen, N. A. Med. and Surg. Journ., v. 321-22.

phia during the summer and autumn of the year 1820. The first case reported was John Hays, a sailor, living at No. 168 North Water Street. He had served on board the ship General Wade Hampton, and had arrived about two weeks previous to his illness from Charleston, S. C. He was, probably, attacked on the 19th or 20th of July, for when first reported to the Board of Health, on the 24th, he was already in a dying state. There is every reason to think that this man took the fever from visiting the brig Susan, which was moored at a wharf a little south of Race. This vessel had left St. Jago de Cuba on the 7th of June, and arrived at the quarantine station July 2d, after a passage of twenty-six days. She lost two of her crew at St. Jago; one on the 12th and the other on the 20th of May.

On the 15th of June, Mr. Geisse, a passenger, died on board from malignant fever. He was taken sick the day after sailing, which was the 8th. His bedding and clothing were thrown overboard, and the cabin was cleansed, and sprinkled with Cologne water. After the arrival of the vessel at the Lazaretto, she was, by order of the Board of Health, cleansed and purified; but whether the process employed for that purpose, or the precautions taken at sea, after the death of Mr. Geisse, were of a nature to insure perfect safety may admit of doubt. The Susan came to the city on the 11th July, eight or nine days before the occurrence of the first case. She lay at the place before mentioned, there discharged her cargo of sugar and molasses, and was removed somewhere else on the 14th. Hays, when visited, could give no information as to his movements prior to his attack; but as he had been discharged from his ship about the time the Susan arrived at the wharf, and had been out constantly along the wharves for employment, it is but reasonable to infer that he may have stopped to inquire about this vessel.

The next case was J. Jackson, residing at a short distance from the suspected vessel. This man had worked in a neighbouring sail-loft, in which one of the sails of the Susan had been placed for repairs. From that time the fever extended among persons residing or working in the vicinity of the wharf, or who approached or attended on those first attacked. It is admitted, that no proof exists of the individuals first attacked having been on board the Susan; but, on the other hand, there is no proof that they had *not* been on board. "The fact that a number of persons had visited this vessel with impunity, is no decision of any point in the case, for the like has often occurred before, not only in the case of sickly ships, but in all circumstances of the most contagious diseases; the evidence is negative." "No man on earth can affirm, that Hays or Jackson, or both of them, and others that took the fever, had not been on board the Susan; and from all we know of the conduct of such persons, on the arrival of a vessel at the wharf, reason, experience, and the result, justify the inference that they had been on board;" while those that did not, if any such there were, may well be supposed to have taken the disease from the sick, in the close vicinity of whom they were placed.

The progress of the fever was arrested near Race Street wharf; but shortly after this new cases appeared at the foot of Walnut Street. The first three were, Mr. South, Mr. Anesly, and Mr. Forsyth. They were taken on the 6th of August, four days after the last cases near Race Street. The first two had been, a short time before, in the infected district to get sugar. Forsyth's case could not be traced beyond his immediate vicinity; still there is no evidence that he was not in that district. The disease near the foot of Walnut may reasonably be traced to those cases which were introduced there from the infected district near Race Street.

A suspicion may rest on the sloop Isabella from Baltimore—and also on the

brig Martha—three of whose men were attacked and taken to the hospital. Some have referred the disease to the sloop Heetor, the greater part of whose crew were said to have died of yellow fever in the West Indies on her passage home. She put into Wilmington, Del., to avoid quarantine ; but subsequently came up to the city and lay at Walnut Street wharf. The case or cases in Second Street near Walnut originated in the same way, as they were too far from the local causes to which the disease has been ascribed, to be due to them. The disease was evidently introduced into Lætitia Court by the first case, Mrs. Townly, who resided about the corner of that court and Market Street, and had waited on Forsyth in Water near Walnut. All the other cases occurred in her vicinity. Those in Second near Shippen (Southwark), may be traced to a woman, Mary Cail, who had been at Arch Street wharf a few days previous to her illness, and passed along Second Street, where there was fever. There existed no other cause to account for these cases ; nor can we refer those that occurred in the Northern Liberties to any source of local infection. Those that have been pointed out as productive of that effect, had long existed without giving rise to the disease ; and besides, between them and the residence of the persons attacked, there was a considerable distance ; and in that space—directly in the direction of the prevailing wind—there was a dense population that did not suffer. It is presumable that the disease was introduced in that vicinity by Thomas Massey, who resided in Green Street, not very far from the residence of the others. He had been in Baltimore, where the disease prevailed, two weeks before sickening.

From all that precedes in relation to the epidemic in question, it is inferred that the disease was introduced by the brig Susan, and that all the cases that occurred are traceable to exposure to that vessel, or to persons already affected with the fever. In many instances, the intercourse is acknowledged to have been indirect and very distant, as they did not visit or had had no communication whatsoever with the sick in the infected district. Granting all this to be well founded, it only goes to prove that a close personal intercourse is not necessary to give the contagion the infecting quality. We never have believed that it was necessary to shake hands with persons labouring under the malignant fever of hot climates, or typhus fever, to give effect to the contagion peculiar to them.¹

Such are the facts and arguments upon which the opinion of the importation of the fever of 1820 is founded. Need I remark that the statements adduced are unsatisfactory, and in great measure conjectural ? The whole account about the source of the sickness of the first case, John Hays, who is supposed to have received the disease from exposure on board the brig Susan, from St. Jago, is of that character ; for it amounts to this—that Hays, who, it is admitted, could give no information, when first visited, as to his movements prior to his attack, may be reasonably inferred to have visited that vessel, because he had been discharged from his ship about the time the Susan reached the wharf, and he had been out constantly along the river for employment. All this may be true—but no proof of it exists—and without such proof the case is of no avail. The *onus probandi* should not be shifted on the opponents of importation. It rests with contagionists to advance their facts, and prove them such. Hence, it is not for their adversaries to show that Hays was not on board ; but for those who think that his having been there may help their cause, to demonstrate conclusively that he was, before the case can be fairly adduced as of any importance in regard to the reality of importation.

¹ Observations on Fevers, &c., read before the College of Physicians, by Elijah Griffiths, Med. Rec., xiv. 289–307.

The same remarks are applicable to the second case—Jackson. He resided at a short distance from the suspected vessel. He had, besides, worked in a neighbouring sail-loft in which one of the sails of the Susan was placed for repair. No one, say contagionists, can affirm that Jackson or Hays, or both of them, and others who took the fever, had *not* been on board the Susan, and “from all we know of the conduct of such persons on the arrival of a vessel at the wharf, reason, experience, and the results justify the inference that they had been on board.” Here, again, the *onus* is improperly placed on the shoulders of the opponents of importation. These may again call upon their adversaries to *prove* that Jackson was on board; for until this is done, the case, so far as that vessel is concerned, is of no avail. But let that pass. It is certain that, in this instance, if not in that of Hays also, the inference is incorrect; for the President of the Board of Health, in his account of the epidemic, remarks: “Jackson himself, to my repeated inquiries, always assured me he had not been on board any vessel in a short time previous to his illness.”¹ The fact of his having resided near the vessel is no proof, for it would be first necessary to demonstrate that the vessel was the source of infection—which was not done. It would be necessary, also, to explain the escape of others who were equally near the vessel, and moreover to show that the attack of the former was not due to causes independent of the presence of the vessel.

As to the agency of the sail in occasioning the disease in Jackson, a circumstance on which much stress was laid at the time, little can be made out of it. It was generally reported that Jackson had assisted in taking the sail from on board the Susan, and in carrying it to the loft. Mr. Drais, who aided in removing the sail, positively contradicts this story. Indeed, Jackson was not employed in the said loft until the 21st of July, whereas the brig left the wharf on the 14th, and the sail was taken into the loft on the 11th.

Jackson did not work on the sail at all. When he was taken sick, it lay furled in the loft, precisely in the state in which it was when brought there, and had never been touched by him. When examined by Dr. Jackson, the sail was found perfectly sweet and clean. In St. Jago it had been used as a middle awning, and was up for about fifty days exposed to all weather—then folded and thrown on top of the cargo—was brought several times on deck during the home passage to be aired, and had not been approached by the only person sick on board.² To this it may be added, that there were nine hands employed in the loft, including Jackson, and that of all these, the latter was the only one attacked. Finally, on returning to and from the loft, he passed daily those localities where the other cases principally existed.

The brig Susan arrived at the Lazaretto on the 2d of July from St. Jago, after a passage of twenty-six days. While at St. Jago, two of her crew died, one on the 12th, the other on the 20th of May; both were sick and died on shore. The diseases were said to be gravel and yellow fever.

On the 15th of June, eight days after the departure of the vessel, Mr. Geisse, a passenger (or supereargo), who had sickened the day after sailing, died of yellow fever. The bed, bedding, and clothing, which Mr. G. had used, were thrown overboard, and the cabin was cleansed and purified in the way mentioned. On her arrival at the Lazaretto, the quarantine master had the vessel again cleansed and

¹ An Account of the Yellow or Malignant Fever as it occurred in the City of Philadelphia in 1820, by Samuel Jackson, M. D., p. 35.

² See Dr. Jackson's statement (p. 35). The testimony of Messrs. Keen & Drais, the proprietors of the loft (*Jackson*, pp. 113, 114), and the examination on oath of the commander of the Susan, by Dr. Lehman, Lazaretto Physician, *ib.*, pp. 114, 115.

purified. On the 10th of July, eight days after her arrival at the Lazaretto, thirty-five after leaving St. Jago, and twenty-five after the death of Mr. G., she was permitted to proceed to the city.

“The Lazaretto physician has informed me,” says Dr. Jackson, the then President of the Board of Health, “that during his residence at the Lazaretto, twenty days from the time of a death or sickness on board of a vessel, has been considered by former boards of health a sufficient time to test the healthiness of a crew and vessel. In the instance of the *Susan*, the time was extended to twenty-five days; a longer period, the Lazaretto physician has stated, than had been deemed requisite with any vessel, under similar circumstances, for the last four years.” (*Jackson*, p. 34.)

The *Susan* reached the wharf on the 11th of July, discharged her cargo, and left it on the 14th; proceeding to the lower part of the city, where she lay during the summer. Her hold was represented by Mr. James Bell, who assisted in unloading her, as also by the health officer, as being perfectly clean, dry, and sweet. Her crew, on her arrival at the Lazaretto, consisted of eight men, none of whom were subsequently sick of the fever. And after the removal of the vessel to the lower or southern part of the city, nothing was said of her communicating the fever to any one. A stevedore and eight men were employed to discharge the cargo, none of whom became affected with fever, or any other disease; and of the owners of the vessel, and some of their friends, to the number of between twenty and thirty, who visited the brig, and of the custom-house officers and coopers employed on board of her, not one was taken sick. The number of persons who had communication with the *Susan*, and consequently were liable to have contracted the disease from her had she been an infected vessel, must have amounted to between forty and fifty; all of whom, however, remained in health. On the other hand, after the most diligent inquiry, it could not be ascertained that a single individual who was attacked in this *locality* with the fever, had been on board the *Susan*, or had directly any communication with her. (*Jackson*, pp. 34-5.)

To these facts, which are fully authenticated, we have opposed the *probability* of the first two cases having visited the incriminated vessel; and the fact of one of these having approached a furling sail belonging to her.

When the disease appeared at the foot of Walnut Street, several vessels were successively accused. As to the *Isabella*, which was at first suspected, it is clear she was innocent of the charge, inasmuch as she came from Baltimore, where the fever did *not* exist, and her crew was and had been in good health. The *Martha* was next accused, but it was found, on investigation, that she had not come up within several hundred yards of the said locality. She lay there two or three weeks, and was subsequently removed to Queen Street wharf, in Southwark—a mile from Walnut Street. Such of the sailors of the *Martha* as were affected with the fever, were subsequently ascertained to have been at Walnut Street wharf, about the time the fever broke out there, and to have slept a night in one of the taverns in that neighbourhood.

With respect to the *Hector*, we learn, from the statement of a highly respectable merchant, Mr. H. Cope, who obtained his information from Mr. Hemphill, to whom the vessel was consigned in order to be sold, that she arrived at Wilmington, Delaware, where she was owned by Mr. Baily, about the latter end of June, from Cape Henry, and discharged her cargo. She was washed out and cleansed, and, after remaining there twelve or fifteen days, took on board a quantity of corn meal, which was consigned to Messrs. Masden and Buncker, and which was discharged at their wharf. She then fell down to the first wharf below Walnut Street. None

of her crew were sick at Cape Henry, on her voyage home, or afterwards, and her original crew all returned in her.¹

As to the introduction of the fever in the upper part of the city, by a Mr. Thomas Massey, residing in Green Street, "not very far from the residence of the other cases," and who had been in Baltimore two weeks before sickening, all we have to say is that the fever did *not* prevail in Baltimore. It had done so, in an aggravated form, the year before; but during the season in question that city was free, though, unlike New York, she forbore to enforce restrictive measures.

The introduction of the disease in other parts of the city was attributed to communication with the sick. The subject appertains, therefore, to another part of our researches, and need not be dwelt upon here.

1853. After an interval of thirty-three years, during which the yellow fever did not show itself, except in a few isolated cases in 1826, the disease broke out in an epidemic form during the hot weather of the summer of 1853. The disease on this occasion was evidently due, according to importationists, to the introduction from abroad of an infectious germ. The grounds upon which the belief is founded are the following:—

1. A vessel, the barque Mandarin, left Cienfuegos, Cuba, for this port, on the 25th of June, all in good health, with a cargo of sugar, molasses, and cigars.

2. At the time of her leaving Cienfuegos, no epidemic prevailed there; but, according to the report of the captain, "a few cases of smallpox and fever existed." The crew lived on board while in port, anchored off the town, were seldom on shore, and, as far as the captain knew, none of them had visited the sick.

3. The vessel arrived at the Lazaretto, July 12, after a passage of seventeen days, her crew originally consisting of twelve men. Of these, two died of fever during the passage. The first was attacked when eight days out from Cienfuegos, July 3, with fever, and died on the 7th. The second sickened on the 4th of July, and died on the 9th.

4. At the time of arriving at the Lazaretto, the crew, numbering ten souls, were, on examination, found to be in good health. Nevertheless, it was considered prudent that the barque should be detained until thoroughly ventilated, cleansed, and fumigated. The bedding and clothing of the deceased sailors were destroyed, the vessel was whitewashed and fumigated in every part with chloride of lime, while the bedding of the crew was aired and their clothing washed. She was detained an entire day.

5. The Mandarin reached the city on the 13th of July, and came to at South Street wharf. On the 16th, she was hauled up to the lower side of the first pier below Lombard Street, where she discharged her cargo. On the 20th, seven days after her arrival, she dropped down to the lower side of the first pier above Almond Street, where she remained until the 26th. From this she was removed, by the Board of Health, to the cove below the Navy Yard; whence, on the 28th, she was remanded to the Lazaretto, in order to undergo a more rigid and thorough purification. The crew having been previously discharged, the captain and mate remained by her, sleeping on board.

6. There was no disease of a malignant type in the vicinity of where the vessel lay, "as far as has been ascertained," either before or during the time of discharging her cargo. No epidemic was prevalent in any part of the city. There appears to have been no cause for alarm until the cargo was out of the vessel, when it was noted that a very offensive smell proceeded from her hold. Subsequently,

¹ Jackson, p. 38. See Cope's letter, *ib.*, p. 115.

but not before the vessel had been removed to Almond Street wharf, the stench became intolerable, especially whenever the pumps agitated the bilge-water contained under the limber planks or flooring of the hold.

7. The first case of a suspicious character which occurred in the neighbourhood was on the 19th of July, six days after the Mandarin reached South Street wharf, three days after she was removed to the pier below Lombard Street, and the day before she left this last position for Almond Street wharf. The individual attacked drove a furniture car, and stood on the upper side of South Street. On the next day, the 20th, five new cases occurred, all in persons residing near, or frequenting many times a day, South Street wharf, or the vicinity. The next, or seventh case, was the mate of the Mandarin. He was attacked on the 21st. Other cases occurred that day and the following, all of whom were traced to South and Almond Street wharves, and other neighbouring localities. On the 23d, consequently while the Mandarin lay at Almond Street, the captain was seized with the disease. He was removed to Clement's Hotel, Delaware Avenue, between South and Lombard Streets, where he recovered.

8. From this time the fever gradually spread, but all the cases that occurred, up to the 27th of July, either resided or did business in the vicinity of South Street wharf. "Nor is it known," says Dr. Jewell, "that any case originated below or south of where the vessel had been last moored, nor has any case come within our knowledge north of Lombard Street." Subsequently, however, the disease extended, and assumed the character of an epidemic, occupying, with few exceptional cases, a locality extending from Union Street north, to Queen Street south (about 600 yards); Second Street west, and the Delaware on the east (200 yards). It did not cease before the middle of October.

We look in vain on all that precedes for facts or arguments calculated to show that the fever of 1853 was due to the importation of a contagious or infectious germ. Indeed, everything tends to prove that the disease was not imported, either already formed or otherwise, and that it was the result of agencies appertaining to the localities where it broke out and prevailed.

1. By the majority of the physicians of the city, who investigated the subject, it is admitted that the disease did not, in the whole course of its epidemic existence, manifest a contagious character. Dr. Jewell, who was the chronicler of the epidemic, and an advocate of the introduction of the disease through the instrumentality of the barque Mandarin, says that it was in no case communicated to any person visiting or engaged in attending upon the sick. Even after the close of the epidemic, when he had ample opportunities of investigating the mode of propagation of the disease, and conferring with those well qualified to decide in matters of the kind, Dr. Jewell found no reason to believe in its contagious nature. In no instance can it be shown that the disease has spread from those labouring under it. As we have already seen, this immunity was exhibited in the various hospitals of the city. In private practice, too, although numerous cases were attended away from the infected portion of the city, we have yet to learn that the disease, in a single instance, was propagated from the sick to the well, although there was an unrestrained intercourse between the patients and their immediate friends.

Such being the case—and no physician who has inquired into the matter during the aforesaid summer and autumn, and watched the progress of the disease from place to place, and the history of the individual cases, from the outbreak to the close of the epidemic, will feel disposed to refuse his sanction to the above statement—together with the absence of any contagious element on board of the vessel

during her stay at Cienfuegos, during her voyage, or at the time of her arrival; and the non-existence of any power of transmissibility of the fever from the sick after it had broken out among us, we naturally infer that if any poison was derived from the culprit vessel, and was the means of introducing the yellow fever in this city, that poison was not the product of a diseased body, in other words, a contagious virus. Dr. Jewell admits this, and even if he did not do so, the inference would be natural; for a fever which, during the whole course of its epidemic career—covering a space of full three months—did not in a single instance, and under every variety of circumstances, betray the least approach to its possessing contagious properties, can scarcely be supposed to have been derived from a contagious poison proceeding from abroad, or created on shipboard—supposing the thing possible—and left among us after the departure of the vessel which had conveyed it to our port. No, the poison supposed to have been introduced here must, and is stated to, have been the result of causes operating in the hold itself—the product of the decomposition of materials contained under the limber planks or in other parts of the vessel.

2. There is no proof—nay, there is no reason to believe—that the sailors who sickened and died on the passage—admitting that they had the yellow fever, a circumstance which has not been conclusively established—took the disease from causes existing in the vessel. It is just as likely that their sickness was due to a febrific poison inhaled by them while at Cienfuegos. We are told that, though no epidemic was prevailing at that place at the time of their departure, sporadic cases had occurred. They generally do so, at that season of the year, in all the West Indian ports, among unacclimatized strangers, and especially sailors. The cause of fever, therefore, was in operation in the town; and we know that the crew of the Mandarin were, if not *often*, at least *sometimes*, on shore, where doubtless they indulged in *sprees*, as sailors are very apt to do. What prevented those who suffered at sea from imbibing the seeds of the fever when they landed? Or, supposing them not to have landed, what prevented the febrile poison from being wafted to the ship, which was anchored off the town, and, of course, at no great distance from the latter, and certainly not beyond the reach of malarial effluvia? Such things have often occurred in tropical ports where there are no wharves or quays; where vessels are all anchored off the town; and where, unless the distance is great, or some intervening cause exists, the men are liable to receive the infection, although carefully refraining from going ashore. Add to this, that the men, as I have lately learned, slept not in the forecastle or other parts below, on account of the oppressive heat that there prevailed, but in the longboat, and that those who died were only taken below after they had sickened. Now, of all places on shipboard, the longboat is about the last where the yellow fever poison could be expected to be formed or to lurk. As well might we suppose it to be formed in the rigging.

3. The period at which those men sickened cannot be urged in disproof of the opinion here suggested; for it is not uncommon, especially when the epidemic influence is not very violent, to find the disease breaking out on the 8th and 9th day after exposure, as was the case in the instances in question, and even later. The very circumstance of the two men taking sick in such rapid succession, would lead me to infer they had imbibed the seeds of the disease before leaving Cienfuegos; for it is not probable, had the poison been formed and exhaled in the vessel, that the two cases would have occurred almost simultaneously, and that the disease would then have ceased to appear until some days after her arrival. The occurrence, of course, is possible. It appears to me, however, more probable, had the fever originated from local sources of infection existing on board, that the

cases which occurred would have done so at a longer interval from each other, or, what might be still more natural to expect, that the disease would have continued to show itself off and on during the rest of the passage. So far, however, from this occurring, no one else on board appears to have suffered from sickness; inasmuch as when the vessel arrived at the Lazaretto, and the men, ten in number, were examined by the physician, they were all found to be in good health; and the captain, before being permitted to proceed to the city, declared on oath, that no sickness, except that resulting in the death of the two seamen, had occurred during the voyage.

4. This was on the 13th of July. On the 16th, the vessel having been hauled up to the lower side of the first pier below Lombard Street, the crew was discharged, the captain and mate alone remaining. None of the men so discharged were since ascertained to have died or sickened. Now, had the Mandarin really been in an infected state, the result here adverted to could not very well have obtained. On examining the records of vessels in which the yellow fever has truly originated and prevailed, matters are usually found to assume a different course. In all, the proof of this origin is established on something more substantial than the sudden appearance of two cases, preceded and followed by the perfect healthiness of everybody else on board. Nor is this all. Had the Mandarin been a focus of infection—had she contained the seeds of the disease in her hold, we might have expected to find those who were subsequently employed on board suffering in greater or less number from the disease, instead of escaping to a man, while others at a distance were attacked. Such certainly has not been the result in instances in which the poison was undeniably formed on shipboard. Now, what has been the case in respect to the Mandarin? We are told, that none of the labourers employed in unloading her, and who must have been sufficiently exposed, in all conscience, to the action of the poison had it been elaborated and accumulated in the hold, took the disease. Furthermore, on the 26th, when the cargo had been discharged, when the hold had been opened, when the disease supposed to have been introduced by her had already broken out, when, as it is stated, the noxious emanations which had been latent in the hold and under the limbers of the vessel had an opportunity to escape from their place of confinement, when, of course, the vessel must have been saturated with them, and when cases of fever are supposed to have originated from merely sitting some hours in her vicinity while she lay at Almond Street wharf; on the 26th, I say, the Mandarin, by order of the Board of Health, was taken in charge by the health officer, who put on board a crew of five men, including the watchman. These men remained in her from that date up to the 29th of July, when she was safely anchored inside the island of Little Tinicum, opposite the Lazaretto, and placed in charge of the quarantine-master. What was the result? Did any of these men suffer from the deleterious effects of the poisoned atmosphere of this vessel? No, far from it. “Neither the health officer nor any of his men, nor did the watchman who remained on board until after her purification and return to the city, experience an hour’s sickness.” At the Lazaretto, the Mandarin remained from the 29th of July to the 2d of August; several of her limber planks were removed. “She was then scuttled and filled with water, which remained in her twenty-four hours. The holes were then plugged up and the water pumped out. Next, she was thoroughly whitewashed, and, in every part of her, chloride of lime was freely distributed.”

5. All these operations—which, let it be said in passing, are not, under conditions of atmosphere such as those that existed at the time, very generally successful in eradicating the disease from a really infected ship—require considerable expo-

sure on the part of the individuals employed in carrying them out, and yet we are told that, during this process of expurgation, "the Quarantine Master, with several of the bargemen at the station, were on board and at work in the hold, cabin, and other parts of the vessel for several hours during each day—the weather excessively hot—without any inconvenience to their health." On her return to the city, the 3d of August, she was moored at Noble Street wharf, Northern Liberties, where she remained until the close of the month; then was removed to the screw-dock, Kensington, for repairs, and on the 6th of September was taking in cargo between Arch and Race Streets, preparatory to a voyage to New Orleans. In these different localities, no cases could be traced to exposure to the atmosphere in or near the vessel. Indeed, as we are informed by Dr. Jewell, no sickness whatever followed in her track after she left Almond Street wharf on the 26th of July.

A result of this kind could scarcely be looked for had the vessel been really a focus of infection. It is one which could not be anticipated by those who have examined the history of vessels in which the fever poison originated. In such instances, the danger of infection is far greater to those who are on board, especially if employed below, than to those who remain outside. In very many cases, indeed, the danger is limited exclusively to the former; and, it is well known, that the operation of expurgation, when performed in yellow fever regions, and especially during hot weather, is one of great danger to all but Kroomen and negroes, to whom, on the coast of Africa and in the West Indies, it is exclusively assigned. In the instance of the Mandarin, however, the danger was, as it would seem, the share of those who abstained from going aboard and remained at a greater or less distance, many of whom not only did not approach her, but did not, and could not, in any possibility, see her.

6. Let it not be said that both the captain and the mate of the Mandarin, who remained and slept on board after the crew had been discharged, took the fever, of which the latter died; and that from this circumstance we are justified in concluding that the poison was generated in the hold of the vessel. These men were attacked some days *after* the disease had broken out in the vicinity; the mate on the 21st of July, and the captain on the 23d. They were not the first cases, as would probably have happened had the poison issued from the hold. They were, while attending to their duties and sleeping on board, exposed to the exhalations from the same local sources of infection which were exercising their baneful influence over the people of the neighbourhood. Indeed, they were more exposed than the generality of others who were attacked during the season, for, like some of the other early cases, they slept on board, and consequently in the focus of the local infection; and it is well known that malarial exhalations are more active and dangerous at night, and are especially liable to affect individuals exposed to them during the state of sleep.

7. Nor can we attach much importance to the circumstance that two of the early cases were those of the captain of a vessel lying in Lombard Street dock, next above the pier where the Mandarin discharged her cargo, and of the mate of another vessel close by, and that subsequently other cases occurred among the men employed in the first-mentioned vessel. There is no proof that these took the disease from the poison issuing from the Mandarin, any more than from a morbid agent originating, as it has done on former occasions, along the wharves. While they were falling sick above the place where that vessel lay, others were sickening below, and without approaching her. In visitations of the yellow fever in this city and elsewhere, the local sources of infection, though at first circumscribed within rather narrow bounds, have seldom been found to remain so beyond a

limited period. They soon cover a surface of some hundred feet, and do not attain their full limits until some time has elapsed. We may very well understand, then, that cases can have occurred almost simultaneously at South and Lombard Street wharves, without being obliged to attribute some or all of them to the Mandarin. Even if we suppose the cause to have been limited to South Street wharf, where, as we have seen, the first case and several of the succeeding ones presented themselves, we can be at no great loss to account for the cases on board the Mandarin and neighbouring vessels; for we are told, that the captain of the former, though sleeping on board of his vessel, took his meals at the Champion House, near the northwest corner of South Street and Delaware Avenue; and it is not improbable, that he as well as the other men who sickened close by, visited that same locality, and that they all perambulated the wharves at night. Captains, mates, and sailors of merchant vessels are not proverbial for their sedentary habits, and when their day's work is over are little apt to seclude themselves within the precincts of their vessels.

8. To this I may add, that little support is afforded to the opinion respecting the agency of the Mandarin in introducing the disease, by the facts that an individual from the country, who had visited the avenue on Thursday and Friday evenings (21st and 22d of July), and sat for an hour or more on the wharf at Almond Street, where that vessel lay, took the disease on the 23d, and that the captain of the brig Reform, lying on the north side of the pier above Almond Street wharf, where the Mandarin lay last, shared the same fate on the 26th; for, independently of the little probability of these persons being morbidly affected by a poison issuing from a vessel which, from the day she left Almond Street wharf (on the 26th), was fully exonerated from the charge of having done any injury to a single one of the many individuals who worked, ate, and slept on board, we find that other persons, about the same time, took the disease in places remote from the vessel, and without having been known to approach her. Thus, for example, Honora Stanton, who resided at No. 16 Little Water Street, above South, some four or five hundred feet from Almond Street wharf, was taken sick on Friday the 22d, and died on the 27th. If cases could occur at a distance from the vessel, and if, amid the large number of individuals who approached her during the time she lay at Almond Street wharf, only two took the disease, and if, besides, after she left that wharf no one sickened on board, we run no great risk in expressing a doubt as to her agency in the production of the two cases in question. The same causes that gave rise to those at a distance can well be considered sufficient to have occasioned those that occurred near Almond Street wharf, without referring the effect to a vessel, the infection of which is, to say the least, very problematical.

9. In addition, let it be here remarked that, when the limber planks of the Mandarin were removed, no mud or other collection of filth was found concealed there. The hold of the vessel had, it is true, often been, as the captain admitted, in a foul condition. It is true, also, that when the cargo was out of the vessel it was noticed that a very offensive smell proceeded from the hold; that after she dropped down to Almond Street wharf, the stench became intolerable, especially when the pumps agitated the bilge-water contained under the limber planks. And we are told, moreover, that in consequence of some defect in the pumps and of her being a tight vessel, the bilge-water could not easily be discharged, and as a consequence soon acquired a disagreeable smell. But all this does not necessarily make a sickly vessel, except under peculiar circumstances, and when carried to a greater excess than appears to have been the case in the instance of the Mandarin. All vessels that carry cargoes of sugar and molasses are more or less foul, for the time

being at least, and there are few of them whose bilge-water does not emit an offensive smell, sometimes an intolerable stench; and yet comparatively few become laboratories of febrile poison. Indeed, although under particular conditions of atmosphere, and in particular latitudes, foul ships are very apt to be unhealthy, they are far from being necessarily so. Nor is it less true that a condition of bilge-water, such as is stated in reference to the *Mandarin*, however often connected with the manifestation of disease, is not always so; cases having been adduced in which the foulest and most offensive water has proved innocuous; while disease has raged where the odour was hardly, if at all, perceptible.

10. I may further call attention to the circumstance that the period at which the first reported cases occurred must, on reflection, prove an obstacle to the idea of tracing the disease to the *Mandarin*. The reader will remember that this vessel reached South Street wharf on the evening of the 13th July; that she was removed to Lombard Street wharf on the 16th; that she there discharged her cargo and was removed to Almond Street on the 20th. It is not presumable, that before the vessel had reached Lombard Street, and the cargo had been discharged, the poison was lurking about the decks, cabin, fore-castle, or longboat. Such a thing seldom occurs even in ships more fully saturated than the *Mandarin* could possibly have been. Besides, the captain and mate, who, as experience showed, were not proof against infection, as well as the eight seamen who manned the barque, had been living on that deck, and in that cabin, fore-castle, &c., with perfect impunity.

The first cases, therefore, if they took the disease from the exhalations issuing from the vessel, did not and could not have done so while she lay at South Street wharf. The effect must have been produced when the hatches were opened, the cargo discharged, and the hold exposed. Dr. Jewell himself acknowledges the fact; for he remarks that "there appears to have been no cause for alarm until the cargo was out of the vessel, when it was noticed that a very offensive smell proceeded from her hold." Now, if we bear in mind that the operation of unloading did not commence before the 16th—the day the vessel was hauled up to Lombard Street wharf—and could not have been effected in less than twenty-four or thirty-six hours, we shall perceive that the morbid effluvium, supposed to have been generated under the limber planks of the hold from the putrescent state of the bilge-water, and which heretofore had there remained in a latent state, could not have had "an opportunity (acted upon by certain exciting causes, as heat and moisture) to disseminate itself," and poison the atmosphere in the immediate neighbourhood of the place where the barque lay, before the evening of the 17th.

If, with this fact before us, we turn to the date of the early cases, we shall find that the first occurred on the 19th, some thirty-six or forty hours only after the poison began to escape. On the next day, 20th, we have five cases. On the 21st, five more occurred. Here, then—supposing the poison to have produced its baneful effect at once on reaching its victims, which is not generally the case—we have an incubation of thirty-six or forty hours at most in one case; an incubation of sixty to sixty-five in five cases, and an incubation of from eighty-four to ninety in five more. The occurrence will appear improbable to those who have paid some attention to the subject, especially if they take into consideration the circumstance that the poison, which is supposed to have so quickly stricken down individuals situated at a greater or less distance from the vessel, was powerless as regards those employed in discharging the cargo, and who consequently were most exposed to its morbid effects. Such instances of short incubation are not usually encountered. Cases, of course, occur shortly after exposure—sometimes a few hours, or a day or two. They are mentioned in many works on the fever, and I have myself, in

this publication, referred to several of a striking character. They are, however, generally observed only in places where the atmosphere is fully saturated with the poison, and in times of violent and wide-spreading epidemics. But, even under such circumstances, they are comparatively unfrequent and exceptional. More generally, the process of incubation is of longer duration, requiring, according to some writers, from three to eight days, and in the opinion of the greater number, from five to ten. It has extended to three or more weeks, or to several months.

Now, it would be rather an extraordinary circumstance that, in the epidemic of 1853, we should have had, at the outset, and in such rapid succession, so large a number of instances of unusually short incubation, and that the disease should have attacked the first sufferer about a day and a half only after the poison could possibly have reached him, and the very next five only at the most twenty-four hours later. More consonant with our former knowledge on the subject, backed by what has been adduced against the supposition of the Mandarin having been the efficient agent in introducing the disease among us, is it to conclude that the individuals thus attacked had imbibed a poison generated elsewhere than in the ship, and which floated in the atmosphere of localities where the disease broke out and prevailed before the cargo was discharged.

11. That causes capable of producing the effect, and similar to those which have again and again occasioned the yellow fever without the agency of a really or supposed infected vessel, existed in those localities, will not be denied by any one who inquired into the subject at the time. "The docks along the Delaware line, between Lombard and Almond Streets," says Dr. Jewell (writing in September), "as usual, contain large accumulations of offensive mud and other filth." "In addition," continues the same writer, "to the prevalence of the morbid atmosphere which we have clearly shown developed itself on board the Mandarin, but not until her cargo was discharged, and which so sensibly affected individuals on approaching her when she lay at Almond Street wharf, we must not, for a moment, conceal the existing causes in the immediate vicinity of South Street wharf, sufficient to justify the supposition of their agency in the development of disease of a malignant type, when subjected to the high thermometrical influence which prevailed throughout the months of June and July. Not the least mischievous of these causes, in the production of an unhealthy atmosphere, was the outlet of the sewer into the dock at South Street Ferry, belching forth continually putrid masses of animal and vegetable filth accumulating around its mouth, and exposed at low water to the rays of the sun, exhaling streams of unwholesome and poisonous gases into the surrounding air. Besides this agent, there was a most foul wharf at the upper side of South Street; a filthy avenue between Lombard and South Streets, without any properly constructed surface drainage; numerous damp confined cellars, subject to an occasional overflow by the ebbing and flowing of the tidewater of the Delaware, and various minor causes that might properly be added to the above category, fruitful in the production of atmospherical changes injurious to health."

At a still later period (October 5), Dr. Jewell reverts to the subject, and after taking some pains to show that the disease was not sustained by an overcrowded population, he remarks: "The whole neighbourhood, however, may be considered as favourable to the production and nourishment of malarious fevers, in view of its proximity to the river docks, the open sewer at South Street wharf, the damp cellars, filthy alleys, and other local causes of disease, under such a long-continued high thermometrical atmosphere, as prevailed during the months of July, August, and September." The very poison, said to have existed on board of the Mandarin,

is admitted to have been elaborated in her hold out of materials of the kind mentioned. And surely, if such has been the case in all the instances referred to, we cannot see why they may not be regarded as capable by themselves of having produced the effect in question in 1853 on our wharves, especially when we bear in mind the atmospheric peculiarities of the season, and the great tendency manifested in most places south of this city and in tropical regions generally to the development of the disease. They certainly existed in greater abundance there than under the limber planks and in the hold of that vessel; and, if the foulness of the latter can be supposed to have been the source of the febrile poison, it remains to show why the foulness on shore could not have given rise to a like effect.

12. It is no argument against the local domestic origin of the yellow fever, and in support of the introduction of the poison through the agency of the Mandarin, to say that the disease broke out immediately abreast of the wharf where she first hauled to, and that, although there existed in the vicinity sources of morbid exhalations well calculated to produce the disease, there were "plague spots" in other parts of our city, remote from South Street wharf, where, had the question been asked, we should, as Dr. Jewell thinks, have unhesitatingly located the first appearance of a focus of infection, competent to exercise its morbid influence on an atmosphere already tainted; and, consequently, that from the absence of fever in those "plague spots," we may infer it would not have appeared at South Street wharf, had not the Mandarin diffused its latent poison there. If the argument is valid as regards the last epidemic, it must be equally so as regards all other epidemics here, and in many other places in this country, in Europe, and even in tropical regions.

If the local domestic origin of yellow fever is to be denied or doubted on the ground that there are "plague spots" at a greater or less distance from the infected locality, which, although more foul than the latter, have nevertheless remained free from sickness, there will be some difficulty in ever connecting, as effect and cause, the appearance of the disease with the exhalations from the localities where it happens to show itself. In the yellow fever epidemics of this city—and the same remark is applicable to those of other American cities and of Europe—the localities infected seldom cover more than a limited surface, and it is almost always found that while the disease prevails only in those localities, other places, no better off in point of cleanliness, where the olfactory nerves are perhaps more offended by nasty smells than in the former, remain healthy. This is the case even when such "plague spots" are situated along the wharves or in the immediate vicinity. I cannot recall to mind more than a few epidemics in which the fever has prevailed over the entire extent of the river-side or port, while the exemption is almost sure to occur when those spots are located afar from the wharves, and in the heart of the invaded city. Why it is so, we may not find it easy to explain; nor is it necessary that I should endeavour here to solve the difficulty. It is sufficient for our present purposes to know that such is the fact, in order to show that the absence of yellow fever in some "plague spots," does not prove that the disease cannot originate from emanations issuing from other spots of the kind, and that it requires for its production the agency of a foul vessel.

13. Let it be remarked that, though the greater number of the cases which prevailed during the season, from the 19th of July to the middle of October, in the vicinity of the wharf where it originally broke out, and where it is supposed the Mandarin introduced and left the poison which contaminated the atmosphere, the disease showed itself at a considerable distance from the place, and without the possibility of its being traced there, directly or indirectly. Dr. Jewell, in that part of his essay which was read before the College on the 7th of September, gives us an

account of forty-four cases that had appeared up to that period. Of these, thirty-seven were traced directly to the infected district. In four, the origin was doubtful; and in three, no clew could be had as to where the disease was contracted. In other words, they could not be shown then, and cannot be proved now, to have taken the disease in the infected district, properly so called, *i. e.* within a short distance of where the Mandarin had lain; while they sickened too far from the district to permit us to think that the air contaminated by the vessel could have reached them, especially as we find that it did no injury to the intervening population. In his last communication (October 5), Dr. Jewell states that the whole number of cases registered amounted then to one hundred and seventy. Of these, one hundred and forty-seven were traced directly to the infected district, twenty-two were of doubtful or unknown origin, and one contracted the fever elsewhere.

Of these twenty-two, three occurred in September, in the upper part of the city, fully a mile and a half north of South Street, but not far from the Delaware front. The history of the first of these three cases could not be ascertained, but there is no reason to suppose that the patient had approached the infected district. He resided in North Front Street, near Callowhill. The subject of the second case was the wife of the first. "This woman declared that, so far from visiting the vicinity of South Street, she had not even crossed the threshold of her own door for several weeks, having a family of small children around her. The room she occupied, the third story front of an unfinished warehouse, was very filthy, but large, and by no means confined. The whole upper part of the building was rented out, in rooms, to different families of the low order of Irish, everything around presenting poverty, rags, and filth." The subject of the third case resided about one square north of the preceding, in Willow Street, two doors west of Front. The individual, as we are told, "unhesitatingly declared that he was not acquainted with the lower part of the city; did not know that he had ever been in the vicinity of South Street wharf; was a shoemaker; worked in Front Street, above Noble, and was not in the habit of going anywhere else but from his shop to his residence in Willow Street." (*Jewell.*)

Here we have cases of unmistakable yellow fever arising far from the infected district, and in a locality where the atmosphere supposed to have been contaminated by a poison derived from the Mandarin could not have penetrated. Dr. Jewell, it is true, speaks of "an epidemic influence stealthily creeping along the wharves from the infected district, and which lent assistance to other causes in exercising a morbid agency on the individuals in question." But he will have some difficulty in making us understand how an epidemic influence (the nature of which, by the way, he does not explain) could have travelled the distance of a mile and a half, and, in its transit, passing over hundreds of vessels, and thousands of people, without influencing any but three individuals residing at the end of its journey. More natural will it be—more consonant with the result of experience here and elsewhere—to conclude that the disease arose, in the instances above mentioned, from the action of a local febrile poison, and, setting aside the afore-said mischievous intruder from South Street, join with Dr. Jewell himself in the opinion that the effect was due to a residence, on the part of the individuals seized, "in the immediate vicinity of Willow Street wharf, where the culvert along Pegg's Run empties into the Delaware, which outlet, at low tide, is fully exposed, and where at all times there is a large deposit of putrefying vegetable and animal remains, and that, from the decomposition of these remains, there arose 'a miasmatic constitution of the atmosphere,' 'from the inhalation of which those individuals contracted the fever, resulting in the death of two of them.'" Surely, to

those who know what the results of the exposure of such remains to the action of a powerful sun commonly are—especially during a summer season such as that through which we passed in 1853, with an average heat several degrees higher than usual—the sickness of the individuals mentioned will not appear difficult to explain, without having recourse to an epidemic influence stealthily creeping along the wharves, and whose existence is founded on purely hypothetical grounds.

14. To the various reasons I have thus far assigned for opposing the belief that the Mandarin was instrumental in introducing the disease among us, I must add a few more. It is stated, and several times repeated, by Dr. Jewell, that, as far as has been ascertained—and the strictest inquiry, we are told, was instituted by Dr. Gilbert, the port physician—there was no development of disease of a malignant type in the vicinity of where this vessel lay, either before or during the time of discharging her cargo. In another place, Dr. Jewell remarks: “Before the arrival of the Mandarin, and up to the 19th of July, the day on which the first case of fever occurred, the vicinity of South Street and the wharf, as well as the entire city, enjoyed its usual degree of health. Certain it is, that no epidemic existed.” Now, as regards the correctness of the last statement, there can be no difference of opinion; for there certainly prevailed no epidemic on or about South Street wharf prior to the date mentioned, and at no time was there anything of the sort in the city at large. We must also admit that, anterior to the 19th of July, cases of fever deserving the name of malignant or yellow were not reported as such to the Board of Health, and must have been few in number. But that the case of the 19th was the first that occurred, may admit of some doubt. Indeed, I have reason to believe that such was not the fact, and that, some time prior to the 19th, cases did occur which exhibited many of the characters of the fever about to prevail epidemically. Of one case I am positively certain. It was as well marked as any I heard of or saw during the course of the season, presenting the peculiar headache and anxious expression of countenance; the frequent and contracted pulse, so common during the stage of reaction; the intense burning at the epigastrium; the red, contracted, and furred tongue; the chocolate, claret-coloured, coffee-ground discharge from the stomach; the tarry, dark, purplish, bloody stools; the jaundice; the bronzed appearance of the skin a few hours after death; and, on the third and fourth day, so sudden and complete a cessation of all the bad symptoms, and so remarkable a retention or return of muscular strength, as to induce the physicians to regard the disease as effectually mastered. This assemblage of phenomena, so strongly characteristic of the yellow fever, is not encountered in any other febrile affection, and I can have no doubt that, were the description of the case inserted in the same monograph with those of other cases of undeniable yellow fever, without reference to the time at which it occurred, the individual would not be supposed to have died of any but the genuine disease.

The conclusion will appear the more natural when we take into consideration the period of the year at which the case occurred; the high range of the thermometer at the time, and for many weeks previous; the localities the patient had visited, and the circumstance that, but a few days after, many other cases, as regards the yellow fever nature of which there can be no difference of opinion, showed themselves in those very same localities.

15. Another and very strong reason for doubting, if not denying, the agency of the Mandarin in the production of the fever of last year, is derived from the circumstance that we are called upon to believe that an epidemic which lasted—as epidemics of yellow fever usually do here and in other parts of temperate regions—nearly three months, was thrust upon us by a vessel which did not remain in port

more than a week after the discharge of her cargo ; which is yet to be proved to have communicated the disease to a single individual who visited or worked on board of her, and which is admitted by all parties to have been perfectly innocuous after she left Almond Street. In a word, we are given to understand, that a fever which continued to prevail a very long while after a vessel left the wharf where her cargo was discharged, was nevertheless introduced among us by that vessel ; that this vessel deposited during the few hours she remained at the wharf after she was emptied, a poisonous agent of some sort, which continued to thrive and spread its baneful effects, after the departure of the instrument of its introduction ; and that the latter, after making the fatal deposit, and being removed from the spot it had helped to contaminate, ceased to be itself a focus of infection.

The occurrence, in so far as regards the yellow fever, does not appear to be probable, and would, if true, be in direct contradiction to the result of former experience here, and in many places where the disease has often been observed. In all other instances on record, whatever may have been the extent and virulence of the infection on board, the fever disappeared immediately after the removal of the vessel that had brought it ; or, if some cases continued to appear after her departure, they soon ceased to do so, and were found to occur in individuals who had been exposed to the infection in or near her, and in whose system the poison had remained in a latent state. In all instances of infected ships, the disease remains in full force on board till removed through means of a thorough expurgation, or till killed by the agency of frost. It is limited to the vessel itself, or its immediate vicinity ; it does not extend beyond ; and as soon as the floating local cause of infection is removed, the disease soon stops and no epidemic ensues. Such has been the case in reference to the ships Hibbert, Dasher, Pyramus, Pique, Rattlesnake, Scout, Euriale, Levant, Peacock, Blossom, Vestal, Bedford ; such has been the case also in reference to the steamers Medway, Conway, Orinoco, Magnalina, Panama, Esk, and many others it is not worth while to enumerate.

All these vessels were decidedly infected ; some of them had been so for weeks or months, and were thoroughly impregnated with the poison ; and yet, though they were admitted into heretofore healthy ports, under circumstances of atmosphere and localities the most favourable for the introduction of the disease, if that introduction were possible ; though individuals labouring under the fever, or its effects, were landed and received into hospital wards, and though the freest intercourse was allowed, no evil consequence ensued. No one took the fever, unless he went on board or hovered in the vicinity. "So," as Dr. Wilson, a competent authority on such matters, remarks, "it happens, if not universally, almost universally. Nearly every man who joins a ship in such a condition has the prevalent disease sooner or later ; but no number of persons taken from such a ship, labouring under the disease in any stage, or in any force, and placed in a situation where the disease does not exist, though in the centre of a mass of healthy people, can excite it in a single instance."¹ And we are yet to learn that, after the departure of those vessels, the disease broke out and prevailed epidemically.

In addition to what precedes, it is remarked, by importationists, that the numerous instances of contagion and communication of yellow fever in the West Indies, in Europe, and in various parts of the United States, recorded by competent and unimpeachable authorities, must be regarded as lending strong support to the views of those who ascribe to the disease of this city an exotic origin, and consider it as having spread by contagion. If the yellow fever, they say, can in

¹ Statistical Report, p. 110.

those countries have been transmitted from some other place; and if, when once introduced it can have been communicated from one individual to another, there is every reason to conclude that such may and must have been the case here also.

The introduction of the fever into Martinique by the Oriflamme, towards the close of the 17th century,¹ and its propagation thence to St. Domingo, St. Croix and other islands;² its introduction from Bulam into Grenada in 1793—an event so graphically recorded by Chisholm,³ Stuart⁴ and Gordon;⁵ its spread from that to other islands during the same year and the succeeding, as well as its propagation among those exposed to emanations from the sick, have been long before the profession, and are glaring and satisfactory instances of the existence of the property contended for. The appearance of the fever at St. Croix in 1817, as mentioned in Dr. Stevens's work on the blood, took place under circumstances which leave no doubt of the transportable character of the disease. Other instances in the West Indies related by Warren (p. 4), Sir W. Pym (p. 11), Gillespie (p. 48), Moreau de Jonnès (p. 181), Bally (pp. 404, 451), Caillot (p. 204), (M'Grigor, *Sketches*, p. 227), Chisholm (*Manual*, p. 191), are to the same purport. So, also, the introduction of the fever into Cadiz in 1800 and 1819; into Barcelona in 1821; into Gibraltar in 1804, 1813 and 1828; and into Leghorn in 1804; as well as its propagation thence to the neighbouring cities and towns—Seville, Medina Sidonia, Malaga, Antiguera, La Rambla, Algesiras, Carthagenas, Alicante, Murcia—and its diffusion among the people in various directions. All those facts which are recorded by Berthe, Arejula, Caisergues, Sir James Fellowes, Sir William Pym, Audouard, Rochoux, Bally, Gilpin, Blane, Vance, Frazier, Palloni, Dufour, must carry conviction to all unprejudiced minds. What can be more strikingly illustrative of the character in question than the oft mentioned occurrence on board the English frigate *Hussar*, described by Sir Gilbert Blane;⁶ or the other cases of similar nature stated by the same high authority,⁷ by Caillot (pp. 198, 202, &c.), Bally (p. 423), Kéraudren (p. 18, &c.), Gillespie (p. 31, &c.), Moreau de Jonnès (p. 122).

The history of the introduction of the fever in Boston, in 1693, from Barbadoes and Martinique by the squadron of Admiral Wheeler; of the epidemics of Charleston, Augusta, and St. Augustine, in 1839, as adduced by Dr. Strobell; of those of New York in 1791, 1798, 1805, 1822, &c., as described by Addoms, Bayley, Hosack, McKnight, and Townsend; of New Haven, in 1794 (Munson and Currie); of Middletown, in 1820 (Tully and Miner); of Providence, in 1805 (Pardon Bowen, and Hosack); of Perth Amboy (Hosack); of Brooklyn, N. Y. (Gillespie); of St. Mary's, Geo. (Seagrave); of Swedesborough (College of Physicians); of Washington, Miss. (Monette); of Washington, La. (Cooke); and more particularly of Ascension, in 1823 (Blane); and of Boa Vista in 1845 (McWilliams)—goes far, it is said, to establish similar views; and, until the facts on which it is based have been proved to be unfounded, it must, as stated, afford, by analogy, ample support to the opinion of the contagious and transportable nature of the yellow fever of this city.

It would be an endless work, and one of supererogation, to pass over in review all the epidemics—even those of recent date—to which the importationists appeal as affording proofs of the correctness of their views. In this country, Boston,

¹ *Nouveaux Voyages in Amérique*, i. 79.

² Moreau de Saint Méry, *Hist. de St. Domingue*, i. 701.

³ *An Essay on the Malignant Pestil. Fever*, &c., i. 49.

⁴ *Med. and Philos. Register*, iii. 183.

⁵ Appendix to Chisholm's Letter to Haygarth, p. 220.

⁶ *Diseases of Seamen*, p. 605 (3d ed. 1807).

⁷ *Select Dissertations*, ii. 141, 142.

Middletown, Chatham, New Haven, New London, Providence, New York, Brooklyn, Wallabout, Perth Amboy, Wilmington (Del.), Brandywine, Baltimore, Alexandria (Va.), Norfolk, Wilmington (N. C.), Charleston, Savannah, New Orleans, Mobile, Natchez, Washington (Miss.), Vicksburgh, Grand Gulf, Rodney, Memphis, Woodville, Opelousas, Pensacola, Key West, Galveston, Houston, St. Augustine, Washington (La.), and some scores of other places, have been visited by the disease to a greater or less extent. Some of these have, like Philadelphia, been the seat of several and even of many devastating epidemics. In others, the disease is of almost annual occurrence; and in nearly all—whether north or south of Mason's line—and at every recurrence of the calamity, the origin of the latter has been ascribed by some to the arrival of an individual labouring under, or convalescing from the disease; or to the introduction of the poison by ships, clothes, merchandise, and the like.

But while such are the sentiments entertained by some professional men, and by a larger portion of the public at large, others—fully as competent—have taken an opposite view of the subject, and piled up facts and arguments to disprove the idea of such an origin, and to place it to the score of local causes, independent of exotic agencies. Let the reader open the annals of the yellow fever in this country, and he will find that while, in some places, as Boston, New Haven, Alexandria, Wilmington (N. C.), Brandywine, Baltimore, little or nothing has appeared in print calculated to make us believe that the disease was regarded by any one whose opinion is entitled to respect, as due to the introduction of an exotic poison; while in others, as Mobile and Norfolk, whatever may be the opinion in regard to recent events, ample reasons were found by competent inquirers to refer the early epidemics to the agency of local causes; and while again in a few—as Boston, Middletown, Newport, and Chatham, the vessels or cargoes to which the disease was traced were in a foul condition, and evidently contained local sources of infection; in others, the statements upon which the doctrine of importation is based are unfounded, and the occurrences adduced incorrectly explained. In some instances the vessel, or box, or trunk, or diseased individual accused, has either arrived too long before, or too long after, or too soon before the outbreak of the fever, to allow us to regard them as the agents of transmission. In some, the inhabitants have been exposed to the influence of the very same supposed sources of communication occasionally, frequently, or even annually—in the same season too—without suffering from the occurrence. In not a few, no communication can be traced between the individuals first affected and the reputed source of introduction, while others who have had such communication escape altogether, or are attacked too long after to have received the disease in that way.

We sometimes find that the fever has broken out almost simultaneously in too many spots, and these at too great a distance from each other, to induce the belief of its being the offspring of a poison introduced from abroad. Sometimes, we find that the vessel or steamboat accused of introducing the disease was in a clean and healthy condition, and had no one sick on board; while, on the other hand, other vessels or boats—perchance the same—coming from infected spots, and even having cases on board, have stopped and discharged cargo, and landed their healthy and sick passengers and crews in the same or other ports without communicating the disease. We find that while a particular epidemic is attributed by some to the agency of a special vessel, or boat, or box, or trunk, or to the arrival of an individual, the same epidemic is, by a different set, attributed to some other mode of conveyance. Again, we find that the localities where the fever was supposed to have been introduced, as well as the meteoric peculiarities of the atmosphere, were generally, if not

always, of the kind that have been found to give rise to, or to be connected with, the prevalence of the fever under circumstances where it would have been impossible to attribute it to a foreign contagion or ferment. Again, we find that many of the places where epidemics of yellow fever were supposed to have arisen from importation, the disease prevails occasionally—in some annually—in a sporadic form, when it is evidently due to the operation of local causes.

In a word, there is scarcely one of the aforesaid epidemics, the exotic origin of which has not been, or cannot be disproved on grounds of the most satisfactory kind; or at least be rendered more than doubtful. I cannot too strongly recommend to such of the readers as may be desirous of ascertaining the small degree of confidence to which the accounts extant of the pretended importation of yellow fever, either by direct contagion or by fermentative assimilation, are entitled, the able analysis of the epidemics of the Mississippi Valley, from New Orleans to Memphis, contained in the second and posthumous volume of the late Dr. Drake's great work. A perusal of that volume will show clearly that in not one of these epidemics has a case of importation been made out. Of those that have occurred since the time of Dr. D.'s inquiries, nothing has been said calculated to show that they were the offspring of other than local causes. To Dr. Fenner, and a few others, credit is due for having shown that in 1846, 1847, and 1853, the disease sprang up in New Orleans, and had spread more or less extensively *before* the arrival of any of the vessels by which it was supposed to have been introduced: while, in other places along the Mississippi, or the Louisiana Lakes, or in Mobile, Galveston, &c., the disease does not appear to have arisen lately from the operation of morbid influences differing from those to which attacks of it in former days were recognized to be due.

Pass we now to Europe, we find that the yellow fever, which has prevailed at various periods at Cadiz, Xeres, Seville, Malaga, Barcelona, Carthagena, and other towns of Spain; at Gibraltar, at the Marseilles Lazaretto, and at Leghorn, have, in like manner, been ascribed to importation, either from tropical regions or an infected neighbouring place, which itself had derived it from a different locality. But there, also, the exotic origin of the disease, though more generally believed, has met with a decided opposition, and contrary views have been advocated with undeniable success. I shall not attempt to analyze the statements that have been made in reference to the occurrences of those periods of calamity; neither shall I enter into an investigation of the manner in which the disease broke out and spread, or point out the unfounded nature of many of the facts adduced, or the erroneous inference drawn from the true ones by the advocates of importation relative to the epidemics of Spain. The details would occupy more space and time than can be spared on the present occasion. Besides, in doing so, I should only repeat what has been done by O'Halloran, Doughty, R. Jackson, Costa, Lassis, Piguilem, C. Maclean, Rochoux, and other writers, and especially by one whose investigations on the subject before us, in the old and new world, are well known and duly appreciated here and elsewhere, the late Dr. Chervin. Following, step by step, the French Commissioners sent to Barcelona in 1821, Bally, François, and Pariset—those great champions of contagion—not only in and about that ill-fated city, but also throughout every part of Spain, where events favourable to the doctrine of importation and contagion were said to have occurred, and collecting, everywhere, facts relative to the antecedent epidemics of that country, he amassed an immense series of unimpeachable documents which completely demolished the fabric which those Commissioners had so pompously raised, and placed in bolder relief than had been done before on the continent—

judging from the conclusions adopted but a few years before, by the highest scientific tribunal of France¹—the truth of the doctrine of non-contagion and non-importation. “Observe,” as is remarked by an impartial writer, in regard to this last subject (the error of attributing to contagion and importation what should be referred to local causes), “what occurred respecting the yellow fever epidemic of 1821 in the unfortunate city of Barcelona. Read the work of the French Commission appointed to investigate that epidemic, and it will be impossible for you (admitting as true the statements therein contained) not to subscribe to the existence of contagion (and, I add, importation). But afterwards—when you have read the precious documents which Dr. Chervin collected with a degree of zeal and patience truly admirable, you will be convinced that the circumstances which induced you to share the opinion entertained by the commissioners are anything but conclusive. Henceforward, those ideas will be effaced from your mind like a vain dream; and, pressed on every side by the evidence of observation, you will be compelled to attribute to local infection those circumstances which, misled by inaccurate statements, you had placed to the account of contagion.”²

The same results have attended the analysis of the occurrences at Gibraltar in 1800, 1804, 1810, 1813, 1814, and 1828, on all of which occasions the disease prevailed extensively, as the reader will not fail to perceive on consulting the writings of Bancroft, Fraser, Gillkrest, P. Wilson, Chervin, Amiel, T. Smith, and Howell, while the whole story of the introduction of the fever at Leghorn in 1804 crumbles to pieces when made the subject of close scrutiny.

Need I enter into an examination of the instances of the supposed introduction of the fever in tropical cities and towns? The histories of these, whether on the coast of Africa, in the West Indies, or on the coast of South America, are not wanting in accounts of such transmissions, by medical writers and travellers, from the earliest records to the present day. But, in opposition to them, and in favour of the local origin of the disease on all the occasions in question, and on hundreds of others that could be mentioned, enough has been said to show the small amount of proofs with which the advocates of importation are apt to be satisfied. Let any one open the works of Dr. H. Warren, of Father Labat, of Father Du Tertre, of Rochefort and Trapham, and weigh what they have said respecting the importation of the fever into Barbadoes from Marseilles at one time and Syria at another; into St. Domingo from the Coast of Africa; into Martinique from Brazil; into Guadeloupe from La Rochelle, and into Jamaica from Panama, and he will feel but little reason to look with a favourable eye on the statements and surmises of those early writers. Let him, coming down to a later period, examine the writings of Pym, Moreau de Jonnès, Bertulus, Kéraudren, and Blane, and consult, at the same time, the rebutting statements of Jackson, Ferguson, H. McLean, Imray, Musgrave, Blair, Lefort, Humboldt, and Bancroft, and he will acknowledge that it is unnecessary to devote time and space to an examination of the supposed instances of transmission of the disease recorded by the former.

Indeed, it may safely be said that all the outbreaks of the fever in tropical regions which have come to our knowledge, have been satisfactorily accounted for by the morbid influence of local causes; that, in numerous instances, the disease has undeniably arisen without the possibility of tracing it to the introduction of an exotic poison; that, in hundreds of others, the freest intercourse between infected and healthy localities has been maintained without the least injury to the latter; that the disease shows itself annually, and almost all the year round, in a spo-

¹ Dupuytren, Rapport sur le Mémoire de M. Costa, p. 205.

² Bouillaud, Art. Contagion, Dict. de Méd. et de Chir. Pratique, vol. v.

radic form, in most tropical cities and towns, and may, therefore, be admitted to assume the epidemic garb without its being necessary to have recourse to an imported poison, or an assimilative ferment; and, finally, that all the attempts that have been made—whether at Sierra Leone and other parts of the African coast, in the West Indies, on the coast of South America, to establish the doctrine of importation, have signally failed. From this I do not except the so often mentioned epidemics of Boa Vista in 1845, and of Ascension in 1823; for, after a careful examination of all the publications that have appeared on the subject, I do not perceive any reason for connecting those events with the arrival of the *Eclair* in the former island, and of the *Bann* in the latter. And certainly those who feel no disposition to believe in the agency of those vessels in the cases in question—two of the most plausible adduced by importationists—can have little hesitation in rejecting the less important ones which, from time to time, have been presented to our notice. On the subject of the fever of Boa Vista, the reader, curious in such matters, is referred to the Report and late work of Dr. King, and to the second Report on Quarantine by the General Board of Health of London (1852).

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